Realizing Reason

A Narrative of Truth and Knowing

Danielle Macbeth

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The world of mathematics is not a world apart. We will not have an adequate account of the physical world and our knowledge of it until we understand better than we presently do the role played by mathematics in our accounts of physical phenomena. And it is not likely that we will satisfy ourselves on that score until we have produced accounts of knowledge, truth, and reality that deal adequately with pure mathematics as well.

Benacerraf and Putnam

Preface

When I began this project it had a different title and a more modest ambition. Only very recently did it become apparent to me that not only was I tracing developments in the practice of mathematics I was tracking also the realization of reason as a power of knowing. In retrospect, I think that it had to be this way: only *after* reason is realized as a power of knowing is it possible to recognize the process of its realization as such. And having become clear on the *end* of project, the book I was actually writing, I became clear also about its beginning, where, how, and when this project seems actually to have started, namely, I now think, with my dissertation in the philosophy of mind written under John Haugeland (and inspired by his reading of Heidegger) at the University of Pittsburgh in the 1980s. I could not have anticipated then that to understand our intentional directedness on objective reality would require, first, a radically new reading of Frege's *Begriffsschrift* notation (the reading I develop in *Frege's Logic*), and building on that reading, the account of the whole of the history of Western mathematics that I offer here. Nevertheless, that, so it seems to me, is what has happened.

Mathematics and philosophy have of course been intimately related throughout their history in the West. For Plato, the study of mathematics was a propaedeutic to the philosophical inquiry he called dialectic. For Descartes, method in metaphysics was modeled on his new method in mathematics. Leibniz, Kant, and Frege all held that one way or another the practice of mathematics was central to the practice of philosophy. And so it is here. To understand the nature of mathematics is, on our account, integral to understanding ourselves as the rational beings we are. This is due in part to the fact that mathematics, like philosophy, is a paradigm of rational activity. But it depends also on the fact that in mathematics, and only in mathematics, can a language be developed that, like natural language, serves as a medium of our cognitive commerce with reality. (This is not to say that one needs to have special training in mathematics to comprehend this work. One does not.) Understanding how a mathematical language such as Frege's concept-script can serve as a medium of our cognitive involvements in the world is the first step in understanding even our everyday involvements in the world. The most technically challenging material here is not, then, in the chapters on mathematical practice but instead in Chapter 7 on Frege's notation. I have aimed, in that chapter, to go into detail sufficient to satisfy anyone who wants actually to work through and master Frege's proof of theorem 133 in Part III of his 1879 logic, *Begriffsschrift*. There is much in that chapter that the less committed reader may want to skim. Nonetheless, the details matter. Frege *is* doing what I claim he is doing but the only way to see that he is, is by gaining proficiency in the language. To understand everything that is claimed in Chapter 7, by having worked through all the details of Frege's proof that are there discussed, is to have the literacy that is needed *fully* to understand Frege's notation.

A proof in mathematics is of course very different from the sort of narrative that is told here. A proof is a kind of *argument*, a giving of reasons, grounds, or justifications, and a narrative is not. But perhaps this is too simple. After all, the proofs that mathematicians concern themselves with generally have, as stories do, a central idea on which the whole development turns. And there *is* development in a mathematical proof. Things *happen* in a (real) proof; proofs unfold and can have surprising turns. Mathematical proofs—good ones, interesting ones—are, in short, rather like narratives. And narratives can in their way provide reasons, grounds, and justifications insofar as they can help one to learn new ways of making sense of things, and to see how old ways of making sense do not in fact withstand critical scrutiny. If one is trying to change a reader's conception of what makes sense at all, trying to change the space of possibilities within which a reader's thought moves (as I aim to here), then a traditional philosophical argument is of little use. Arguments can be formulated only within established disciplinary boundaries, where the rules and acceptable starting points have already been agreed upon, at least in the main. What is needed, and what these pages provide, is a narrative of our intellectual maturation and growth, one that, if successful, will change a reader's way of looking at things.

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John Wiley and Sons: "The Coin of the Intentional Realm" (based on work in my dissertation), *Journal for the Theory of Social Behavior* 24 (1994): 143–66, for material in section 1.3.

College Publications: "Diagrammatic Reasoning in Euclid's *Elements*", *Philosophical Perspectives on Mathematical Practice*, Texts in Philosophy, vol. 12, ed. Bart Van Kerkove, Jonas De Vuyst, and Jean Paul Van Bendegem (London: College Publications, 2010), for much of Chapter 2.

New School for Social Research: "Viète, Descartes, and the Emergence of Modern Mathematics", *Graduate Faculty Philosophy Journal* 25 (2004): 87–117, sections 3.2 through 3.4.

Oxford University Press: "Logic and the Foundations of Mathematics", *The Oxford Handbook of American Philosophy*, ed. Cheryl Misak (Oxford: Oxford University Press, 2008), material in section 5.4.

Institute of Logic and Cognition, Guangzhou: "The Problem of Mathematical Truth", *Studies in Logic* 2 (2009): 1–17, material in section 5.4.

Editions Kimé: "Proof and Understanding in Mathematical Practice", *Philosophia Scientiae* 16 (2012): 29–54, section 6.2.

National Center for Logical Investigation, Brussels: "Writing Reason", *Logique et Analyse* (2013) 221: 25–44, parts of section 6.4.

Editions Rodopi: "Striving for Truth in the Practice of Mathematics: Kant and Frege", *Grazer Philosophische Studien* 75 (2007): 65–92, a first attempt at some themes in Chapters 7 and 8.

Springer Science + Business Media: "Diagrammatic Reasoning in Frege's *Begriffsschrift*", *Synthese* 186 (2012): 289–314, a second pass through themes of Chapters 7 and 8.

I also gratefully acknowledge the support of the American Council of Learned Societies, which awarded me a Burkhardt Fellowship in 2000, and the Center for Advanced Study in the Behavioral Sciences where I was a Fellow in 2002/03. It was while at the Center that I began to work through the (often overwhelming) literature in the history and philosophy of mathematics; I would not have managed half so well without the excellent research support of the CASBS staff. And without subsequent sabbatical leaves, with the full financial support of Haverford College, I could not have continued to progress year by year.

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Introduction

In mathematics it often happens that a problem can be posed long before the resources required for its solution are, or even can be, developed. The Pappus problem, for example, was formulated already in antiquity but could be solved only in the seventeenth century using the new algebraic resources of Descartes' analytic geometry. Fermat's Last Theorem was formulated by Fermat in 1637 but solved only in 1995 by Andrew Wiles using mathematical ideas and techniques that were wholly unimaginable in the seventeenth century. The same is true of our leading problem here, the problem of understanding how mathematics works as a mode of intellectual inquiry. Only very recently have the resources needed for its resolution been developed. And as is the case in mathematics, the reason those needed resources become available only very late is that they involve a degree of complexity and sophistication that is made possible only through the processes of intellectual development and growth of which they are the fruits.

Immediately we are faced with a difficulty: in order so much as to understand those resources, the resources that will enable us to achieve an adequate conception of mathematical practice as a mode of intellectual inquiry (and, we will see, much else besides), we need explicitly to embrace a very different conception of philosophical understanding from that generally assumed today. Although many otherwise quite different accounts could be cited, we here take Dummett's account of the shape any acceptable theory of meaning must take as exemplary of the usual sort of philosophical understanding, the sort that it is generally assumed is needed in philosophy.

What Dummett is after is a philosophically illuminating theory of meaning and that means, he thinks, that the theory must be formulated in terms that make no appeal to meanings. The goal is a systematic description of our linguistic practices, where

> such a description will give a representation of what it is for the words and expressions of our language to have the meanings that they do. It must embrace everything we learn when we first learn language, and hence cannot take as already given any notions a grasp of which is possible only for a language-speaker. In this way, it will lay bare what makes something a *language*, and thus what it is for a word or sentence to have a meaning. (Dummett 1991, 13)

What we grasp when we understand the words and sentences of our language is to be explained as if from the outside, independent of any and all appeal to meanings. Such a theory is reductive insofar as the aim is to reduce meanings to something else, something that is not meaningful in the same way, and it is, if only implicitly, mechanistic insofar as having so reduced meanings it should be in principle possible to build something that can do what the theory describes us as doing in our linguistic practices.

The problem with such an account, McDowell (1987) and (1997) argues, is that no such reductive account of meaning is possible. A theory of meaning must be "modest"; it starts, must start, "in the midst of content" (McDowell 1987, 105). If McDowell is right, the meanings that are expressed in our linguistic utterances are inaccessible except to one who already grasps meanings. From Dummett's perspective this view of meaning makes of it a complete mystery; the only way to demystify meaning (he thinks) is to reconstruct it in a way that makes no appeal to the meanings we seek to understand. How, if not reductively, is the philosopher to understand meanings? The theory of biological evolution by natural selection provides, I think, the beginnings of an answer.

Although it is possible to understand processes of biological evolution by natural selection merely mechanistically, as a kind of as-if design the fruits of which are complex biological machines, it is also possible (and perhaps in the end necessary) to understand those processes differently, as processes that realize something essentially new, something that cannot be adequately understood in the reductive, mechanistic terms that are antecedently available. The basic idea (explored in more detail in Chapter 1) is this. In inanimate nature, before there have evolved any living beings, there are only physical, strictly law-governed processes. There are no norms and no significances but only processes that are fully explained by appeal to various exceptionless laws of nature. In inanimate nature, things are merely caused to happen: iron is caused to rust by there being moisture on it. And, again, one might think that the behaviors of animate beings are similarly caused to happen. Certainly it is possible so to explain various processes underlying such behaviors, for instance, the chemical processes that are involved in digestion. But digestion is not merely a chemical process. One misses something if one thinks of digestion only at that level of description insofar as digestion has a point, a role to play in the life of the organism: digestion is for nourishment. The processes of evolution realize in this way new significances for things. Rain, for instance, acquires the significance of being nourishing for plants. This is not mysterious but a simple consequence of the fact that the modus operandi of biological evolution inevitably accords significance to the capacity of, say, a plant to survive long enough to produce viable seeds, seeds that in time will grow into (the next generation of) plants. This simple fact, that a living thing is successful if it produces viable offspring, is what enables, indeed requires, our drawing a normative distinction between what is good for the plant, what promotes its flourishing, and what is bad for it, what impedes its flourishing.

Early modern philosophers liked to ridicule the ancient Greeks for taking animate nature as their model for inanimate nature. It seemed right to mock them for thinking that mere physical stuffs, such as, say, fire, have natures and powers, for example, the power of fire to move itself upwards (to where, so Aristotle thought, it belongs). And it was of course an important discovery of early modernity that the behaviors of merely physical stuffs are instead to be explained by appeal to laws of physics. But that by itself is not a reason to understand *animate* nature on this new model of inanimate nature. Indeed, to think of animate beings on the model of inanimate beings, as nothing more than complex mechanisms the behaviors of which are governed by the laws of nature, would seem to involve precisely the mistake for which the ancient Greeks were ridiculed, only in the converse direction. There is nothing magical or mysterious about the fact that rain nourishes plants, but nor can that fact be reduced to something merely mechanistic. The theory of biological evolution by natural selection enables us explicitly to recognize the intelligibility of the idea that a theory of life must start in the midst of life. It provides, or at least makes room for, a form of understanding that is neither reductive nor mechanistic.

Living things, animate beings, are very late fruits of a very long process of biological evolution, and although we have a tendency, inherited from early modernity, to try to understand living things mechanistically, as if they really were nothing more than complex biological machines, it is in this case *obviously* possible to resist the reduction without being intellectually defeatist or obscurantist. There is, again, nothing intellectually defeatist or obscurantist in claiming that rain nourishes plants, in denying that that fact can be reduced to some fact about inanimate nature. The task for us here is to understand how we might similarly resist any such reduction of mathematical practice and mathematical understanding while at the same time providing philosophical insight into that practice and form of understanding. Clearly no mere biological account can explain the practice of mathematics. The ability to do mathematics is an ability that only certain animals—rational animals, animals with the power of reason—have. Our concern is with the nature of this power as it is manifested in mathematical modes of inquiry. First, we need at least the outlines of an understanding of the emergence of rational animals, of how there might come to be any such animals at all. This task, taken up in Chapter 1, is discharged by appeal to certain processes of social evolution that are made possible by the appearance of animals with (biologically based) powers of imitation, cooperation, and cognitive map-making. The appearance of rational animals is only a beginning, however, and reason itself, as it at first appears, is not yet a fully realized power of knowing but only a capacity for critical reflection. Pure reason has, to begin with, the form only of a canon (in Kant's sense), not that of an organon by which to extend our knowledge. To understand reason as a power of *knowing*, first and foremost in mathematics, we need to trace yet another process, not now a process of evolution but instead one of maturation and growth. We need, and Chapters 2–9 aim to provide, a dialectical, historicist account of the realization of reason from its first appearance in ancient Greek mathematical practice through to its full flowering in twentieth-century fundamental physics.[1]

This, then, is a story with a beginning, a middle, and an end, the story of reason's unfolding, its maturation and growth to become a power of knowing, first and foremost in mathematics. But although both the story we tell of the realization of reason as a power of knowing and our telling of that story have three parts, the three parts of the telling do not exactly correspond to the three parts of the story. Because the telling has had to be complicated by certain contingent, historical details, it will help to provide here a brief account of the story that is told, the story of the realization of reason as a power of knowing, as that story contrasts with the telling of it.

Our story begins in immediacy, with ourselves as we first find ourselves in the midst of the perceptible things of everyday life. What we know first and foremost at this stage are objects in their natures, as what they are, Socrates, say, as an instance of human being, a particular form of life. Mathematical knowledge, similarly, is knowledge of objects, paradigmatically how to construct them out of canonical elements, points, lines, and circles, because it is in the nature of mathematical objects to be at once unities, wholes, but also and equally to consist of parts in relation.[2] To solve a construction problem in ancient diagrammatic practice is to achieve systematic knowledge of a mathematical object in its nature. And as ancient Greek mathematics in this way investigates the intelligibility of things, so ancient Greek philosophy grapples with the problem of the intelligibility of this mathematical practice, in particular, with the ontological status of its objects, the mathematicals.

For the ancient Greeks, perception (conceived as a power of knowing) is the model for intellection: to grasp intelligible things is to see with the eyes of the mind. And as already noted, the animate is likewise the model for the inanimate: as living things have their natures, powers, and characteristic behaviors, so mere stuffs, earth, air, fire, and water, have their natures, powers, and characteristic behaviors. Early modernity, born in the discovery of Descartes' new mathematical practice that is the first actuality of reason as a power of knowing, constitutes a reversal in our conception of knowing and in our most fundamental understanding of the nature of that which is known. As Descartes understands it, his new mathematical practice reveals the power of reason, the pure intellect, as a power of knowing independent of any and all sense experience; and what is known is not objects in their natures but instead relations that objects can stand in, relations that can be expressed in equations and

[1] Of course it is not only in ancient Greece, with its mathematical practice, that reason makes its first appearance. Ancient Chinese and ancient Indian mathematics were also rational enterprises; they also concerned themselves with proof, the grounds for holding some claim true or some algorithm correct. (See Chemla 2012.) Nevertheless, our story of the realization of reason as a power of knowing begins with ancient Greek mathematical practice in particular because it is this practice, together with ancient Greek philosophy, that provides the historical basis for all that follows.

[2] As we will later see in some detail, mathematical objects are real wholes (as essential unities are), but they also have (as accidental unities do) real parts. They are, as I will put it (following Grosholz 2007), *intelligible* unities.

algebraic formulae. To know, for this shape of spirit, is to judge that things are thus and so, to affirm that one's mental representation of some bit of reality is true. All knowing, including even our perceptual knowledge, is now to be understood as constitutively mediated.

Descartes' new mathematical practice, which reveals, so he thinks, the pure intellect as a power of knowing, enables in turn a radically new understanding of inanimate nature in terms of exceptionless physical laws governing the motions of matter. This understanding of inanimate nature furthermore comes to provide the model for a mechanistic understanding of animate nature, and even for a mechanistic understanding of the power of reason. To understand the mind, and the power that is reason, it comes to be assumed, is to understand the laws that govern (or ought to govern) its motions. But how, Kant asks, is knowledge of these laws possible? With Kant, the ancient philosophical problem of the being of mathematical objects is superseded by the early modern problem of synthetic a priori knowledge, and what this problem reveals is that reason has not, *pace* Descartes, yet been fully actualized as a power of knowing but is, as yet, only a regulative ideal.

Early modernity begins with Descartes' discovery of a radically new way of doing mathematics, is followed by Newton's radically new way of doing physics, and ends with Kant's discovery of a radically new way of doing philosophy, critique. In nineteenth-century Germany, mathematics is again transformed to become, as it remains today, the practice of reasoning deductively from concepts, thinking in concepts, *Denken in Begriffen*. And fundamental physics is similarly transformed in the twentieth century, in special and general relativity and in quantum mechanics. All the resources are available for a transformed philosophical understanding both of knowing and of the nature of what is known, a new (third) shape of spirit that is and knows itself to be in an essentially mediated relation of immediacy with what is known.

This has not happened.

The nineteenth-century revolution in mathematical practice and the twentieth-century revolution in the practice of fundamental physics have not been followed by a revolution in the practice of philosophy.[3] As we will see in the chapters to follow, twentieth-century philosophy has, at least for the most part, remained merely Kantian. Because it has, our telling of the story of the realization of reason as a power of knowing must be a little more complicated than the story itself is.

[3] The rise of analytic philosophy, which in its own self-understanding constituted a sharp separation from the past, is often seen as just the revolution that is needed here. See, for example, Ayer et al. (1957). And Dummett (1973, 669) credits Frege in particular with commencing a revolution in philosophy on the order of the revolution begun with Descartes. I do agree that Frege has a pivotal role to play in the needed revolution, one that is in certain respects analogous to that of Descartes. As will become evident, however, I have a very different reading of Frege from Dummett's, one that grounds a very different unfolding from that which we find in twentieth-century analytic philosophy. For reasons that will become clear, twentieth-century analytic philosophy is not the revolution that is wanted.

Ancient mathematical practice poses for philosophy the problem of the ontological status, the being, of its objects, the objects that came to be called the mathematicals. Early modern mathematical practice instead poses the problem of synthetic a priori judgments, of how we are able to come to know truths that are necessary but not logically necessary. The mathematical practice that emerged in nineteenth-century Germany poses a different problem again, the problem of ampliative deductive reasoning, of how, by the exercise of pure reason alone, one might extend one's knowledge. To solve this problem is to understand how reason, pure reason, can be a power of knowing. But we *can* solve it only if we understand that, and how, this is an essentially new problem, much as Kant's problem of the synthetic a priori was, in his day, essentially new.

Kant famously remarked that it was Hume who wakened him from his dogmatic slumbers, who showed him the shape of the problem he faced. Hume had argued that knowledge of causal relations can be neither a relation of ideas, because the negation of a judgment about cause and effect is not contradictory, nor a matter of fact, because experience can tell us only what is, never what must be. This is a distinctively modern problem, one that arises only because things are no longer conceived as having their own natures and powers, natures and powers that are expressed in their characteristic behaviors. Once the essentially modern idea of things as governed by exceptionless laws of physics is in place, it is no longer possible to see the behaviors of things as *expressive* of their natures and, as a result, a logical gap opens up between, on the one hand, the behaviors to be explained, and on the other, the law that is to explain them. With the advent of modernity, Hume's commonsense division of all knowledge into relations of ideas and matters of fact had, Kant saw, to be replaced by two different distinctions, that between analytic and synthetic judgments, on the one hand, and that between what is known a priori and what is known a posteriori, on the other. Only given this distinction of distinctions is it possible so much as to formulate the problem of the synthetic a priori, to recognize that in addition to analytic judgments (which are and must be a priori) and a posteriori judgments (which are and must be synthetic) there are also synthetic judgments that are nonetheless known a priori.

As Hume held, though not in just these terms, that all a priori judgments are analytic and all synthetic judgments a posteriori, so we follow Kant in thinking that all deductive reasoning is merely explicative, that ampliative reasoning cannot be strictly deductive. Because we are Kantian, because the space of possibilities within which our thinking moves continues to be defined by fundamentally Kantian theses (in ways we will be concerned to explicate), we cannot see that reason has now been realized as a power of knowing, in mathematics in the nineteenth century and in fundamental physics in the twentieth. And because we do not see this, we cannot understand either contemporary mathematical practice, in particular the fact that its proofs can be at once deductive and ampliative, real extensions of our knowledge, or contemporary fundamental physics, in particular the peculiar role that mathematics plays in it.

By the end of the nineteenth century, developments in mathematics had seemed decisively to show that Kant's account of mathematical practice in terms of constructions in pure intuition was fatally flawed. What those developments did not show was just how Kant had gone wrong. Two very different responses emerged. The first, which would come to dominate the philosophical culture throughout the twentieth century (at least in the English-speaking world), was to jettison the idea that constructions have any role at all to play in mathematical practice and so to hold, with Kant, that without pure intuition to provide it content, mathematics must be understood in terms of the notion of form alone, as strictly deductive and hence purely formal and merely explicative.[4] The second, more radical response was to jettison not only Kant's account of mathematics as founded on constructions but also Kant's account of logic as merely formal and of deduction as merely explicative. Knowing nothing of each other's work, both Charles Sanders Peirce, an American logician, and Gottlob Frege, a German mathematician, pursued this second path. Both held that even strictly deductive reasoning by logic from definitions can extend our mathematical knowledge.[5] Only Frege managed to take the further step of showing how this can be by developing a mathematical language within which to reason deductively to conclusions that are ampliative in Kant's sense. Indeed, it will be argued, Frege's language, *Begriffsschrift*, together with the logical advances it embodies, holds the key not only to an adequate philosophical understanding of advances in mathematics in the nineteenth century but also to such an understanding of advances in physics in the twentieth, and much else besides.

This work has, then, three principal and interrelated aims. The first is to trace the essential moments in the historical unfolding, from the ancient Greeks to the present, that culminates in the full realization of pure reason as a power of knowing that is manifested first and foremost in ampliative, deductive mathematical proofs. The second is to provide a cogent and compelling account of how the practice of mathematics works as a mode of intellectual inquiry into objective mathematical truth, and the role that various written systems of signs have played and continue to play in this practice. The third is to develop and defend a new conception of our being in the world, one that at once builds on and transforms the now standard conception (sometimes called the sideways-on view) according to which our experience of reality is the result of the merely causal impacts on our sense organs and ultimately our brains of physical stuffs that surround us, according to which "inside"

[4] It was just this sort of view that led Poincaré to deny that developments in mathematics had shown Kant to be wrong about the nature of mathematical practice. According to him, mathematical reasoning clearly is ampliative and hence it cannot be strictly deductive. To take a mathematician's proof and formalize it, make it strictly deductive, is to somehow destroy its character as a mathematical proof. See Detlefsen (1992) and (1993). We return to Poincaré's views in section 5.4.

[5] Enlightening though it would be, I cannot in this work take up the question of the similarities and differences between Peirce's philosophy of mathematics and Frege's. A very brief discussion of Peirce's philosophy of mathematics is provided below in section 5.4. For other recent work on Peirce's philosophy of mathematics see Moore (2010).

are meanings and meaningful experiences, while "outside" is merely brute, causally efficacious reality.[6] The task here is to overcome that conception, to recover ancient insights into our being as the rational animals we are but in a way that incorporates also the insights of early modernity. None of these aims, we will see, can be achieved without the others. It is impossible to achieve an adequate understanding of the striving for truth in the exact sciences from within the standard conception of ourselves and our cognitive relation to reality with its meaningful inside and meaningless outside; and the way to overcome that conception is through a more adequate understanding of the nature of mathematical practice together with the profound transformations it has undergone in the course of its more than two and a half millennia history, the history through which reason is first realized as a power of knowing. It was developments in mathematics (we will see) that first made the sideways-on view possible and it is further developments in mathematics that provide the resources that are needed to articulate its successor. We need, then, to attend to mathematical practice, and in particular to its development and transformation over the course of history beginning with ancient Greek diagrammatic practice, through early modern constructive algebraic practice, to the tradition of reasoning from concepts that became dominant in Germany over the course of the nineteenth century. Chapters 2, 3, and 5 take up this task.

To understand reason as a power of knowing, how it is that by deductive, truth preserving inferences from definitions one can extend one's knowledge, is at the same time to understand the practice of mathematics as a mode of intellectual inquiry, as the striving for truth. But if we are to understand mathematical practice, how it works as a mode of intellectual inquiry, we need to see it at work; and that requires in turn attending to the systems of written signs within which to do mathematics. We need both a clear and self-consciously reflective understanding of the role of written marks in the practice of mathematics and a good grasp of at least the basics of the principal systems that have been developed within which to reason in mathematics, namely, Euclidean diagrams, the symbolic language of arithmetic and algebra, and Frege's concept-script *Begriffsschrift*. It is these systems of signs, we will see, that put

[6] Compare Matthews (1977, 25): "The 'inside' is a mind or subject of consciousness, whose acts and states are indubitable to itself. The 'outside' is a body known to the mind or 'inside' only indirectly through quite dubitable inferences from sense impressions. / The picture of human beings as having, in this way, both an 'inside' and an 'outside' is so commonplace, so (as it may seem to us) commonsensical that we find it hard to realize how strikingly modern it is." Price (1997, 174) argues that this inside/outside, or sideways-on, conception of our being in the world is something science has revealed, that "physiology . . . teaches us that Kant was right: what we get from our sensory apparatus depends on quite contingent features of our physical construction, as well as on the nature of the external world. . . . This product of a sideways-on scientific perspective is not a kind of comatose version of transcendentalism, but a plausible first-order theory about the way in which our brains are linked to their environment. Nor is it a kind of philosophical opening bid, which we can abandon on the grounds that it causes problems elsewhere in philosophy. To all intents and purposes it is a fact of modern life, within the constraints of which philosophy must operate." In fact, we will see, this conception is not a finding of science. It is a metaphysical thesis that we inherit from early modernity and will need ultimately to jettison.

mathematical reasoning in its various guises before our eyes. What would otherwise be a private mental act of reasoning becomes, with the development of a system of signs within which to reason in mathematics, a public act of reason, one that is amenable to philosophical reflection and understanding. Although nowhere collected together and made fully explicit, there is in this work all the essentials of a theory of notation, and of mathematical notation in particular.

Analytic philosophy has always had especially close ties with the sciences and has taken understanding the practice of science as one of its central concerns. Analytic philosophy has also taken as one of its guiding tenets the thought that fundamental philosophical problems can fruitfully be addressed by reflecting on language. Analytic philosophers have nonetheless tended—for contingent, historical reasons that will eventually emerge—to be quite uncritical about language. It has been assumed in particular that the symbolic languages of mathematics differ from natural languages only in their degree of rigor, clarity, and perspicuity, that a sentence of natural language can be translated without losing anything essential into a symbolic language of the appropriate sort.[7] In fact, there are significant differences between the two sorts of languages, and these differences matter to how we should understand them. First, whereas natural language is first and foremost a spoken (or signed) language and a medium of communication, the symbolic languages of mathematics—for instance, the language of arithmetic and algebra, and Frege's concept-script—are instead essentially written and serve primarily as a vehicle of reasoning. Spoken natural language is fully intelligible independent of written language; symbolic languages are not. Natural languages, at least those that are (as we say) living rather than dead, also evolve with use; they are constitutively social and historical. Symbolic languages by contrast are self-consciously created, often by a single individual, and they have no inherent tendency to change with use. Finally, as Wittgenstein reminds us in the *Philosophical Investigations*, natural language is enormously versatile. We do all sorts of things in and with natural language. As Frege reminds us in his little monograph *Begriffsschrift*, symbolic languages are instead special purpose instruments designed for particular purposes and useless for others. If we are to understand the practice of mathematics as it contrasts with our everyday cognitive involvements in the world we need to understand much better than we currently do both the essential similarities and the profound differences between natural language, on the one hand, and the languages of mathematics, on the other. Although it is only in Chapter 8, in our discussion of the philosophical implications of Frege's work, that we will be in a position adequately to explicate their essential differences, it will help to keep in mind

[7] The expression "natural language" is sometimes objected to on the grounds that the relevant sort of language is not "natural" but instead "social" or "cultural." Certainly it is not something we have as a matter of our biological endowment; language is not instinctual in us in the way various sorts of cries are instinctual in certain sorts of animals, the way cries of pain are instinctual in us. Natural language *is* social and cultural. But it is also in its way natural to us as the kind of being we are. How we should think of it, on the present view, is laid out in Chapter 1.

from the outset that natural language and the languages of mathematics serve very different purposes (if natural language can be said to serve any purpose at all) and function in profoundly different ways.

The first pillar on which this work rests is Frege's *Begriffsschrift*, a two-dimensional system of written marks that was self-consciously designed as a language within which to engage in the sort of deductive reasoning from definitions of concepts that since the nineteenth century has been the norm in mathematical practice. It is this mathematical language, we will see, that is the resource we need if we are to understand how mathematics works as a mode of inquiry. But that language alone is not sufficient for understanding the practice of mathematics. The second pillar we need is the pragmatist idea—derived in part from Kant but then developed further in the work of Peirce and Sellars, and assumed also by Frege—that inquiry is, as Sellars puts it, "rational, not because it has a *foundation* but because it is a self-correcting enterprise which can put *any* claim in jeopardy, though not *all* at once" (Sellars 1956, sec. 38). Not only is nothing Given as the firm and indubitable foundation for knowledge—whether in mathematics, in the natural sciences, or even, we will see, in logic—no Given is needed for knowledge. The fact that we have no Olympian standpoint, no "God's eye view" from which to see things as they really are, the fact that we are essentially contingent, finite, and embodied knowers does not entail that properly objective truth is beyond our ken. Paradoxical though it may at first seem, our contingency is instead *constitutive* of our capacity for knowledge of things as they are, the same for all rational beings. It is *because* nothing is Given, *because* the spontaneity of thought is radically unconditioned and so can call anything into question as reason sees fit, that we can make sense of the idea that in our scientific investigations we are after, and in time can achieve, knowledge of things as they are in themselves, the same for all rational beings.

This idea, that the rationality of inquiry is manifested in our capacity for self-correction, for second thoughts, directs us, again, to the processes of inquiry as contrasted with their products, and indicates thereby that we need to consider not only our contingency and finitude but also our historicity, that the philosophical understanding we seek must take the form of a narrative of our intellectual maturation. And, again, it must take this form because our knowledge of things as they are in themselves is an essentially late fruit of intellectual inquiry, an achievement of reason that is possible at all only in the wake of profound cognitive transformations. We begin, as the rational animals we are, in what is understood to be an immediate cognitive relation to reality, then achieve with the rise of early modern mathematics, early modern science, and early modern philosophy what seems to be an ineluctably mediated relation to reality, and only in the end, with the full realization of reason as a power of knowing in the nineteenth and twentieth centuries, achieve an adequate understanding of our cognitive relation to reality according to which we are in a recognizably mediated, and hence stable, relation of immediacy to reality.

With the rise of early modern thought there came to seem an insurmountable barrier to understanding truth and knowledge in the exact sciences, a barrier that can be expressed as an inconsistent triad: the reality on which thought aims to bear is radically mind-independent; the spontaneity of thought is radically unconditioned; and yet judgment in the exact sciences constitutively involves rational constraint by the reality on which it aims to bear. The task is to dismantle this barrier so that we may take in our stride the pursuit of truth in mathematics and the other exact sciences. There are two very different ways of conceiving this task. One way is to take our perplexity to be due to some kind of bad, but optional, prejudice. Both Rorty and McDowell, each in his own way, take our problems concerning truth and knowledge to have this shape, McDowell (1994) arguing, for instance, that our problems are due to a scientistic prejudice, that they are dissolved once we recognize that nature must not be identified with the realm of law. This seems clearly right as far as it goes: we are, as the rational, autonomous beings we are, a part of nature just as anything else is. But it is one thing to know that this is and must be so, quite another to understand how it *can* be so. To understand how it can be that autonomous rational beings are a part of nature, the same nature that is the subject of inquiry in fundamental physics, we need something more than a diagnosis of a mistaken prejudice. We need, as I aim to show by example in the chapters to follow, a narrative of our becoming, and we need this because our problem is not like a disease to be diagnosed and cured, restoring us thereby to health, but instead like adolescence, a natural and necessary stage on the way to full maturity. As will become clear, the resources we need in order to resolve our difficulties are not, as they are on Rorty's and McDowell's views, antecedently available. They are instead something essentially new, something discovered only in the light of the difficulties we face.

Aristotle's response to the ancient problem of change provides a good illustration of the shape of the philosophical move that I think is needed here. Despite being ubiquitous in our daily lives, change seemed, before Aristotle, to be utterly inconceivable, literally unthinkable. And it did so, we can see in retrospect, because no distinction had yet been drawn between two essentially different modes of being, between what something *is* (say, a human being, a horse, or a house) and what it is, as we say, *like* (courageous, fleet-footed, or wooden). Because this distinction was not yet drawn, it seemed that change could come only from what is or from what is not—only neither option made any sense. Because nothing comes from nothing, change cannot come from what is not. And because what is already is, change cannot come from what is. Change seemed, then, to be impossible. But of course it is not impossible, and Aristotle was able to show that it is not because he explicitly realized that being is not univocal, that a thing can be, as Aristotle thinks of it, one in number but two in account. One and the same thing can be both a human being, say, which is something a thing *is*, and also, say, courageous, musical, or pale, that is, something that a thing can be *like*. Change, Aristotle taught us to see, comes in a way from what is, say, a human being, and in a way from what is not, say, unmusical: the unmusical man can become a musical man.

Much as change seemed unintelligible to Aristotle's predecessors, so knowledge of objective truth, and especially knowledge of objective truth in mathematics, seems unintelligible to us. And it seems unintelligible to us because we are barely modern, because we draw distinctions, for instance, that between causes and reasons, that the ancients did not draw, at least in the same way. Nevertheless, we cannot just return to pre-modern modes of thought. Early modernity, like adolescence, must be taken up and gone through if we are to come to a satisfactory resolution of our difficulties. Early modernity was born in the recognition of distinctions not hitherto drawn, and the project of modernity will be fully completed only with the discovery of further distinctions on those distinctions. In the end we will be able take the full measure of our autonomy, of the radical mind-independence of reality, and of our capacity to know things as they are, but in order to do that we must recognize that, and how, the power of reason is realized in time, through the processes of intellectual transformation and maturation that are traced in this work.

A narrative of our intellectual growth and maturation must begin at the beginning, where we ourselves begin, and so with ourselves as we first understand ourselves to be, rational animals with various powers and abilities, among them the capacity to perceive and so to know. We will not, however, assume with the ancient Greeks that these are capacities we are born with. With Sellars, McDowell, and others, we take these capacities instead to be something we acquire only in the course of our acculturation, a central element of which is our initiation into a natural language. It is the acquisition of natural language that first provides us with a view of things, the eyes to see things as they are. From being mere animals we become, with the acquisition of language, rational animals, that is, a fundamentally different sort of living being, one that inhabits the world in a very different way from the way mere animals inhabit the world. And the world itself is transformed in the process. Much as a terrain acquires the significance of being an environment with opportunities for and hazards to survival with the emergence through the processes of biological evolution of mere animals, so an environment acquires the significance of being (a part of) the world, the locus of truth, with the emergence of rational animals. As animals and their environments arise together in the course of biological evolution, so knowers and the known arise together in the course of our social and cultural evolution. Various aspects of this evolution are the topic of the first chapter.

We begin with an account of ourselves as biologically and socio-culturally evolved rational animals. In order to go on we need something more, a vision of what might be but is not yet, and this is provided by the ancient Greek mathematical practice of reasoning with diagrams. Using diagrams, the Greeks were able to discover an extraordinary array of mathematical truths. We need to understand how this works, and we need to understand it in a historically sensitive way, that is, in a way that is compatible with an understanding of ancient Greek thought more generally as it is enabled by natural language. The reason is obvious. Our project of understanding how new shapes of consciousness can emerge in the course of history cannot succeed if elements of consciousness that are essentially late, that can emerge only out of some earlier shape of consciousness, are then read back into that earlier shape. Chapter 2 attempts to understand ancient Greek diagrammatic practice in its historical context and as a distinctively mathematical practice, one that enables the discovery of timeless objective truths.

Written natural language serves to record utterances and is read, at least at first, as a record of speech. It is the spoken, not the written word that is (at first) the bearer of meaning. Euclidean diagrams are obviously not a record of speech, but nor, we will see, are they merely records of things as, say, tally marks are a record of things in a collection, one tally mark for each. Euclidean diagrams do not record but instead formulate content in a mathematically tractable way. They provide a language within which to reason. Equations in Descartes' symbolic language of algebra are different again. Like a Euclidean diagram an equation in the language of algebra serves as a vehicle of mathematical reasoning, but it does so in a way that is radically different from the way a Euclidean diagram does. We will see in Chapter 3 that Descartes' form of mathematical practice with equations is made possible only through a profound and thoroughgoing transformation, a metamorphosis in his conception of space and of spatial drawings of, for example, circles and triangles. It will also be shown that Descartes' metaphysics, both his new conception of the order of nature in terms of laws of motion and his conception of the mind as a self-standing realm of freedom, are made possible by, and only fully intelligible in the light of, Descartes' radically new sort of mathematical practice. Because this new practice is—or at least seemed to Descartes to be—the work of the pure intellect, with Descartes the immediacy of perception, of our capacity to take in things as they are, is supplanted by the new way of ideas. Our intentional directedness on reality is henceforth to be conceived as inherently mediated, as constitutively involving both the will and (mental) representations, representations that are judged to be true through acts of will. The age of understanding has begun.

Descartes' transformed understanding of mathematical practice realizes a new mode of intentional directedness and a new conception of reality. The ancient paradigm of being, a living animal with its form of life and characteristic powers, both motor and perceptual, gives way to a new paradigm, that of an object governed by laws, laws of nature in the case of bodies, and laws of reason and freedom in the case of the mind. Implicit in Descartes' conception of a law is, furthermore, a new conception of concepts. Whereas on the ancient view concepts are constitutively sensory and object involving, on the modern conception made possible by Descartes' new form of mathematical practice, and made explicit in Kant, concepts are merely predicates of possible judgments. They do not give objects but are instead that through which independently given objects are thought. In distinguishing between concepts and intuitions as he does, separating thereby the logical functions of referring and predicating that are inextricably combined in the thought of the ancient Greeks, Kant makes the first significant advance in logic since Aristotle.

According to the account developed in Chapter 4, Kant's critical philosophy steers us between the horns of a dilemma that inevitably arises in the wake of Descartes' transformed conception of being, between, that is, dogmatism and skepticism, and it does so by rejecting the foundationalist picture of knowing on which the dilemma rests. There is no given foundation for knowledge and nor, as Kant almost sees, is any such foundation needed for knowledge. It is the *activity* of inquiry, not its products, that holds the key to an adequate conception of truth in the exact sciences, the activity of construction in pure intuition in mathematics and the activity of hypothesis, experimentation, and correction in physics, and in metaphysics.[8] But because, on Kant's account, all objectivity lies in relation to an object, which is understood in turn as an all-sided determination, it follows directly that no finite contingent being can know an object as it is in itself, as it would be grasped in the intellectual intuition of an infinite being. We finite beings can know things only as they appear to us, as they are informed by the forms of sensibility and understanding. Because these forms are the conditions of the possibility of the activity of reason both in its intuitive use in mathematics and in its discursive use in pure natural science, they are at the same time the conditions of the possibility of knowing.

Cognition, on Kant's account, constitutively involves both the spontaneity of the understanding and the receptivity of sensibility and it does so (we will see) in a way that enables Kant to recover something very like the ancient conception of experience as revelatory of things as they are, not, however, in perception of an object (as for the ancient Greeks) but in acts of judgment. With Kant, cognition is to be conceived not as a relation to an object (as what it is in its nature) but instead takes the form of a judgment, that things are thus and so. And at least empirically, judgment on Kant's view is an act of what we will call expressive freedom that is completed by the reality on which it aims to bear. It is revelatory of manifest facts. But such freedom is possible, Kant holds, only in light of a transcendental synthesis. Transcendentally, judgment is an act of productive freedom, of a form of freedom that makes something from something that is found. Hegel saw that this cannot be right. He also saw, in essence, how Kant's transcendental idealism is to be avoided, namely, by recognizing that contingency is not incompatible with knowledge of things as they are in themselves, with Absolute Knowing, but instead constitutive of it. And we will be able to recognize this, he saw, when we take into account also our historicity, the fact that the pursuit of truth is not only an activity but one with a dialectical history of growth, development, and transformation. What Hegel lacked was any adequate basis on which to develop this idea. In the end, it was not Hegel's critique that dealt the

[8] Kant does suggest, in the last paragraph of the Introduction to the *Jäsche Logic*, that hypotheses are not allowed either in mathematics or in metaphysics; but in the B Preface of the first *Critique* he describes his method as "imitated from the method of those who study nature" and suggests further that the "experiment" that is the first *Critique* "succeeds as well as we could wish" (Bxviii). Kant's understanding of sensibility and understanding as the "two stems of human cognition," in particular, seems not to be something that Kant has discovered; it is instead hypothesized. (See the Introduction, A15/B29.)

deathblow to Kant's critical philosophy but instead the emergence over the course of the nineteenth century of the new mathematical practice of reasoning in concepts, *Denken in Begriffen*.

The focus of Chapter 5 is the new mathematical practice that emerged in nineteenth-century Germany, both the respects in which it is different from the practice with the symbolic language of arithmetic and algebra that was inaugurated by Descartes and developed further by Leibniz, Euler, and others, and the ways it seems to have been understood by its practitioners, in particular, by Galois, Riemann, Dedekind, Hilbert, and others.[9] In Chapter 6, we turn to the twentieth-century response to these developments as it was shaped by the rise of mathematical logic. As we will see, the path forward from nineteenth-century developments in mathematics that jettisons constructions in favor of an essentially Kantian account of deductive reasoning from concepts holds out little prospect of shedding light on the problem of truth and knowledge in mathematics.

Chapter 7 makes a new beginning with Frege. As Kant made the first real advance in logic beyond the Greeks by distinguishing logically between concepts and intuitions, so, we will eventually see, Frege makes the first real advance beyond Kant. Much as, from the perspective afforded by Kant, we can see that the ancient notion of a term conflates the logical function of referring (to an object) with that of predicating (of an object), so from the perspective afforded by Frege we see that that latter distinction between referring and predicating is similarly a conflation. It conflates the distinction between concepts and objects with that between Fregean sense (*Sinn*) and signification or meaning (*Bedeutung*). It is in just this way that Frege achieves an essentially new, post-Kantian conception of concepts as fully objective entities in their own right, entities that can be grasped in thought and as so grasped provide the basis for judgment and inference. In so doing, Frege realizes a new conception of language, one that enables us explicitly to recognize the transformed mode of intentional directedness on reality that first emerged in the nineteenth century, the third shape of spirit that is reason.

Frege's *Begriffsschrift* was to be a mathematical language, modeled on the symbolic language of arithmetic and algebra, within which to exhibit the contents of mathematical concepts and to reason deductively from them. *Begriffsschrift* was, that is, to be a mathematical language that could serve as a vehicle of the sort of reasoning that has been standard in mathematical practice since the nineteenth century. Frege's 1879 monograph of the same name introduces the language and aims to show by

[9] Readers may be surprised, and dismayed, to find that there is no sustained discussion of Leibniz's work in the chapters to follow given that our central concern is with mathematical practice. Certainly it is true that already in Leibniz's work one finds many, many ideas and intimations that will come to fruition in the late nineteenth century. As I understand the situation, Leibniz was extraordinarily prescient, and for just that reason cannot be a focus here. Leibniz did not have, and if I am right could not have had, the influence on the unfolding of events that concerns us here that he in some sense should have had precisely because he was so much before his time.

example how it is to be used in defining concepts and deducing theorems on the basis of definitions. The proof of theorem 133 in Part III of that work is to illustrate how it is that a strictly deductive proof from definitions can constitute a real extension of our knowledge. Chapter 7 clarifies and explains all the essentials of this proof.

In Chapter 8 we see that Frege's proof shows, to one with the eyes to see, both that and how even reasoning deductively from concepts in mathematics can be ampliative, a real extension of our knowledge. Frege's fundamental logical advance is clarified, and this clarification enables us finally to make precise various essential differences between natural and mathematical language. The account of the practice of mathematics that emerges reveals not only that this practice is an achievement of reason but also that through it the power of reason itself is, for the first time in history, fully realized as a power of knowing.

We turn in the final chapter to some of the implications of the developments of the previous chapters for the practice of fundamental physics in the twentieth century, in particular, special and general relativity and quantum mechanics. A sketch is provided of some essential features of this new way of doing physics and of the peculiar role mathematics plays in it, and we see how certain characteristic themes, sounded already in our discussion of the practice of mathematics since the nineteenth century, can be discerned again in this new practice of physics. As we will also see, on this way of doing physics one does not merely use mathematics; in an important sense this physics just is mathematics. With twentieth-century fundamental physics, the project of modernity first begun in the seventeenth century is completed.

Perception

The cognitive transformations that realize us as knowers of things as they are in themselves, the same for all rational beings, belong to our intellectual history as the rational animals we are. We need to begin, then, with what it is to be an animal with the power of reason, the capacity for critical reflection and second thoughts, an animal answerable to the norm of truth.

Rational animals are not merely biological beings but instead essentially social, historical, and cultural; we are not—and for reasons that will become clear, could not be—born rational. Instead we become so through our acculturation into a tradition, a central element of which is a natural language that is itself socially evolved. It is natural language that at once empowers us to see, to have the world in view, and realizes the world as world, as that to which our judgments answer. There is, then, a pre-established harmony between ourselves as the rational animals we become through our acculturation and the world as it first comes into view. We can *see* how things are, perceptually take in things as they are. For us as we first find ourselves to be, this capacity, realized with the acquisition of a natural language, provides the model for all intellection, all thought.

Perception is of objects, first and foremost living beings, paradigmatically animals with their characteristic natures and powers of self-movement. Inanimate stuffs, fire, water, earth, and air, are conceived analogically, as likewise having natures and the power of self-movement to their natural places. The world as perceived is a world of beings and the characteristic happenings of such beings, for instance, as in the case of animals, their birth, growth, decay, and finally, death. Time passes, things change, and we perceive that this is so. But we also learn to perceive, as with the mind's eye, things that do not change but seem timelessly to be what they are, mathematical things such as triangles, circles, and numbers. And we discover truths about these things, in some cases merely by reflection and inference, in others through the diagrams we draw. And we know ourselves to do this. Because this first shape of consciousness, realized in and through natural language, has its primary mode of intentional directedness in perception, it is natural to try to account for this power of knowing mathematical truth in terms of another mode of being than that of the sensible, changing things we find all around us, a mode of unchanging being, the mathematicals and ultimately the Platonic Forms that are grasped with the eye of the mind in a kind of intellectual seeing.

To be for this first mode of consciousness is to have a nature, to be an instance of a kind, paradigmatically a form of life; and to know, for it, is to know a thing as what it is, in its nature. Knowing, that is, is constitutively of objects, of things with natures. Like the natural language that speaks it, this knowing is inherently sensory and object involving. Our first cognitive involvement in the world thus has the form of immediacy. Things just do show up, are revealed as they are, in perception, and in intellection conceived on the model of perception, as the cognitive grasp of a thing with the mind's eye. Objectivity correspondingly lies in the distinction between an object and one's experience of it: to err is to take what is merely an appearance, merely a feature of one's experience of a thing, for reality, as what is the case with an object independent of one's experience of it.

Then, with Descartes, there is an inversion, a reversal in our mode of consciousness. Not perception of an object but (as Descartes thinks of it) pure intellection, thought trained not now on an object but instead on the mathematical relations that can obtain among objects, becomes the paradigmatic mode of intentional directedness and the model even for perception. Our understanding of the being of beings is likewise transformed. What most fundamentally is is not now to be understood in terms of the notion of a form of life, an animal with its nature and characteristic powers, but instead in terms of the essentially modern notion of a law. To be, on the modern view, is to be subject to a law, either a law of nature or a law of freedom. Likewise, truth is not now a matter of revelation, the immediate grasp of a thing as what it is, in its nature, but is instead correctly to picture something that is the case, a fact.

And our understanding of objectivity is similarly transformed. It lies not in the distinction between an object and one's experience of it but instead in the God's eye, purely intellectual view (a view that Descartes thinks we also can achieve), in the view from nowhere as contrasted with the essentially sensory view from here. And all this occurs because it is not now natural language but instead mathematical language, in particular, the symbolic language of arithmetic and algebra first devised by Descartes—the language that "proved to be the greatest instrument of discovery in the history of mathematics" (Grabiner 1974, 357)—that provides us, that is, the pure intellect, with the eyes to see, to know things as they are. For this shape of spirit that is and knows itself to be radically other than its ancient forebear, our everyday experience of the world comes to seem to be nothing more than a kind of dream from which we have finally awakened. As we now know ourselves to be, we are strangely situated in a world grasped in thought—a world that as wholly other must ultimately withdraw even from the eyes of the mind.

1

Where We Begin

Imagine a cat lapping some milk. It would clearly be a mistake to suppose that although the cat is a certain sort of living organism with a characteristic form of life, the milk she laps is merely a chemically described physical stuff. Of course we can conceive the milk in chemical terms, as indeed we can conceive the cat. That is, we can think of the cat not as a living organism but as a mere lifeless thing, as a collection of biochemical processes that can equally well be observed in a test-tube. But if we are going to talk organism and form of life talk on the side of the cat then we need to talk in the same terms on the side of the milk she laps: the milk is *food* for the cat; it *nourishes* her. And the same is true of the cat's environment generally; it is replete with what Gibson has taught us to call affordances.[1]

We are not much tempted to suppose, in the case of an animal such as a cat, that "inside" is a living sentient being while "outside" is mere physically describable stuff, that mere stuff somehow becomes nourishment for an animal on being ingested. But we are very tempted to suppose in our own case that "inside" is a conscious, rational being while "outside" is mere stuff as described in our best physics, that, for instance, mere wavelengths of light somehow cause color experiences in us on being seen.[2] In our own case, we talk, that is, in normative terms about our "insides," about us insofar as we are self-conscious rational beings, but then switch over to non-normative, merely causal terms in our talk about the world around or "outside" us. Here again, one *can* talk physicalist causal talk across the board, for instance, about light hitting our retinas, about neuron firings, and so on. But much as in the case of the cat, if one is going to talk normative talk on the side of a person then one must talk in the same sort of terms about that person's environment.[3]

[1] This notion is developed and explained in section 1.2 below.

[2] As already noted in the Introduction, this conception can seem obviously right, something science itself has taught us. As McDowell (1994, 82) puts it, "this kind of naturalism tends to represent itself as educated common sense." But, he thinks, it is not: "it is really only primitive metaphysics." As already indicated, I would agree that it is something that must be gotten over, but I do not think that it is merely accidental that we find ourselves with such a view at a certain point in our intellectual history.

[3] This is, of course, not an argument but merely a beginning. The argument will come with the unfolding of the history we are here concerned to trace. In particular, we will need to understand the advances that come *after* the "discovery" of the sideways-on view before we can fully understand why it is mistaken, why we can and should let it go.

As mere stuff does not become food for an animal on being taken up and eaten so wavelengths of light do not become (experienced) colors by being taken up by our eyes and processed by our brains. Mere physical stuffs are not transformed into food by being eaten by an animal but instead already have the biological significance of food by the time there is anything to call an animal on the scene at all, and mere physical stuffs are not transformed into perceptual experiences by some activity of our brains but instead already have the perceptual significance of being, say, red, by the time there is anything to call a perceiver on the scene at all.[4] We already understand, well enough, how biological evolution might realize living organisms with their biologically significant environments. The task of this chapter is to understand, well enough, how processes of social evolution might realize self-conscious rational beings in a world of perceptible objects, and to clarify in particular the role that language plays in this process. All that we will suppose, by way of the peculiar biological endowments of animals capable of enacting such processes of social evolution as eventuate in the emergence of rational animals, are capacities of our ancestors to imitate one another and to cooperate and share goals, and the ability to synthesis into a single whole one's procedural knowledge of the layout of a terrain.

1.1 The Limits of Cartesianism[5]

Although not in the case of mere animals, we have a tendency in our own case to think that "outside" are merely physically described stuffs while "inside" are normatively significant, meaningful experiences. And this picture we have of our being in the world is, we think, required by advances in the natural sciences, and as such is a given for any and all philosophical reflection. It is most obviously manifested in work—for instance, in cognitive science and consciousness studies—that aims to understand how the characteristic activities of brains might realize, or eventuate in, consciousness, that is, minds.[6] It is not, however, confined to such work. Just the

[4] As we will eventually recognize, a sensory property such as redness is not the most basic sort of thing we perceive. What we are aware of first and foremost are things that matter to us, friend and foe, predator and prey, and so on. Once we understand the nature and role of a socially evolved language in our coming to be aware, this will seem much less surprising than it must at first seem.

[5] Those who take the sideways-on view do not generally think of themselves as Cartesians, and in a way, they are not, not if being a Cartesian requires that one is a substance dualist about mind and body. Those who take the sideways-on view today are most likely to think that somehow the "inside" is a product or effect of "outside" processes in brains. But that is merely one way one's thinking might go after the sideways-on view has already been taken up. In this section we consider another. The real target is the sideways-on view itself, which is what I mean by Cartesianism. Why that is the right label will become clearer in Chapter 3.

[6] Chalmers (1995), for example, assumes that we will have an explanation of some cognitive ability when we have specified a mechanism that performs the relevant function: "To explain reportability, for instance, is just to explain how a system could perform the function of producing reports on internal states. To explain internal access, we need to explain how a system could be appropriately affected by its internal states and use information about those states in directing later processes. To explain integration and control, we need to explain how a system's central processes can bring information contents together and

same tendency is manifested also in a dominant strand of social pragmatism according to which "outside" is the world as described in our best physics, while "inside" are socially instituted norms. An especially well developed account of this sort is Brandom's in *Making It Explicit*. Understanding its limitations will clear the way for the very different sort of account that is pursued here.

In *Making It Explicit*, Brandom aims "to offer sufficient conditions for a system of social practices to count as specifically *linguistic* practices" (1994, 7), an account of "the social practices that distinguish us as rational, indeed logical, concept-mongering creatures—knowers and agents" (1994, xi). The task is to spell out, make explicit, what something would have to be able to *do* in order to count as *saying*, to specify under what circumstances a response to something is not merely causal, but instead an instance of speaking, of deploying concepts, of judging. As Brandom (1994, 87) explains:

> classification by the exercise of regular differential responsive dispositions may be a necessary condition of concept use, but it is clearly not a sufficient one. Such classification may underlie the use of concepts, but it cannot by itself constitute discursiveness. The chunk of iron is not conceiving its world as wet when it responds by rusting. Why not? What else must be added to responsive classification to get to an activity recognizable as the application of concepts? What else must an organism be able to do, what else must be true of it, for performances that it is differentially disposed to produce responsively to count as applications of concepts to the stimuli that evoke those responses?

As Brandom sets things up, to see and so to say, for instance, 'that's yellow' when presented with a ripe Chiquita banana is similar to a chunk of iron's rusting in the rain insofar as both responses involve a reliable differential responsive disposition, a merely causal process. But they are of course also importantly different. And here Brandom follows Kant in taking it that we have two options: either we think of the sequence of events presentation-of-banana-followed-by-an-utterance-of-'that's-yellow' as behavior *in accordance with* a rule or norm, that is, as *causally* necessitated, or we think of it as an action *according to one's conception of* a rule or norm, that is, as *rationally* necessitated. The first sort of necessity is clearly not sufficient for an adequate understanding of the response as a piece of language use; in speaking one is not merely causally necessitated to make the noises one makes. So we must choose the second: to say 'that's yellow' when presented with a ripe Chiquita banana is to act according to one's conception of a norm or rule. But this, Brandom thinks, will not do either because such an action presupposes rather than explains one's capacity to

use them in the facilitation of behavior. These are all problems about the explanation of functions" (Chalmers 1995, 202). What Chalmers thinks is the hard problem of consciousness is the experiential or felt aspect that in our mental lives accompanies the performance of such functions, and it is hard "precisely because it is not about the performance of functions" (Chalmers 1995, 202). "Why," Chalmers (1995, 201) asks, "should physical processing give rise to a rich inner life at all?" Humphrey (2006) aims to answer just this question. It is, however, the wrong question to ask—as we will eventually see.

use language with meaning and understanding. To follow an explicitly formulated rule is already to be able to assign it the requisite meaning, already to have the conception in question, on pain of a vicious regress. This, Brandom suggests, is precisely the point of Wittgenstein's regress argument in the *Investigations*.

We begin with an explicit expression of a rule, for example, a signpost pointing the way. But there is no given or intrinsic meaning to the signpost, no meaning that it has independent of the ways we actually go on in light of it. The signpost is in itself merely some physical stuff in some configuration; it does not itself settle what is the right way to go on along the trail. And because the signpost does not itself point the way for us, we suppose instead that the signpost has the meaning it does for us because we *respond* to it in a certain way, because, as Wittgenstein puts it, the signpost is *interpreted* as pointing one way rather than another. (That is, having supposed already the sideways-on view, we take it that "outside" is mere meaningless stuff, that such stuff comes to have meaning, significance for us, only in virtue of an "inside" response that confers meaning on it.) But it can then be argued that this interpretation too has no given, intrinsic meaning, no meaning independent of the ways we actually go on in light of it. Suppose, for example, that having seen the signpost I raise my arm and point in the direction we are to go. That bodily movement has no intrinsic meaning; it does not, in itself, mean that (say) the trail goes off to the left. Similarly, if I speak, say, for instance, the words 'here we turn left', the same problem arises. The utterance is itself nothing more than sounds in the air; it has no intrinsic meaning. In order for my interpretation of the signpost, whatever form that interpretation takes, to be a normatively significant response, a *taking* of the sign as meaning such and such, a further interpretation would seem to be needed. But this clearly starts a regress. Insofar as my interpretation, whatever it is, is itself normatively inert, that interpretation cannot resolve our original difficulty. Like the signpost, my interpretation "just stands there"; it is utterly meaningless. "Interpretations by themselves do not determine meaning" (Wittgenstein 1976, sec. 198). Brandom's thought is to stop the regress of interpretations by appeal to norms implicit in practice.

As Brandom reads Wittgenstein, the regress of interpretations that is generated by thinking of rule-following as an action that is according to one's conception of a rule can be stopped by norms implicit in practice. As he writes (1994, 20),

> norms explicit as rules presuppose norms implicit in practice because a rule specifying how something is correctly done (how a word ought to be used, how a piano ought to be tuned) must be applied to particular circumstances, and applying a rule in particular circumstances is itself essentially something that can be done correctly or incorrectly. A rule, principle, or command has normative significance for performance, only in the context of practices determining how it is correctly applied.

To understand, then, the essential difference between a person's saying 'that's yellow' in the presence of something yellow and, say, a parrot's (trained) squawk of 'yellow' in the same circumstance, we need to consider the norms implicit in practice that for the person but not the parrot underlie her commitment to explicit linguistic rules such as that enjoining us to call yellow things yellow.

We have seen that Brandom's account begins with Kant's distinction between behavior in accordance with a rule and action according to a conception of a rule, between the realm of nature and causes, and that of freedom, reasons. But because action according to a conception of a rule presupposes rather than explains both one's understanding of the rule and one's awareness of the circumstances in which the rule is correctly applied, Brandom also distinguishes within the realm of freedom between action that is explicitly undertaken in accordance with a rule and action that is only implicitly, in practice, undertaken in accordance with a rule. Thus, he thinks (1994, 46), "there are three levels at which performances can be discussed: a level of norms explicit in rules and reasons, a level of norms implicit in practice, and a level of matter-of-factual regularities, individual and communal." It cannot be explicit norms all the way down because, again, such a conception presupposes rather than explains both one's understanding of the norm and one's awareness of the circumstances in which the rule is applicable. And it cannot be reliable differential responsive dispositions all the way up because although such dispositions can account for the responses we in fact have to things, they cannot provide any basis for the distinction between what we do and what we ought to do.

> If anything is to be made of the Kantian insight that there is a fundamental normative dimension to the application of concepts (and hence to the significance of discursive or propositionally contentful intentional states and performances), an account is needed of what it is for norms to be implicit in practices. Such practices must be construed both as not having to involve explicit rules and as distinct from mere regularities. (Brandom 1994, 29)

We are to start with norms implicit in practices that do not presuppose conceptual content, and so truth, and then build up to such content.

Practices, and the norms that are implicit in them, are the raw materials out of which Brandom aims to build something that counts as speaking. But of course it is not enough that one takes the signpost, in practice, to point this way or that (by behaving in relevant ways). For in that case whatever seems right to one is right, and then we cannot talk of right, or wrong, at all.[7] Instead, on Brandom's account, a piece of behavior that is a case of rule following in practice—which is a matter of having a certain status, of counting as something that is constrained by norms—is itself constituted as such by various attitudes, "effective assessments of propriety, in the form of responsive classifications of a performance as correct or incorrect" (Brandom 1994, 35). Brandom needs, then, to tell us what something would have to be able to

[7] Wittgenstein (1976, sec. 258) makes this point in the context of a discussion of the idea of a private language: "I have no criterion of correctness. One would like to say: whatever is going to seem right to me is right. And that only means that here we can't talk about 'right'."

do to count "as engaging in practices that implicitly acknowledge the application of norms... The question is what role such a response must play in order to deserve to be called a practical taking or treating of some performance *as* correct or incorrect" (Brandom 1994, 33).

As Brandom understands it (1994, xiv), "the natural world does not come with commitments and entitlements in it; they are the products of human activity. In particular, they are creatures of the *attitudes* of taking, treating, or responding to someone in practice *as* committed or entitled (for instance, to various further performances)." The thought is to "demystify norms" by

> [understanding] them as *instituted* by the practical attitudes of those who acknowledge them in their practice. Apart from such practical acknowledgement—taking or treating performances as correct or incorrect by responding to them as such in practice—performances have natural properties, but not normative proprieties; they cannot be understood as correct or incorrect without reference to their assessment or acknowledgement as such by those in whose practice the norms are implicit. (Brandom 1994, 63)

Nature itself is normatively inert but on Brandom's account our responses to it, most fundamentally, our attitudes, are not; and these attitudes are basic for him. The "basic building block" of Brandom's version of what he calls I—Thou sociality (as it contrasts with I—We sociality) "is the relation between an audience that is attributing commitments and thereby keeping score and a speaker who is undertaking commitments, on whom score is being kept" (Brandom 1994, 508).[8]

Brandom's account is clearly Cartesian insofar as, for Brandom, what is outside, "nature," is normatively inert, merely causal. In particular, "authority is not found in nature. The laws of nature do not bind us by obligation, but only by compulsion. The institution of authority is human work; we bind ourselves with norms" (Brandom 1994, 51). And we can so bind ourselves, on his account, because we are capable of normatively significant attitudes of assessment. These attitudes are the unexplained explainers of the account; they are intrinsically normative and as such are irreducible to anything in nature because nature is non-normative, merely causal. In sum, for Brandom, what is "outside" is merely causal, normatively inert, while what is "inside," attitudes, are essentially and irreducibly normative. Merely causal stimuli occasion normatively significant responses, attitudes of assessment.

Like any other physical object, we respond to stimuli. Brandom's task is to understand how our responses to impacts of the merely causal world, unlike those of chunks of iron or of parrots, can be counted as properly conceptual, answerable to the norm of truth; and the idea, we have seen, is to appeal to norms instituted by

[8] It is worth remarking that, despite the importance of procreation in animal life, the "sociality" of animal life is not I–Thou but I–We. As we will see in the next section, animals are constituted by their natures, their forms of life; they are essentially instances of kinds. We will later see that the same is true in other cases as well.

attitudes and implicit in practice as the basis for understanding explicit rule following. There are at least two fundamental objections to any such project.[9]

First, although Brandom aims to demystify norms by grounding them in attitudes, in fact his account makes a mystery of norms by founding them on responses that must be conceived as inherently, that is, irreducibly, non-natural. Because norms and nature, as Brandom understands them, are essentially opposed, the norm-laden responses that are the unexplained explainers of the account simply cannot arise within the causal realm, the realm of nature. We just are, on this account, capable of having attitudes that are norm-laden. And given the original dichotomy of reasons and causes, these attitudes are and must be primitive; they cannot be reduced to something non-normative, something merely causal. There are, then, in the world as Brandom depicts it, not only merely physical stuffs with their causal powers but also unexplained, and ultimately inexplicable, normative attitudes that we just are capable of adopting. To suppose otherwise, to suggest that the normative attitudes that are his fundamental building block might arise out of the merely causal, would be to fall into what Sellars calls the Myth of the Given; it would be to take something merely causal somehow to have normative significance. Brandom's account does not, then, meet the constraint of naturalism. It must suppose something essentially non-natural, something opposed to nature, in order so much as to begin.[10]

The second objection concerns not Brandom's starting-point—which, I have just suggested, is and must be non-natural—but instead his endpoint, that is, the notion of objectivity that he is in fact able to achieve. Brandom aims to avoid more blatantly Cartesian projects by starting not with full-blown intentionality, that is, with rational beings answerable to the norm of truth, but more modestly with creatures capable of taking up attitudes that institute, at least in the first instance, social (rather than objective) proprieties of correctness and incorrectness. His task, the project of the book as a whole, is to build up to the sort of intentionality that is characteristic of rational beings answerable to the norm of truth. Although we do not start with them, the aim is to end up with something that is evidently an account of rational beings. Thus, in Chapter 8, we are provided with what Brandom describes as his "objectivity proofs" to show that the notion of truth is distinguished in his account from the notion of assertibility. Brandom (1994, 606–7) claims, in particular, that those proofs

> define a robust sense in which the facts as construed in this work are independent of what anyone or everyone is committed to. The *claim-making* practices described here are accordingly properly understood as making possible genuine *fact-stating* discourse, for they incorporate practices of assessing claims and inferences according to their *objective* correctness—a

9 Rödl (2007, 114–17) discusses a third sort of problem an account such as Brandom's faces.

10 It should not be supposed, though it often is supposed, that the only way to be a naturalist is by taking the sideways-on view, as if the only respectable intellectual endeavor were natural (i.e., empirical) science. Mathematics is not an empirical science and it is a respectable intellectual endeavor; and the same is true of philosophy, at least when it is done well.

kind of correctness that answers to how things actually are, rather than to how they are *taken* to be, by anyone (including oneself) or everyone.

In fact, as many readers have recognized, Brandom does not, and cannot, prove what is needed, that what is objective is what is the case anyway, however we take things to be, *and* that in judging we are answerable to what is objectively the case. Haugeland (1998, 358, n. 14), for instance, notes:

> what the proofs show is that there is no legal move, in Brandom's system, from 'Everyone believes *p*' (or: 'I believe *p*') to '*p*'. But they don't show anything at all about what *could* legitimate '*p*' instead; in particular, they don't begin to show how '*p*' could "answer to how things actually are"—which is just to say that they don't show how any of the moves could be *claim*-makings or *fact*-statings.

On Brandom's account judgment is not answerable to things as they are; and nor can it be given his conception of the way the normative is contrasted with the causal. If what is "outside" is merely causal, if it is normatively inert, then there is simply no possibility of its rationally constraining our judgments. And then, as Haugeland notes, there is no reason to call it *judgment* at all.

Wittgenstein argues that to understand the meaning of, say, a signpost by appeal to an interpretation of it—on the grounds that independent of any interpretation the signpost is a merely causal thing, that is, normatively inert—generates a vicious regress of interpretations. Brandom suggests that the regress can be stopped by appeal one's implicit grasp of a rule as manifested in one's practice. We have seen two problems with this response. First, it requires introducing something essentially non-natural into an otherwise merely causal world. This is mysterious and hence unsatisfactory. The second problem is that Brandom cannot in any case provide what is wanted, an account of the objectivity of judgment, the fact that in judging we are answerable to how things are. A more radical—and arguably more Wittgensteinian (see McDowell 2002)—response to Wittgenstein's reflections on rule following in his *Philosophical Investigations* is to jettison the whole Cartesian/Kantian framework within which causes and norms are ineluctably opposed together with the sort of reductive, mechanistic explanation that that tradition engendered. Although what is *merely* causal is normatively inert, nonetheless, we will see, what is merely causal can *acquire* normative significance in certain contexts. Much as a mere stuff can acquire, in the course of the evolutionary emergence of animals, the significance of being food, that is, of being (appropriately, or normally) nourishing for an animal of a certain sort, so what at first is merely causal can come also to be a reason for us. The signpost itself, in such a context, can tell us how to go on.[11]

[11] As McDowell puts the point in *Mind and World*, on this sort of account there is no outer boundary around the conceptual beyond which lies the reality on which thought aims to bear, that is, no conceptually articulated "inside" and non-conceptual "outside." The sideways-on view has been dismantled. Again, fully to understand why it can be dismantled in this way requires resources that will become available only much later in our account.

We will be interested, for the remainder of this chapter, in three different processes: the emergence of life through the process of biological evolution, the emergence of games through a process of social or cultural evolution, and the emergence, finally, of rational beings through a profound but explicable transformation in players of a particular sort of socially evolved game. In each case, what emerges is not merely a new kind of being within what is correctly described as a merely causal world but also a new sort of significance for the world itself. As biological evolution realizes not only sentient animals but also the environments within which they live out their characteristic forms of life, so social evolution realizes not only players of games but also the arenas within which those games are played. The emergence of rational beings, finally, is at the same time the emergence of the world as such, the objective reality on which thought aims to bear. And all three are needed because although biological evolution can realize conscious beings, animals that are perceptually sensitive to their surroundings, and can feel, for instance, pain, anger, and fear, it cannot realize self-conscious rational beings, beings answerable to the norm of truth. Truth is not a biological category. Nor is it a merely social category. Although we must begin with biological evolution through which living beings first emerge, that account must be supplemented with a further evolutionary process, the social evolution of creatures with characteristic practices, creatures able through their practices to institute the purely social significances that are involved in games. These purely social significances will then provide in turn a medium for the coalescence of the loci of authority that are the hallmark of the cognitive orientation on the world that is required for the pursuit of truth. As we will see, it is possible to be fully naturalistic (non-dualistic and non-magical) about our own cognitive and intellectual capacities, but only if we begin with life itself as the fruit of biological evolution, and then supplement that conception of life with the fruits of social, and ultimately intellectual, evolution as well.

1.2 Biological Evolution and the Concept of Life

With the rise of modern science we learned to distinguish between, on the one hand, the order newly discernible in nature in terms of exceptionless physical laws, and on the other, ourselves as radically autonomous, self-defining rational subjects, between, that is, the realm of nature and the realm of freedom. But commitment to the idea that everything in nature is describable in the vocabulary of fundamental physics is compatible with the idea that there are other orders of intelligibility to be discerned in nature as well.[12] The theory of evolution by natural selection shows that

[12] Here again we find a tendency inherited from early modern science, the reductive tendency to want to explain all orders of intelligibility in terms of one favored one. Though, as we will eventually see, it is not surprising that scientists in the seventeenth and eighteenth century would have thought that there is only one, or only one uniquely privileged, view of things as they (really) are, we today have very little motivation

these two ideas are compatible by showing how radically new *sorts* of beings can come into being, and how, as a result, inanimate nature too can acquire a radically new sort of significance.[13]

According to the theory of evolution by natural selection, evolution occurs if there is, first, variation, an ongoing source of novel features, second, a mechanism of their replication or inheritance, and third, differential fitness among those replicable variations. Over time, millions and millions of years, biological evolution realizes beings that are properly described as alive, as having biological natures and natural habitats. Living things are not, in other words, merely things that can do things, as, for instance, mere mechanisms are; they are instead constitutively instances of kinds or species. Living things have *natures*, and thereby a distinctive mode of being (relative to the being of a mere mechanism, a merely causal structure). They have characteristic forms of life.

We find it easy to fall into thinking of living things in terms of various characteristic features or behaviors. Much as Brandom asks in *Making It Explicit* what something would have to be able to do to count as speaking, deploying concepts, so we tend to ask what something must be able to do in order to count as alive. We list various features and capacities of living things: being highly organized, homeostatic, adaptive, able to reproduce, and so on. But as Thompson (2008) has argued, the question what something would have to be able to do to count as alive is the wrong question. What distinguishes living things first and foremost is not what they can do, but what they *are*. Living things may be, as we say, as if designed by the processes of biological evolution rather as the machines we build are actually designed; nevertheless, living things have an essentially different mode of being from mere mechanisms. They are, constitutively, instances of life forms.

Consider, first, the ability to respond to stimuli, which is something both living things and merely physical things, including mechanisms, for instance, automated doors, can be said to have. Thompson (2008, 40) writes:

> *The warming of an asphalt roadbed* and *the train of photosynthetic events in a green leaf* are both of them, in some sense, the effect of sunlight. And *the thawing of icy ponds* and *the opening of maple buds* are each occasioned by rising spring temperatures. It is natural, though, to think that the two vegetative phenomena belong together as instances of a special type of causal relation, or a causal relation with special conditions, distinct from any exhibited in asphalt or water.... On the other hand, though, the effect of *the hydrogen bomb* on a rose, and on a road-bed, will be pretty much the same—at least if they are both at ground zero. I mean

for such a view. Animals are not mechanisms, and the theory of evolution helps us to see that even so they are fully, and unproblematically, natural.

[13] To one who has already taken up the perspective of the sideways-on view, this last point will seem very puzzling insofar as biological evolution by natural selection is understood from that perspective as a kind of as-if design of the organism conceived as nothing more than a complex mechanism. What we in fact ought to see that is the idea of biological evolution by natural selection itself already provides us the resources to begin dismantling the sideways-on, inside/outside view.

> not only that the effects will be similar, but also that the type of causality will be the same. It is in a more restricted range of cases that we seem to see a difference, if the affected individual is an organism.

Intuitively, the difference Thompson points to in this passage should be clear. Our understanding of the process <sunlight → warming of asphalt roadbed>, on the one hand, and of the process <sunlight → train of photosynthetic events in a green leaf>, on the other, are very different. In the first case we have a merely causal connection, one that is fully explicable by appeal to certain molecular structures (in asphalt) and the effects of sunlight on such structures. Thompson's interest is in the fact that if we try to conceive the second process as another instance of such a causal connection, something essential goes missing, something that can be highlighted by the question: what happens next?

In the case of a photosynthesizing leaf, there is a natural answer to the question 'what happens next?', namely, whatever then goes on in the course of a properly functioning, that is to say, living, thriving, plant leaf. There is no comparable answer in the case in which the connection is merely causal.

> In a description of photosynthesis, for example, we read of one chemical process—one process-in-the-sense-of-chemistry, one "reaction"—followed by another, and then another. Having read along a bit with mounting enthusiasm, we can ask: "And what happens next?" If we are stuck with chemical and physical categories, the only answer will be: "Well, it depends on whether an H-bomb goes off, or the temperature plummets toward absolute zero, or it all falls into a vat of sulphuric acid . . .". That a certain enzyme will appear and split the latest chemical product into two is just one among many possibilities. Physics and chemistry, adequately developed, can tell you what happens in any of these circumstances—in *any* circumstance—but it seems that they cannot attach any sense to a question "What happens next?", *sans phrase* . . . . it is not just that 'the rose and maple are subjects of processes of their own': they are also subjects of a special type or category of process—'biological' processes, if you like, or 'life-processes'. (Thompson 2008, 41–2)

Such processes, Thompson argues, are distinguished not by their content but instead by their form. They exhibit a different sort of unity from merely physical, that is, non-biological, processes.

In the case of a living organism, the question 'what happens next?' (*sans phrase*) makes sense because living organisms have characteristic forms of life, because they are instances of biological species. What happens next is what happens in the life not of this or that individual but in the life of that species of organism: when springtime comes and the snow begins to melt, the female bobcat gives birth to two to four cubs. The bobcat is a species or life form, something about which just such a judgment can be made. Furthermore, such an answer to the question 'what happens next?' can be correct even if the answer is in fact false of most instances of the kind. For example, it is perfectly correct to say that the mayfly breeds shortly before dying despite the fact that most die long before breeding. A judgment such as that the mayfly breeds shortly before dying "may be true though individuals falling under both the subject and predicate concepts are as rare as one likes, statistically speaking" (Thompson 2008, 68). To understand something as alive is in this way to see it *as* an instance of a biological kind and so as having a distinctive form of life, one that takes the form of a story, a narrative with a characteristic beginning (what happens first), middle (what happens next), and end (what happens last), whether or not all or even most instances of the kind in fact realize such a life.

As Thompson argues, it is possible to understand what a living being *does* only in light of what it *is*, namely, an instance of a particular form of life. An animal's being the particular sort of living thing that it is cannot be reduced to its capacities and behaviors because those capacities and behaviors are intelligible as the capacities and behaviors they are only in light of what the animal is, an instance of a particular species of animal. A form of life furthermore serves, at least in some respects, as a norm for the individual. In such cases, where the individual deviates from that norm it is correct to say that it is abnormal. Of course, this does not imply that the particular (deviant) case ought in any moral sense to have the characteristics of normal instances, nor even that the features of the normal instances are optimal for survival and reproductive success. They may not be, and in some cases certainly will not be. Abnormal cases can be better fitted for survival than their more normal relatives and can over time become the norm. Nevertheless it is correct to describe them as abnormal if they deviate in relevant ways from what is, at the particular stage in the evolutionary process, the norm.

This distinction between what is normal and what is abnormal is stronger than that between what is typical and what is atypical. The latter is not a normative notion; the former is, albeit a weak one. The amount of rainfall in a given month can be typical or atypical, that is, statistically likely or not. It is not properly speaking normal or abnormal because there is no sense in which the rain is for anything. Rain just does fall provided the physical conditions are thus and so. Biological evolution realizes entities that are not merely typical or atypical but normal or abnormal because with biological evolution new, functional unities are realized. We explain the behavior and workings of the parts of animals by appeal to the notion of a function, and hence that of proper functioning, and so also that of a norm, one that the normal instance of the kind meets.[14] But again, normal does not imply optimal; it may well be the abnormal instance that is fittest, that will, other things being equal, eventually become the norm.

[14] It follows directly that the idea of a single animal independent of any reference to the kind is unintelligible. It is unintelligible because in that case no sense can be made of the notion of what is normal. Is, for example, some given animal blind (in the sense of suffering a defect, not merely in the sense of being sightless)? One cannot answer the question without know what kind of animal it is, whether animals of that kind are normally sighted. If animals of that kind are normally not sighted as this one is not, then though it cannot see, it is not blind (in the sense of suffering a defect). The account is in this way I–We rather than I–Thou; individuals are constitutively instances of kinds, of life forms.

Biological evolution realizes a new sort of being, a living being with its nature or form of life, one that requires different ontological categories than are required in the case of merely physical phenomena in order to be understood. This nature furthermore serves as a norm for the individual relative to which it is normal or abnormal. But as already indicated, the process of evolution by natural selection does not only realize living things. Nature, the world in which animals are to be found, acquires thereby a new sort of significance as well. What are otherwise merely things, for instance, various rock formations, bodies of water, climactic conditions, come to have the significance of what J. J. Gibson (1979, 127) calls affordances for animals, where an affordance is "what [the environment] provides or furnishes, either for good or ill" and "implies the complementarity of the animal and the environment." By contrast with the merely physical properties of things, affordances are intelligible only relative to a species of animal:

> the words *animal* and *environment* make an inseparable pair. Each term implies the other. No animal could exist without an environment surrounding it. Equally, although not so obvious, an environment implies an animal (or at least an organism) to be surrounded. This means that the surface of the earth, millions of years before life developed on it, was not an environment properly speaking. (Gibson 1979, 8)

For an animal, the environment affords both that which is needed for its survival, food, shelter, sexual partners, and so on, and hazards to its survival, dangers of various sorts. Animals and environments co-evolve; they emerge together, and neither is intelligible without the other.[15]

Gibson's notion of an affordance belongs to an account of perception aimed at supplanting the standard conception in terms of stimulus and response, representation and information processing.[16] Instead of conceiving the animal as representing things around it in response to inputs or stimuli to its sense organs, an animal, on Gibson's account, is directly perceptually aware of affordances, biologically significant aspects of its environment. An animal has the capacity to perceive affordances, a capacity that is actualized in the course of its characteristic activities as the animal it is.

The notion of a capacity that is wanted here, like the essentially related notion of life, fell into disrepute with the rise of modern science. We have already noted that as in ancient Greek thought inanimate nature is understood on the model of animate nature, so in early modern thought animate nature is understood on the model of inanimate nature. According to Aristotle, for example, the four stuffs—earth, air, fire,

[15] Lewontin (2000) pursues a different but also important relationship between the organism and the environment. An organism, he argues (2000, 20), "is not specified by its genes, but is a unique outcome of an ontogenetic process that is contingent on the sequence of environments in which it occurs." How an organism develops depends both on its genetic inheritance and on the environment in which it develops. (This would seem to be true not only of biological development but also of social and cultural development.)

[16] Haugeland (1998) usefully explores in some detail differences between the two approaches to perception.

and water—have internal principles of changing and staying the same just as living beings do; as an acorn grows in a characteristic way into an oak in virtue of the sort of being it is, so fire goes up in virtue of the sort of being it is. And both movements have natural ends. In early modern science one argues in reverse: the movements of inanimate things are due to forces that act on them, and similarly, the movements of animate things are due to forces that act on them. Animate things are really just complex mechanisms, like clockworks. But we can and should treat the two cases differently. We should accept with the early moderns that merely physical processes such as stones rolling down hillsides once dislodged from the places they happened to be cannot be adequately understood by appeal to a power of movement that stones have. Although Aristotle would have said (as we might in everyday conversation say) that, for instance, an acid has the power to dissolve something, that dissolving something is something that the acid does and does to something as agent to patient, we need not say this, and should not insofar as we speak carefully. Something merely happens when a solute dissolves in acid; there is a change but there is no agent, and no doing properly speaking. But this does not require us to jettison as the early moderns did the concept of a capacity or power as applied, on the one hand, to living things such as mammals, and on the other, to affordances in their environments. It does not follow from the fact that, say, stones do not have the power of movement that animals do not have such a power, do not do things as agents. Indeed, it is impossible properly to understand a form of life of any complexity without appeal to this notion of a power.

A power contrasts not only with the various dispositions of mere physical objects such as stones and pieces of iron but also, and more tellingly, both with a habit and with what, corresponding to a power, an infinite being might be said to have. First, a power is unlike a habit in having what Rödl (2007, 141) calls a "normative measure": an act of a power unlike an act of habit brings that act under a standard. An animal (of the hunting sort) does not, for instance, have merely the habit of hunting for food, though it may habitually do so in one location rather than another. Instead it has the power of hunting, a capacity to pursue prey in characteristic ways, and on any given occasion is successful or not. The end for the sake of which the animal hunts is to be fed; and this end, what it is for, provides the measure. Being a good hunter is having the power to bring about the relevant end. But of course in some cases the animal, even one that is a very skilled hunter, will fail, perhaps because the prey runs too quickly to be caught. And this is true of powers generally; they can be exercised—as the powers they are and to the ends for which they are powers—only in favorable circumstances. To have a power is not, then, the same as having the possibility of doing something because the possibility of doing something within one's power can depend on the circumstances in which one finds oneself. Riding a bicycle is within my power but it is nonetheless possible for me to do that only if my circumstances afford me a bicycle to ride and the requisite conditions for riding.

The fallibility of a power, the fact that it can be thwarted when the circumstances are not propitious, is, as Rödl (2007, 153) emphasizes, not a limitation of the power but instead a "logical or metaphysical fact." Powers are by their natures as powers inherently fallible; the successful exercise of a power requires propitious circumstances. An infinite or eternal being does not have capacities or powers for precisely this reason. Such a being engages in activity (we can suppose) but without thereby exercising a capacity. The act of such a being is "pure act; the contrast of power and act does not apply to it" (Rödl 2007, 153; see also Beere 2009, 296). Only finite beings can have powers.

The powers that animals have as the animals they are, are inherently fallible; they can be exercised, achieve their ends, only in favorable circumstances. The capacity for nourishment is such a power, as is the ability of birds, say, to build nests. That is, both biological processes such as digesting food for nourishment and an animal's doings, say, a bird building a nest, are for some end and because they are they can be thwarted by unpropitious circumstances. The "food" is poisoned; the bird is caught in a ferocious storm. Such doings and processes can also be interrupted. That is, the animal can be digesting food and the bird building a nest even in the case in which the food is never digested and the nest never built. Mere physical processes are never like this. Because they have no particular end, no *telos*, they cannot be interrupted before reaching their end. They just happen as they happen, one thing after another after another. A stone, for example, rolling down a hill is not interrupted when it is stopped by a felled tree; it cannot be interrupted because it is not aiming to get anywhere in particular. It is simply stopped.

Powers are furthermore intrinsic to the animals that have them insofar as for an animal to gain or lose a power is for that animal to change. An animal that is blinded, for example, loses the power of sight. But not all animal powers are merely biological endowments (as it is natural to think sight is). Some are learned, and others are improved by learning. An ape, for instance, can learn a new way of getting food—say, how to crack a nut with a rock—in an environment that affords such learning, that includes rocks of the right size for the job that are near enough to the nuts to facilitate the discovery. An animal's communicative powers similarly can be either innate or learned. Wolves, for example, instinctively growl and bear their teeth as if about to bite when they are angry, and other wolves instinctively withdraw. Chimpanzees also have such instinctual vocalizations and gestures expressing feelings and emotion, but they also can acquire certain powers of communication. Tomasello (2008, 25) describes how very young chimpanzees learn the "touch-back" gesture.

> The infant initially grabs the mother's back and pulls it down physically so as to climb on. Mom comes to anticipate on the basis of the first touch, and so lowers her back when just this initial part of the sequence is produced. The infant learns to anticipate this response, and so comes to use the gesture intentionally, touching mom's back lightly and waiting for her to lower it in response as expected.

Of course, the infant does not do this intentionally in the same sense that a rational animal might do something intentionally; the infant chimpanzee could not, for instance, deliberate and then decide on the basis of its deliberations to make the gesture. The point is that now what the infant is *doing* is gesturing by touching its mother's back as contrasted with taking a preliminary step in pulling her back down. What had before been an early stage of doing something else (pulling the mother's back down in order to climb on her back) is now the doing itself. The infant has acquired the power to communicate to its mother its desire to climb on her back, which of course equally well involves its mother acquiring the power to respond appropriately to such a communication.

Mere animals can be very intelligent, resourceful, and creative in getting what they want, and can learn to use things as tools and to communicate their desires as well as to respond appropriately to the gestures of others. They are clearly conscious, though not self-conscious. They can sleep and can awake, and when they are awake they are aware of and respond to things in their environment that matter to them. What mere animals do not have is any awareness of themselves as conscious in all these ways. Our own body language provides a model for thinking about this. We now know that we learn over the course of our upbringing to engage in and respond to various sorts of body language, how to sit and stand, how and when to touch or look at another, and that all this is quite culturally specific. But there was a time when we knew none of this about body language, or even that such a thing exists, though we were doing it. That is, we were engaging in subtle and complex behaviors in relation to others, learned behaviors that involve quite acute sensory-motor skills and also learned emotional responses, clearly conscious and intelligent behaviors, all without the least idea that we were doing it. The whole constellation of affective, sensory, motor, and intellectual factors involved in our body language was in our possession as part of our power to communicate with our gestures, postures, and facial expressions. And someone who knew about such powers would be able to see, that is, to know, that we had this power. But there was a time when we did not know about such things and could not realize that we had such a power. Thus, in one sense, even before we knew about body language, we could see another's body language, obviously so given that we could and would respond appropriately to the body language of another with body language of our own, but in another sense, we could not see this because our seeing though conscious was not self-conscious. And there are presumably aspects of our lives that are like this even now; though we, unlike other animals, are self-conscious, we nonetheless can possess merely animal powers as well, powers that we exercise skillfully and aptly without having the least idea that we do.

A non-rational animal is neither a mere mechanism to be understood in merely physical terms nor a self-conscious, critically reflective agent responsive to reasons as the reasons they are. A non-rational animal is an instance of a life form, one with characteristic sensory and motor powers that lives out its life in an affordance-rich environment that is made for it in the sense of co-arising with it through its having evolved, come into being, in that sort of place. It is this understanding of an animal in its environment, with its various powers and abilities as the form of life it is, that provides the basis for what follows. What we need now to understand is how certain merely biological animals, together with their environments, can themselves be transformed through a process of social, historical evolution that culminates in a new sort of being, not yet a rational animal but the precursor of one.

1.3 The Emergence of Human Culture and Social Significances

Evolution by natural selection realizes a new order of intelligibility in nature (affordances), a new mode of being (animate), and new characteristic behaviors that are not reducible to the behaviors exhibited by mere physical stuffs (exercises of powers). Animals can be intelligent, resourceful, even playful. And primates in particular can also have complex social lives grounded in kinship, friendship, and dominance rank. We are such animals. But we are peculiar among such animals insofar as we are rational, insofar as we speak and know ourselves to speak. What is the nature of this capacity? One familiar answer, indeed the only possible sort of answer from the sideways-on perspective, is that we have evolved to have a language gene or an innate universal grammar, that we are programmed, built, to use language. The alternative to be pursued here is to see language and thought as distinctively social, as powers we acquire in the course of our acculturation into a sufficiently and appropriately complex culture.[17]

Human culture is extraordinarily rich and multifaceted, and it came to be so, Tomasello argues, because we as a species are uniquely capable of imitative learning, which enables in turn what he calls a "ratchet effect." Because we imitate conspecifics, "individual and group inventions are mastered relatively faithfully by conspecifics, including youngsters, which enables them to remain in their new and improved form within the group until something better comes along" (Tomasello 1999, 512).[18] We know, for example, that *Homo erectus* had stone tool technology and that that technology remained unchanged for over a million years; even if, now and again over this period, individuals hit upon improvements, as is likely, those improvements were not culturally preserved. Then, about one hundred thousand years ago, tools began to become increasingly complex; that is, innovations were preserved in ways that enabled further innovations on those innovations, giving rise

[17] The approach taken here thus belongs with recent work that rejects the autonomy of syntax, that is, the idea that syntax can be treated in separation from semantics. See Tomasello (1998) and Langacker (2006). As the point is sometimes put, natural language unlike mathematics simply does not support a principled distinction between syntax and semantics. There is something to this, but we will see that even in mathematics the distinction between syntax and semantics, or form and content, is not as sharp as generally supposed.

[18] John Haugeland has independently suggested something similar, that in a community of conformist creatures, creatures that not only imitate but censure variation, norms governing the behavior of members of the community can coalesce. See Haugeland (1998, 310–13).

to distinctive, and elaborate, cultural practices (Alvard 2003, 141). This is the ratchet effect, and it is made possible by the capacity or power of imitative learning.

Even non-human primates can learn from others. Chimpanzees, in particular, as Tomasello (1999, 520) reports, "are good at learning about the dynamic affordances of objects they see being manipulated by others.... For example, if a mother rolls over a log and eats the insects underneath, her child will likely follow suit." But as he immediately goes on to argue, "this is simply because the child learned from the mother's act that there are insects under the log." What even chimpanzees do not do is to copy the methods of others; "as far as the animal is concerned there exists only the bait and the obstacles standing in his way" (Vygotski 1978, 37). Human children by contrast are concerned not merely with the outcome, the bait, but also with the means that are used to achieve it. They do copy methods. They (we) live lives shaped not only by biological imperatives, the need for food, shelter, and so on, but also by the (innate or genetically determined) imperative to do not only what but also as "we" do.

Humans are also distinctive, Tomasello (2008) argues, in being helpful to others and able to cooperate with them; we but not chimpanzees can do things not merely side-by-side but *together*, and in this regard are uniquely adapted for cultural life. For example, chimpanzees do not naturally point to things but learn to do so when they grow up around humans. They learn, that is, to indicate to humans what they want by pointing because they have learned that humans, unlike other primates, will help them achieve their goals. Significantly, they seem unable to comprehend a helpful gesture, for example, someone's pointing to where there is food for them (Tomasello 2008, sec. 2.3.2). Beginning around the end of the first year, human children, like chimpanzees in captivity, begin to point imperatively to something they want or want done. But at the same age they also begin to point informatively, that is, helpfully to offer information, and can understand such a gesture made by another. And at the same time they begin to point as an expression of an emotion or attitude to be shared (Tomasello 2008, sec. 4.1.2). Human beings thus seem to be not only capable of imitative learning but also highly motivated to cooperate with and be like others. If Tomasello is right, we, unlike other primates, are motivated not only to do what and how "we" do but to have the emotions and values that "we" have. As a result, by way of the ratchet effect, we develop distinctive and highly complex social practices, that is, a new sort of form of life, one that is structured first and foremost by social rather than biological imperatives, and by biological imperatives as they come to be socially articulated.[19]

With the emergence of social practices through imitative learning and the ratchet effect, certain items of behavior acquire the significance of being performances of

[19] The fact that our acculturation fundamentally shapes myriad aspects of our lives, not only how we take things to be but even our powers of perception and cognition, is becoming increasingly well known in the social sciences. See, for example, Henrich, Heine, and Norenzayan (2010).

practices. They are significant as normal, or abnormal, performances relative to the practices of the group, and as is the case with biological organisms, such performances are at once constituted by and constitutive of what counts as normal behavior. These practices also confer significance on entities that do not evolve as practices do but are in one way or another caught up in those practices: the food stuffs that, according to what is normal for the community, are to be prepared and eaten in certain ways, the materials that are used for various purposes, the natural phenomena that are responded to with particular sorts of behavior, and so on. As the surroundings of a particular species of animal come to have the shape of a habitat for that species, so the environment of practitioners of conformist practices takes on a particular shape relative to those practices. All this we might expect in light of what we have learned about the distinctive motivational structure and capacities of human animals. But we can also, in a more speculative vein, divide these practices into two sorts, into practices that are the producings and usings of things, food and shelter, for example, as well as the tools and materials that are involved in their production and use, on the one hand, and practices that are themselves the items to be produced and used, on the other. It is the latter sort of practices that we will be concerned with here because they alone can come to be instituted as a kind of game that "we" play.

Among culturally evolved practices, we can imagine some that are verbal doings that in fact make a difference only to the behavior of others in the community, though we should of course expect that these verbal doings will concern things that matter to us, things such as food, shelter, safety, sexual partners, and so on. Because such practices have in this way a purely social significance, because they do not make any material difference in things but only make a difference to the behaviors of others in the community, they can come to constitute a kind of game that one enters by, say, verbally responding to something with extra-game significance, perhaps some event or thing in the environment, that is exited by a performance that is not a verbal response but is made in response to a verbal performance, and that has also intra-game moves that are verbal performances in response to verbal performances.[20] Sayings that are entries effect what we can think of as a kind of translation of things into tokenings that entitle other performances; the thing to which one responds gets caught up in the game by being responded to verbally. And as we should by now expect, this is not a matter of making a normative response to something non-normative any more than an animal's eating something that is food for it is to make a biologically significant response to something that is not biologically significant. Instead, as animals and foodstuffs co-evolve, so verbal practices and things as to be so called co-evolve. In this way, various features and aspects of the environment

[20] To call this a game is not to contrast it with things that matter to us as the sorts of animals we are, but to distinguish it from other sorts of socially evolved practices that we engage in. What it is to be a game in the sense of concern here will soon be clarified.

come themselves to have normative significances; they acquire significance in the game. With the emergence of a game of the right shape, a full moon, say, can come to constitute an opportunity for a particular utterance or verbal tokening. And such verbal tokenings in turn entitle players, both to other sayings and to doings that are not sayings. An action or game exit effects the converse translation, the translation of a game token or utterance into a non-verbal performance. One hears a certain call and straightaway one acts, one's action entitled by the cry one hears. Utterances are, then, entitling, and responses to utterances entitled.

As just indicated, games involve more than evolved behavior, what is normal or abnormal for some life form. They involve also the notion of entitlement. In a game one does not merely do as we do; in a game, one is in addition entitled (or not) to perform certain actions at certain points. Entitlement is furthermore a purely social status: to be entitled is nothing more or less that to be taken to be entitled by those with whom one plays the game. Once our animals begin to distinguish in practice between those who "count" as players and those who do not, an equilibrium can be reached in which one is treated as a player by all and only those whom one treats as players. At equilibrium, a space emerges as the space of the game. A simple economic example illustrates the basic idea.

In a barter or pre-market economy, a transaction is merely a function of what two traders settle on in a particular trade. There is no money and nothing that commodities can be said to be worth; the terms of each trade must be worked out independent of any others. But if some one commodity comes to be something that everyone is willing to accept and to offer in trade, competition becomes possible. That is, with the emergence of some one commodity as, in effect, a kind of money, the terms of any trade are easily replicated and also differentially fit because each trader acts so as to make the most advantageous deal. And with competition and the differential fitness of various trades, prices emerge and a stable market is created. This market does not yet have the status of a game, however. Because the money of this market is itself a commodity, something of value to traders independent of the market, each trader agrees to a trade solely on the basis of the perceived relative worth of the two commodities involved. Although things now have the social significance of being worth so much—they have a price—nothing so far is a pure social significance. A commodity money market is, ontologically speaking, nothing more than an evolved set of practices.

In a commodity money market some one commodity plays *de facto* the role of the money of the market. In a fiat money economy, something of essentially no value independent of the market, for instance, a piece of paper on which is inscribed so many units of market value, is used instead. Obviously, then, one could not begin with fiat money. Because it has no value independent of the market, fiat money can be introduced only after the market has emerged and prices have stabilized through the use of commodity money. But once prices *have* stabilized, anything at all can come to play the role of money; once prices have stabilized, the market can be instituted as a kind of a game in which certain tokens play the role of money, serving, in effect, to keep score of one's purchasing power in the market game. If traders start to accept and to offer certain otherwise useless tokens in trade, then at equilibrium, when everyone whom one treats in practice as having "good money"—that is, legitimate tokens—so treats oneself, such tokens are instituted as the money of the market. The tokens have come to have a purely social significance; they are valuable solely because they are treated as valuable by market traders. They serve to keep track of one's status in the market game, one's purchasing power, one's entitlement to goods.

In a general way, games differ from other practices in virtue of involving both the notion of a purely social significance and the related notion of what one is permitted or entitled to do as that notion contrasts with that of what it is normal, or abnormal, to do. A player in a game is not only someone capable of making the relevant moves, the moves "we" make in appropriate circumstances, someone with the power to respond appropriately; a player is someone who is *entitled* to make moves, and who is properly held responsible for playing by the rules. Being a player is having a certain socially instituted status. We have already seen this in the market case. Commodity money—because it is a commodity, something that is valuable to individuals independent of the market—has a kind of natural or normal significance; it need not be thought of as entitling in any significant sense. Fiat money, because it is not a commodity, has only social significance; it is intrinsically entitling, and this entitlement is a purely social status. To have fiat money is to have a certain socially instituted status as a player in the market game; it is to have the power to buy goods.

To enter the market game one sells one's goods for fiat money, acquiring thereby the potential, or entitlement, to buy in the market. To enter a verbal game by responding to something non-verbal with an utterance is similarly to acquire a potential, to become entitled in a certain way. One's performance is normal relative to the relevant practice—for instance, one utters (say) 'food' in response to edible things, and not in response to what is inedible—but it is not entitled by that to which it is a response. The entitlement to make moves in a game is a purely social entity; it cannot be conferred by a non-verbal event or state of affairs. Verbal responses to non-verbal things, although they are entitling of other performances, both intra-game moves and exits, are not themselves entitled. Correspondingly, a game exit, which is a non-verbal performance in response to an utterance (analogous to a purchase of some good with fiat money), is entitled without being entitling. Of course, what one does by way of a game exit can occasion a verbal response. Nevertheless, the two are essentially different. They are different much as a purchase and a sale are different in a fiat money economy (though not in a commodity money economy). In the verbal game, entries are entitling of moves but not entitled by moves; exits or actions are entitled by moves but not entitling; and intra-game moves, that is, utterances in response to utterances, are at once entitling and entitled.

Suppose, now, that among the practices constitutive of the verbal game there comes to be one to the effect that only in certain circumstances are correctly made utterances to count as entitling. An utterance of 'this is ripe' when made in response to an object seen in the full light of day, for example, is treated as entitling of other moves, but that same response, even to the same object, made at twilight is not. Players, in such a case, learn to discriminate not only objects and events but also the circumstances in which they are discriminated; they learn to treat some discriminations of objects and states of affairs as entitling and others as not. Those that are treated as entitling are made in what we can think of as standard conditions. If, according to the game, the circumstances count as non-standard then an utterance made in them ought not to be treated by the players as an entitling move. An utterance made in such circumstances is, in effect, counterfeit, as, of course, fiat money, though not commodity money, can be.

This distinction between standard and non-standard conditions is and must be instituted by the players themselves. We say that standard conditions are the conditions in which things are as they appear to be; in standard conditions things show up as they are, in their true colors—or so we say. But no more than the players of the language game envisaged here do we have access to things independent of how they show up for us. Therefore, we have no basis for the claim that things are in one sort of circumstance shown in their true light, while in another not, no basis for the claim that the conditions deemed standard are those in which things are as they seem. To say that standard conditions are those in which things are as they appear to be is only to say that they are the conditions in which utterances are (taken or treated as) entitling of other sayings and of doings. Non-standard conditions, similarly, are just those in which an utterance, while behaviorally correct insofar as one says of the object that appears, say, ripe that it is ripe, are not entitling of other moves (say, picking and eating it). The notion of objective truth as it contrasts with the notion of what is assertible or correct according to the practices of the linguistic community, has and can have no place in the linguistic game so far described.

A verbal game of the sort we have been imagining could, by way of the ratchet effect, come to look very much like a language proper: its game entries like assertions, its exits like intentional actions, the conclusions of chains of practical reasoning, and its game-game moves like inferences. For instance, it could become appropriate in the game to question another's entitlement to a move, and to respond by producing the utterance on the basis of which it was made (or by pointing to that to which it was a response). A distinction between moves one is obliged to make and moves one is permitted to make could also be instituted, and we can imagine that failing to make obliged moves, like failing to pay one's taxes, would result in one's losing one's status as a player. Indeed, even a notion of 'looks' as a kind of flag to indicate that one's move has not the entitling force of a move—say, because conditions are perceptibly non-standard—could be introduced.[21] But for all such embellishments, the game is

[21] See the discussion of the logic of 'looks' in Sellars (1956, Part III).

only a game. Entitlement is thus far a merely social status; one is entitled to a move or makes an entitling move just if one is so taken by those with whom one plays the game. We have in place a notion of assertibility, of the acceptability of an entitling move, but not yet the notion of objective truth.

The idea that judgments are fundamentally like moves in a game is a familiar one. But it is not fully adequate because games, and the legitimacy of moves in them, are merely socially instituted. The correctness of a judgment is not socially instituted; it is instead objective. *Judgment* is answerable to how things *are*, not merely to how in the game they are to be taken to be, and because it is, judgment cannot be understood as a move in a socially instituted game. Nevertheless, much as merely biological animals provide the basis for the emergence of players of games, so players of the sort of verbal game we have described, with its entries, exits, and intra-game moves, provide the basis for the emergence of rational animals. Players of games, we will see, can be transformed into rational animals through the coalescence of loci of authority over commitments and entitlements that realize game exits and entries as, respectively, active and passive, as the actions and perceptions of a properly rational being.

1.4 The World in View

An important difference between the game-players we have been envisaging and ourselves is that *we* do not merely talk and do as we were brought up to do as part of an elaborate social game (although we do that too). We do not merely say what it is socially acceptable to say, and we do not do only what is expected of us. At a certain stage in our maturation we begin to question whether what it is socially acceptable to say is true, whether we should do something simply because it is expected of us. *We* do not merely speak and act. We are aware of ourselves as speaking and acting, and because we are we can question the wisdom of what we find ourselves inclined, by virtue of our acculturation, to do and to say. We are distinctive, then, in drawing a distinction in principle between what we find ourselves prone or inclined to say (or do), on the one hand, and what we perhaps should say (do), on the other. And we acquire thereby the capacity for second thoughts. The task now is to understand, if only in broadest outline, what is involved in doing this, both how it is that players of the sort of verbal game we have described might come to have the capacity for second thoughts, and more importantly, how we should understand this capacity itself. Again, an analogy will prepare the way.

It is a familiar fact that an animal can learn successfully and intelligently to navigate some portion of the landscape, its neighborhood, say. What the animal learns is various routes through the landscape guided by landmarks, and such knowledge is procedural. It is knowledge of how to go on at various points to get to somewhere in particular. What non-human animals seem not to acquire in the course of learning to navigate the terrain is a cognitive map of the whole, and thereby the capacity to navigate between two landmarks not previously traversed.[22] We do precisely that. Like other animals, we come to know our way about in a new terrain by exploring it, taking various routes through it from one landmark to another. But we also achieve something more, a cognitive map of the whole that is not merely the sum of its parts. The various routes from landmark to landmark that we have learned are synthesized into a single, unified whole of all the landmarks in their relative locations. What I want to suggest is that essentially the same process can be envisaged for the case of one's acquisition of natural language.

We have seen that to be acculturated into a verbal practice is to learn to respond to various things, verbal and non-verbal, by making various noises. One comes to say what "we" say, that is, to be an instance of a socially evolved form of life. In so doing, one learns one's way around what we can think of as the cultural terrain. One learns verbal "routes" that involve both landmarks, that is, proto-referential relations (one learns what things are called), and paths from one landmark to another, proto-inferential relations (and implicitly thereby what is a reason for what).[23] But in learning to play this socially instituted game that just is what "we" do and say, one need not synthesize into a single integrated whole the various paths that one is learning through the social terrain. At first one has, as an animal does for the case of navigation, only procedural knowledge, the capacity to make the moves, the utterances, "we" make in a given circumstance, but without synthesizing that knowledge into a cognitive map of the whole.

To have procedural knowledge of what we are calling the cultural terrain clearly involves being aware of things. What it does not involve is any awareness *that* one is aware. One is in such a case aware much as an animal, a dog, say, might be said to be aware of where it is in a familiar terrain without yet knowing—that is, being aware that it is aware of—where it is. In the case of a human navigator, whether of the geographical or of the cultural terrain, we now suppose the synthesis of all one's various routes into one, into a kind of a map of the whole. To have such a map of the whole, while of course always remaining somewhere in particular, is, I will suggest, to have a *view* properly speaking of things, the capacity to take in things as they are, and it is a view of things *because* to have such a map just is to have the capacity for second thoughts and thereby the capacity for any thoughts (properly speaking) at all.[24]

The capacity for second thoughts is the capacity to step back from what one finds oneself inclined to say and to do, the capacity to distinguish in principle between how

[22] See Sterelny (2003, sec. 3.3).

[23] Such knowledge is, at least at first, not propositional knowledge-that but instead know-how. As Ryle (1950, 333) argues, "the activities of asserting and following both hypothetical statements and explanations are more sophisticated than the activities of wielding and following arguments. A person must learn to use arguments before he can learn to use hypothetical statements and explanations." One must be able to make inferences before one can learn to say what the rules are that govern the inferences one makes.

[24] As will become clear, although this is a view of the world, it is not a fully objective one. It is only our first, essentially sensory, view of the world.

things seem to one and how things are.[25] This capacity, I am suggesting, is enabled by the synthesis into a single integrated whole of all the cultural "routes" that are learned in the course of one's acculturation into the verbal game. Although we acquire the capacity to make moves in the verbal game piecemeal, and at first have only procedural knowledge of the cultural terrain, eventually all the routes we know are synthesized into a single unified whole, into a kind of cultural map of the world as "we" know it. And having so synthesized these routes, we can distinguish, in principle, between how things seem to us perceptually, from where we are, and (by way of inference) what is.

To be able to perceive and judge that something is so requires the capacity to recognize that one may be mistaken, that things may not be as they seem, and for that one needs a view of the whole. For it is only if one has a view of the whole that one can infer that things are not as they appear to one here and now. McDowell (1994, 30) makes this point for the case of color perception when he claims that

> no subject could be recognized as having experiences of colour except against a background understanding that makes it possible for judgements endorsing such experiences to fit into her view of the world. She must be equipped with such things as the concept of visible surfaces of objects, and the concept of suitable conditions for telling what something's colour is by looking at it.

Having the whole in view through the synthesis of the various paths through the cultural terrain, one knows not only how things show up to one here and now but also, inferentially, how things actually are. One is able in this way to distinguish in principle between what one sees or seems to see, how things show up to one, and what one should say, what is so. A person is correctly described as having experiences of color, as taking in the color of something, then, only if she has a view of the whole and so recognizes that things can appear differently than they are.[26] And she has such a view in virtue of having synthesized into one integrated whole the various cultural routes through the language that are acquired in the course of one's acculturation.

In the verbal game considered above, language entries are moves that are entitling. A game entry effects a translation of something with extra-game significance into a game-significant utterance. An edible object, for example, is translated into the game-significant utterance 'this is food'. With the transformation effected by the synthesis of all the routes through the cultural terrain into an integrated whole, we need to think of language entries not as moves that one makes but instead as prima facie

[25] It is also what underlies the power of reason as Aristotle characterizes it, a two-way capacity for opposites. Having the capacity to step back is to be able to ask not only whether I should affirm or deny but also whether I should say what I think or instead should lie. In the limit, it is the capacity to ask how I should live my life.

[26] Once again the example of color is not really the best sort of example. Better is something like food: this looks good to eat but it is not because it comes from such and such sort of tree and the fruit of that tree is inedible.

entitlements that one simply finds oneself with. Instead of making an utterance in response to something in the environment, one merely finds it to be so. In this way, the content of a claim is separated, in principle, from the act of claiming. One makes oneself available to the world—one opens one's eyes, runs one's hand over a surface, takes a taste of something—but the entry that is made into one's cognitive account is an entry that, from one's own perspective, just happens. Things just do show up for one as thus and so. Perceiving, properly speaking, can thus be thought of as a translation of an actual state of affairs into a pure potency. From being an entitling move, something one does (in the socially instituted verbal game), perceiving becomes a pure entitling; perception in this way brings to mind a mind-independent entity, and as such entitles one (at least prima facie) to a claim. Perception is, then, wholly passive; because it is entitling, it must be authorized from without. In perception one's "score" is changed by a change in what one is entitled to assert, and this change is not one that the perceiver, or indeed anyone else in the linguistic community, has the power to bring about. It is things in the world that cause one to perceive as one does and thereby to entitle one to claims.[27]

Perception, on this account, effects a translation of an actual state of affairs into a pure potentiality; it brings to mind something that (ostensibly) is the case. Action, conversely, can be thought of as the successor to game exits, as effecting a translation of a potentiality into actuality; in action, one realizes, or at least tries to realize, something as what one intended. Game exits, we saw, are entitled but not entitling responses to sayings. Actions, which are the successor to such game exits, are an expression of human agency. They are authorized by the agent and performed as the realization of one's cognitive potential. One is not merely entitled, or even required, to act (on the basis of some game move), as was the case in the context of the socially instituted game; one is now instead properly committed to acting as one does. Action is an expression of the agency of the subject, and a particular action is the realization of a particular commitment one has undertaken. Thus, although the notion of entitlement is sufficient to distinguish between game entries, game exits, and intra-game moves—they are, respectively, entitling, entitled, and both entitling and entitled—to understand the difference between perception and action in the properly intentional case that is of concern to us now requires two notions, that of a commitment as well as that of an entitlement.[28]

In an intentional context, two notions, that of a commitment and that of an entitlement, replace the notion of (purely social) entitlement that is in play in the

[27] That perception is wholly passive at the intentional level is fully compatible with its involving a great deal of non-intentional activity "lower down." As J. J. Gibson emphasizes, perception involves movement. It also obviously involves a great deal of sub-personal processing. Nonetheless, it is cognitively passive.

[28] Although it will not be explored here, this difference has an analogue in the case of the market. In a fiat economy there is only (entitling) money, which realizes the distinction between buying and selling insofar as in the one case one offers money and in the other receives it. In a pure credit economy, to which a fiat money economy can give rise, one needs, in place of the notion of money, two notions, both that of (entitling) credit and that of a debit as authorized by oneself alone. See my (1994).

context of a game. To think of commitments and entitlements as distinguished merely by their direction of fit, as entitled and entitling respectively, is a mistake; commitments and entitlements are distinct, though related, potentialities. Commitments are the expression of one's agency; they are intentions that have the form of rules one has freely undertaken to follow. Entitlements, by contrast, are impressed upon one by how things are; they are actualizations of one's receptivity and have the form of facts (more exactly, of things as what and how they are). Acting and perceiving, then, differ along two different dimensions: with respect to direction of fit, insofar as in acting one realizes one's intention in a state of affairs and in perception some thing is brought to mind as what and how it is, and with respect to the sort of authority that is involved, that of a commitment, which has the form of a rule that one undertakes to follow, and that of an entitlement, which has the form of a fact, something as something, with respect to which one is wholly passive.

Perception is an act—that is, an actualization rather than a motion—with respect to which one is entirely passive; one simply finds things to be thus and so in one's perceptual experience. Perception is also world to mind in the sense that perception effects what I have been describing as a translation of a mind-independent state of affairs into a cognitively significant perceiving; in perceiving one *takes in* how things (ostensibly) are. Such perceivings are prima facie entitling. Actions, by contrast, have the character of exits from the cognitive realm on this model. They are the expression of one's agency in realizing one's intentions in actual states of affairs. Because, on this account, judging is the successor of intra-game moves, which are like exits in being entitled and like entries in being entitling, one would expect that judgment also involves in some way aspects of both perception and action.

To judge is in some way to make a commitment, regarding how things are in assertion and how they ought to be in intention. In this respect, judgment is like action; it is something one does and for which one is responsible. But whereas action is responsible to the intention it aims to realize and is successful just if it realizes the state of affairs intended—it is in this regard mind to world—a judgment is instead responsible not to an intention but to how things actually are, to what is true in assertion and what is good in intention. An act of judgment aims to get things *right*; and insofar as it does, it is not merely a commitment but something more like an *acknowledgement*, at least in intention. In judging, one aims to acknowledge the truth of what is true (in judging what is the case), and the goodness of what is good (in judging what one will do). As an act for which one is responsible, then, judgment is a rule-following and an act of freedom (better: spontaneity); but because it is successful only if it gets things right, only if the content judged is true in assertion and good in intention, it is, like perception, world to mind in fit. As we will soon see in more detail, successful judgment reveals things as they are (in assertion) or ought to be (in intention); it makes manifest the otherwise latent nature and structure of the world.

Imagine the following little game. To play this game one has a sheet of paper divided into two columns. On the right, entries just appear, much as one just does find things to be thus and so in perception. Entries on the left are one's own responsibility and are constrained only by the inferential rules of the language as they apply to the entries on the right. For example, seeing something as red, finding an entry to that effect in the right-hand column, gives one a prima facie entitlement to enter a statement of one's own to that effect, that is, to commit oneself to the truth of the claim.[29] Other entries can override this entitlement. Perhaps one has already committed oneself (by making an entry in the left-hand column) to the circumstances being non-standard, to the circumstances being those in which white things appear red. In such a case, the rules prohibit an entry to the effect that the object is red. The point of the game is to make entries on the left that are entitled by those on the right consistent with the rules.[30]

This little game, unlike the socially instituted verbal game described above, is not socially instituted, though socially instituted games obviously underlie its constitution. In this game entries appear on the right independent both of the player and of everyone else; they are just there, given as fodder for one's moves. Entries that the player makes on the left, by contrast, are that player's responsibility; the only constraint on them is provided by the rules of the game. So, for example, the distinction between veridical and illusory perception is not now grounded in a socially instituted distinction between standard and non-standard conditions, but instead in the rules governing what may legitimately be inferred from what. Like players of more familiar sorts of solitaire, the player of our little game has, at least in this regard, only herself to answer to.

But what is it to have only oneself to answer to? It is nothing more and nothing less than to have the power of reason, the power to know how things are. The rules by which one plays are neither brutely alien impositions (as if we were somehow programmed so to act, say, by our upbringing) nor freely chosen (they could not be given that they are constitutive of rational freedom). Yet they are one's own as the rational being one is. Much as an animal's life is its own as the sort of animal or life form that it is, so the rules of thought that one finds oneself with in light of one's acculturation into a human form of life are one's own as the rational, critically reflective animal one is. We need, in other words, to distinguish (following Rödl

[29] Although judgments correspond to intra-game moves, although they are what intra-game moves become with the coalescence of loci of authority that is achieved when we acquire a view of the world, it does not follow that our perceptual judgments are inferences from premises (judgments) about how things appear to us. Being prima facie entitled is not to have premises to which a rule of inference might be applied. Having the power of perception, which is a power of knowing, is to be able to see how things are. Such a power, like any power, is fallible; it can fail if the circumstances are unpropitious. And if one knows that they are unpropitious then one knows that the entitlement fails.

[30] Of course in our actual lives we always already find ourselves with an extraordinary amount of knowledge about the world around us. There is no point at which the "game" begins; as far as our self-conscious lives are concerned, we are always already in the midst of it. That just is what having the world in view is, and must be. It is our capacity for critical reflection, second thoughts, that is constitutive of our rationality, not some fantasy of finding indubitable foundations on which to "build the edifice of knowledge."

2007) three different ways a thing can be in relation to a rule (or law). First is the way of what Rödl calls, following Kant, heteronomy: "a law of heteronomy is one according to which one thing is determined to act by another thing; that is, a law of heteronomy bears the following form: 'An *N* does *A*, if an *M* does *B* to it'" (Rödl 2007, 118). We suppose in this case that *M*'s doing *B* to *N* is indifferent to *N* in the way that a chunk of iron is indifferent to the place in which it is located. Merely physical causal processes take this form: iron rusts if moisture gets on it. Animals' responses to affordances in their environments are, we have seen, different insofar as it can belong to an animal form of life not only that it does things that are immediately expressive of its nature but also that it has done to it things that are natural for such a life form. "Laws of a life-form place its instances in circumstances that solicit the dispositions and powers characteristic of the life-form: pine trees grow in sandy ground; chimpanzees eat fruit. We need not go beyond the laws of a life-form in order to account for the conditions of actualization of its characteristic powers" (Rödl 2007, 119). In this case, by contrast with that of rusting iron, the act that is solicited and the circumstances that solicit it are fully explained by the form of life of the agent. That pine trees grow in sandy soil is not merely something that is caused to happen to pine trees; it belongs to that form of life, to what it is to be a pine tree. The law is in this case a law of autonomy insofar as it is not an imposition but constitutive of the life form. What is distinctive, finally, of the law of autonomy of a rational being is that it includes no conditions of the actualization of its power (such as sandy soil or fruits). Such a law "explains acts that exemplify it by the nature of the subject of this act and by it alone" (Rödl 2007, 119). The rules by which one plays are one's own as the rational animal one is.

Perhaps it will be objected that even so there is no reason for saying that the animal I have described is rational, answerable to the norm of truth, because such an animal has and can have no grounds for claiming that anything really is as it appears to her to be, no basis on which to claim that her considered judgments, those that appear in the left-hand column, are revelatory of things as they are. She has, at best, a coherent view of things as they show up for her. This, we need to see, is not right.

On the account just sketched, the agency of the subject and the objectivity of the world are to be understood in terms of the notion of a locus of authority. To say that a subject is an agent is to recognize her as having authority over, responsibility for, what she thinks and does. To say that the world is objective is to recognize it as the locus of all entitlements, as that to which one is responsible in all one's judgments. What is not yet clear is why we are entitled to think that the notion of objective truth, rather than some weaker notion, has any role to play in our account. By what right do we claim that the world, as the object of our empirical investigations, is anything more than, in Rorty's (1982, 15) words, "the purely vacuous notion of the ineffable cause of sense and goal of intellect, or else a name for the objects that inquiry at the moment is leaving alone"? Why, given the contingencies of our biology and cultural histories, should we think that things as we find them in our perceptual experience have anything at all to do with the way things actually are? There is both a short answer and a long one. The long answer is the account to be developed in the remaining chapters of this work. The short one is as follows.

I have suggested that it is in virtue of our being the particular sort of being we are, namely, rational animals capable of second thoughts, that it is right to say that we experience things as they are, at least in cases of veridical perception, that, as McDowell (1994) argues, to acquire one's first language is thereby to acquire a view of the world. There is more to be said given that our view of the world is at first shot through with the contingencies of our biology and socio-cultural history. And more will be said. Nonetheless, this is the first thing to be said: we are rational beings, where to be rational is to have the power of knowing, a power that in the first instance is manifest in our knowing about the world as it first comes into view.

Following Gadamer, McDowell contrasts our view of the world with the perceptual sensitivity of a mere animal to its surroundings. For a mere animal, that is, for a creature whose perceptual sensitivity "is in the service of a mode of life that is structured exclusively by immediate biological imperatives," its surroundings "can be no more than a succession of problems and opportunities, constituted as such by those biological imperatives"(McDowell 1994, 115). We begin our lives as such animals. But unlike other animals, a human offspring has the potential to become something quite different from a mere animal, namely, a rational animal with a view of the world. Our nature is second nature. Now according to Aristotle, from whom McDowell borrows the idea of a second nature, we have, as the merely natural animals we are born, already a view of the world.[31] According to Aristotle, our capacity to perceive things as they are is a capacity we acquire in the course of normal embryonic development, and in a corresponding way, a perceptible object, a wind chime, say, acquires the capacity to be heard by the wind blowing through it, when it is chiming. But although both the capacity to perceive of the newborn child and the capacity to be heard of the sounding wind chime are in this way actualizations of a potential, in another way they are merely potentials insofar as the newborn child may have the capacity to perceive without perceiving anything and the sounding chime may have the capacity to be heard without being heard. Both the child's capacity to perceive and the sounding chime's capacity to be heard are fully actualized in the child's hearing the sounding chime on Aristotle's account.

McDowell's account differs from Aristotle's in holding that the first actualization of the child, the realization of her capacity to perceive things as they are (for example, the chime as sounding), is acquired not in the course of embryonic development but

[31] Thus he thinks that what we perceive are only the proper and common sensibles, that is, colors, shapes, smells, and so on, and not things such as other people, apples, and the like. Only a social, evolutionary account can make sense of the manifest fact that what we perceive first and foremost are biologically salient objects with their natures and powers, the very things that Aristotle describes as primary substances.

instead in the course of the child's acculturation into language. And just the same must be said of the object of perception. Much as learning one's first language realizes one's potential as a perceiver so the emergence of a natural language with which to talk about such perceptible objects as sounding chimes realizes the potential of things to be perceived, and the world as a whole to be in view. The point is not that truth is *relative* to language; it is rather that the world is actualized as knowable, much as an animal is actualized as a knower, through the emergence of language.[32] Independent of the emergence of a language with which to address the world, the world is rather like a wind chime when it is not sounding. The world, more exactly the things in it, are not yet perceptible though they have the potential to be so. Our acquiring the eyes to see, the power to take in things as they are through the acquisition of language, is *by the same token* the world's acquiring a face or presence for us, the power to be seen. As there come to be stuffs with the significance of food with the emergence of animals for whom the stuffs are food, so things come to have the significance of being perceptible, knowable objects with the emergence of rational animals capable of perceiving and knowing them.[33]

The emergence of perceivers and agents through their acculturation into natural language is by the same token the emergence of the world with a face by which to be seen. And this is the right way to think about what language realizes at least in part because the emergence of rational animals of the sort we have described, animals capable of second thoughts, enables in turn a process of *intellectual* maturation through which we acquire not only new beliefs about how things are but also radically new conceptions of what is possible at all. Because what we are aware of, as the rational animals we are, acquires a presence for us *only* as it is caught up in the socially, historically evolved languages we speak, it follows directly that *anything* we think can be called into question, and improved upon. As the course of the story to be told in the chapters that follow aims to show in some detail, the contingency and historicity of our natural languages, far from *barring* us access to objective truth, things as they are in themselves, is what *enables* that access. Precisely because our natural languages are merely contingent and historical, we can, and in the fullness of time do, come to radically new sorts of languages with which to address the world and thereby radically transformed cognitive orientations on reality, and, ultimately, a new conception of language itself as it is the medium of our cognitive involvement in the world. But all this is possible only because and insofar as we first emerge as rational animals through our acculturation into natural language, an acculturation that culminates in the synthesis into one unified and integrated whole all the various

[32] The idea that language is in this way the medium of our awareness of the world around us can be fully explicated only much further on in our story. It is only very late in our intellectual development, for reasons that will become clear, that we acquire the resources properly to understand the nature of language and its role in our lives. It is only at the end of the story told here that we can fully understand its beginning.

[33] Haugeland (1998) also defends this point, and offers various details that we cannot go into here.

paths through the cultural terrain, and realizes thereby the power of knowing of a rational animal, the capacity to take in things as they are.

1.5 The Nature of Natural Language

Natural language is a socially evolved verbal practice into which one is acculturated in childhood and through which one is realized as a rational animal capable of properly intentional action, perception, and thought. As such it has various distinctive and fundamental features. First, as we have already seen, to be initiated into a natural language is to acquire an understanding of what is a reason for what, that is, of the inferential linkages among the various thoughts expressible in the language. But we have also seen that to be initiated into a natural language is to acquire the eyes to see things as they are, to take in manifest facts. Natural language must involve, then, both inferential (that is, intentional "word–word") relations and referential (intentional "word–world") relations, neither of which is intelligible without the other: referential relations are properly speaking referential only in light of the relevant inferential relations, and inferential relations are properly speaking inferential only in light of the relevant referential relations. There can be a view from here for me now, that is, things showing themselves as thus and so to me here and now, if and only if I have the world in view, if and only if the view from here and now is conceived as a part of the larger whole that is the world.

Natural language, on this conception, is obviously object involving. We come to speak about the things in our environment, things that matter to us as the animals we are and that we can see, hear, taste, and so on. But it is not only objects that are caught up in the "web of language" on this account. The look of things too is so caught up.[34] This is especially clear in the case of a sensory quality such as red but it is no less true, I will suggest, in the case of other sorts of concepts of natural language. McDowell (1994, 29) describes concepts of sensory qualities in particular, qualities such as, say, that of redness, as "those which cannot be understood in abstraction from the subjective character of experience." As the point is then explicated,

> what it is for something to be red, say, is not intelligible unless packaged with an understanding of what it is for something to look red. The idea of being red does not go beyond the idea of being the way red things look in the right circumstances.

Obviously, then, one could not have the concept of something's being red unless one was capable of the appropriate experiences. Someone blind from birth, for example, someone lacking the relevant experience and so an understanding of what it is for

[34] And it is not only the look of things, their sensory qualities, that is so caught up. Again, things matter to us; they are dangerous or funny, beautiful or disgusting, magnificent or base. These too are perceptible features of things and they are inevitably caught up in the "web of language" as well. See McDowell (1983) and (1985).

something to look red, could not properly understand what it is for something to be red. And, of course, we may ourselves be blind in a broader sense to some sensory qualities; as McDowell (1994, 123, n. 11) says, "there might be concepts anchored in sensory capacities so alien to ours that the concepts would be unintelligible to us."[35]

Now it is standard to contrast such a concept as that of being red, that is, a concept of a secondary quality, the idea of which does not go beyond the way things having that quality look (smell, taste, feel, or sound) in appropriate circumstances, with concepts of primary qualities that are not phenomenal in the same way. But it does not take much reflection to realize that most concepts of natural language are concepts neither of primary nor of secondary qualities as these have just been characterized.

Notice first that McDowell gives what are in fact two quite different characterizations of sensory qualities, both a weaker characterization according to which such concepts cannot be understood in abstraction from the subjective character of experience, and a stronger one according to which the idea of being red (say) does not go beyond the way red things look in the right circumstances. The weaker claim suggests that the subjective character of the relevant experience is necessary, the stronger that it is sufficient. Both are true for the case of a sensory quality such as redness. (It is sufficient because the appeal to the way things look already takes care of the need for inferential articulation; so it is perfectly correct to say that a concept of a sensory quality does not go beyond the way such things look in the relevant circumstances. One is not thereby saying that the only thing one would need to have the concept is the experience in the sense that a non-rational animal has experience, because one cannot have the relevant experience, its looking to one that there is a red thing there, except against the background of a view of the whole.)

Having the relevant experience is at once necessary and sufficient for grasping the concept of a secondary quality such as that of redness. The case of primary qualities is very different. Consider, for example, our modern concept of a sphere, that is, the concept of a two-dimensional surface all points of which are equidistant from a center. This is not an experiential concept; unlike Aristotle's notion of a sphere as a common sensible, that is, an object with a characteristic look and feel, the modern notion of a sphere is not the notion of a sensory object.[36] Most concepts of everyday natural language are neither concepts of secondary qualities nor concepts of such primary qualities. The concepts of natural language, such as that of a horse or a cat, that of coffee and of chocolate, of fur and of ice, certainly go beyond the way such things appear sensorily to us in appropriate circumstances; but it is far from clear that our everyday concepts of such things could be understood in abstraction from the

[35] This may seem—perhaps especially to readers of Donald Davidson's "On the Very Idea of a Conceptual Scheme," in Davidson (1984)—to be problematic, but we will see that it is not. It is not at all surprising that the natural languages of rational beings radically unlike us in their biological makeup would be unintelligible to us.

[36] We will see in Chapter 3 how this modern notion might arise.

subjective character of experience. Could a being that knows nothing of what water, for example, feels and tastes like to us, of how it sounds and looks in various circumstances, have our (everyday) concept of water? The answer seems obvious. Such a being could no more have our concept of water than someone blind from birth could have our concepts of colors.

Our sensory experience of the world pervades our understanding of the things we find in it in everyday experience; our concepts of the objects that are caught up in the web of our natural languages are ineluctably concepts of sensory objects, that is, of objects that look, feel, taste, smell, and sound in characteristic ways.[37] Our concepts of such objects do go beyond the way such things show up in our sensory experience of them, but they nonetheless cannot be understood in abstraction from the subjective character of our experience. Natural language is not, in other words, merely a repository of tradition in McDowell's sense, "a store of historically accumulated wisdom about what is a reason for what" (McDowell 1994, 126). It is also an embodiment of a particular sensory view of the world, one that is inextricably tied to a particular biological form of life, to a particular sort of sentient being.

I have suggested that our sensory experience of things pervades our everyday understanding as embodied in the natural languages we speak. It is just this common experience grounded in our common (biological) form of life that accounts for the inter-translatability of all human natural languages, and predicts the untranslatability of the natural languages of creatures evolved to have radically different (biological) life forms. Given the role of acculturation into natural language, any being capable of learning a natural language must share the sense modalities of other speakers of the language (at least some of them); but nothing in the very idea of acculturation requires that there be any overlap between the sense modalities of one life form and those of another (any more than there need be an overlap between means of reproduction, self-movement, and nourishment). It follows that it is perfectly intelligible that there would be natural languages that are completely untranslatable into our own. It is just as Wittgenstein says (1976, 223): "if a lion could talk, we would not understand him."

Natural language, I have suggested, is at once inferentially articulated and essentially referential. It is also essentially sensory; not only our concepts of sensory qualities but many, perhaps all the most basic concepts of our natural languages would be unintelligible to a being that lacked the sorts of sensory capacities we have evolved to have. The fact that all humans share, at the level of biology, a form of life explains the fact that all human languages are inter-translatable.[38] But why, then, is

[37] Again, I abstract from the affective features things inevitably have for us, the aesthetic, moral, and emotional coloring we experience them as having.

[38] As Tomasello (2008, 310–11) notes, "people speaking in any language conceptualize the world in similar ways in terms of such things as agents acting on objects, objects moving from and to locations, events causing other events, people possessing things, people perceiving and thinking and feeling things, people interacting and communicating with one another—all involving a basic event-participant

translation from one natural language to another sometimes so difficult? An answer is suggested by the fact that the more distant the alien culture, either in space or in time, the more difficult the translation. The problems that might arise due to distance in space, that is, translation from a language that evolved in one sort of environment into a language that evolved in another, very different environment, are easily comprehended based on what has already been said about the role of sensory experience in our natural language conceptions of things. Distance in time points in a different direction, to the essentially historical character of natural language.

Wittgenstein argues in the *Philosophical Investigations* that while some of our concepts might be adequately understood by reference to a kind of standard or paradigm case, many others are correctly applied to a range of things that exhibit only a "family resemblance" in common.[39] Whereas in the former case all correct applications refer back to the one standard or paradigm case (so are all alike in the same respect, namely in being like the standard or paradigm), in the latter case similarities between the correct applications need only overlap, like fibers twisted one on another over the length of a thread. (See Wittgenstein 1976, sec. 67.) Cavell (2000, 30) offers this example: "we learn the use of 'feed the kitty', 'feed the lion', 'feed the swans', and one day one of us says 'feed the meter', or 'feed in the film', or 'feed the machine', or 'feed his pride', or 'feed wire', and we understand, we are not troubled." As he notes, these various projections (as Cavell calls them) pick up on different aspects of what it is to feed an animal. To feed a meter is to put coins into it as one might put morsels of food into the open mouth of a small child, and in both cases it is critical that the thing that is put be appropriate: one does not feed a meter by putting wafers in the coin slot and one does not feed a child by putting coins in its mouth. To feed a machine is to put some material, some stuff into it that will be processed in some way by the machine as part of what it is designed to do. One feeds a sausage maker with sausage meat; one does not feed it with the oil that keeps its gears running smoothly, even though oil is put into a machine too and is in its way processed as a part of what the machine is designed to do. And although one may also feed a machine some energy source, perhaps gasoline for its motor, one does not feed it gasoline in quite the same sense as one feeds it sausage meat. One is in the two cases projecting different features or aspects of feeding. Feeding someone's pride is different again because in this case the feeding can involve also the idea of growth: a meter or sausage machine does not grow when it is fed, but someone's pride can. One can also feed a drug addiction but nothing is being fed when one is vaccinated.

distinction." As he further notes, we also share similar "social intentions and motives" and "manipulate the attention of others in similar ways"; we "learn and process information in similar ways," have the same vocal-auditory organs and skills, and share a common evolutionary history. There are differences across different languages, but there is also a great deal of commonality. It is this commonality that grounds and explains the inter-translatability of all human languages.

[39] What Fauconnier and Turner (2002) call conceptual blending would seem to be a closely related phenomenon.

As these examples indicate, successful projections will seem appropriate, natural, and even illuminating. Learning that some material is fed, as opposed to merely put, into a certain machine, for example, is to learn something about its function relative to the workings of the machine as a whole. Other projections will not work as well, or at all. Furthermore, the answer to the question of what does and does not work can depend on what sorts of uses have already become entrenched in the language and thereby on which features or aspects have already been highlighted as salient. We do not take the building of a model airplane or the playing of a musical instrument simply for one's own enjoyment as instances of playing a game. But it is not hard to imagine an alternative history of the use of the word 'game' in which these activities did count, for instance, a history in which solitary games such as patience, which are played simply for one's own amusement, were more salient instances of games than they have in fact turned out to be.[40] And like Burge's arthritic patient (Burge 1979), one can find oneself surprised one day to find that one's own language does (or does not) countenance some application that for all one knows oneself is not (or is) a correct application.

There are, then, two sources of variation across human natural languages (aside from the contingencies leading to various sounds serving their roles in the language): the physical and cultural environment in which it is embedded, and the contingencies of its historical development. It follows that translation from one natural language to another can never be merely mechanical, indeed that learning a second language is in fundamental respects like learning one's first language. Although it may be the case that the meanings of some words can be learned by appeal to simple rules, the fact that languages are historically evolved in ways that give rise to family resemblances ensures that the meanings of many words cannot be learned that way. In some cases one may be able to find a more or less close analogue that, perhaps with commentary, conveys the meaning in question, but in others one will have to learn as a child learns by being initiated into the language through practice, that is, by being immersed in it.

We have seen that natural language is inherently sensory, object involving, and historical. It is also inherently narrative, a language within which to tell not only what happens but also, and more importantly, what happens next.[41] The capacity to tell what happens requires, as Aristotle already saw, that there be two quite different modes of predication in the language.[42] To say what happens one must be able both

[40] I have been told by a native speaker that in French one does call building a model airplane a game.

[41] Turner (1996) argues that narrative in this sense, the capacity to tell stories, is fundamental to our thinking.

[42] In *The Discovery of Things*, Mann makes the essentially related point that Aristotle is the first explicitly to recognize that things show up for us as things (with properties and relations). Indeed, he claims that before Aristotle things did not show up as things, that earlier Greek thinkers "did not recognize things as things" (Mann 2000, 5). This seems to me too strong or at least misleading. Before Aristotle thinkers were recognizing things as things only they did not yet explicitly know themselves to do so. Aristotle's discovery is a philosophical one, not an empirical one.

to say what something is and to say what it is like, what attributes it has. We say, for example, that Socrates is a man. And as Aristotle notes already in the *Categories*, we can also say that Socrates is what a man is, namely, a rational animal. But, although we also say that Socrates is, say, just, we cannot say that Socrates is what Justice is, namely a particular virtue. 'Man' is said of Socrates synonymously, 'just' of him homonymously. We have, then, two fundamentally different sorts of predication, essential or substantial predication, predication of what a thing *is*, and accidental predication, predication of what attributes a thing has, of what it is *like*. The two are logically different.[43]

In the *Physics*, Aristotle extends the account to clarify the nature of change. Because every thing (object or stuff) both is what it is—that is, the kind or sort of thing it is—and also has various properties, for instance, shape, size, color, location, and so on, we can understand change in terms of the idea that while the object or stuff persists through the change, some property of it comes to be or passes away. A person can become musical (that is, knowing music) from being non-musical (not knowing music); a lump of bronze can become a statue from being (merely) a heap of bronze; and so on.[44] That is, we can talk of things doing things and changing, of what happens, because there are two essentially different ways to be, and correspondingly two logically different modes of predication. It is in just this sense that natural language can be said to be essentially narrative: it is addressed not merely to the world but to the world as it unfolds as a story does, over time, and so speaks of kinds of things as doing things, as coming to have this or that property, and as coming to be in this or that relation.

Change, we have seen, is constitutive of living beings insofar as they live narratively structured lives, lives that have characteristic beginnings (what happens first), middles (what happens next), and ends (what happens last). It follows that though the sensibility of another sort of rational being could be very different from our own, so different that we would be incapable of understanding what it is like to be such a being and thereby incapable of understanding the natural language of that being, nonetheless we can know at least that its language would be narrative, that it would involve two modes of predication. Not merely human natural languages but any natural language must be as we have described, inextricably inferential and referential, essentially sensory, object involving, historical, and narrative.[45]

[43] See Gupta (1980) for a more recent defense of the idea that predicating (say) 'man' of Socrates involves a different logical form from predicating (say) 'pale' of Socrates.

[44] Aristotle, of course, also distinguishes between coming to be (that is, generation) and coming to be something (which involves the acquisition or loss of some property).

[45] Tomasello (2008, 290) claims that "all people in all cultures tell stories" and argues, in Chapter 5, that doing so helps to forge cultural identity. The claim here is the stronger one that *any* sort of rational animal, whatever the details of its biology, would have a narrative language, one within which to tell what happens. On this point compare Rödl (2012), which also argues, though in a very different way, that the most fundamental categories of thought are temporal. Rödl takes this to be true of (finite) thought as such. But it is not, as we shall see. Mathematical thought is not temporal but it is, or at least can become, a fully fledged form of thought about reality.

1.6 Conclusion

As our discussion of Brandom's project in *Making It Explicit* aimed to bring out, the account we have begun to develop must satisfy two desiderata. It must be naturalistic, that is, fully compatible with our best scientific understanding of what there is, and it must enable us to understand ourselves as rational beings answerable in our judgments to the norm of objective truth. We can satisfy these desiderata, however, only if we make a decisive break with the tradition that, since Descartes, has understood what is "outside" to be merely causal, normatively inert, although what is "inside" is and must be conceived as normative. That this distinction is profoundly misconceived is indicated already by the case of living organisms, which are evolved together with their affordance-rich environments through the process of evolution by natural selection. But we are not ourselves merely biologically evolved. Although we are born merely natural animals, albeit ones with a few distinctive, biologically endowed capacities such as the capacity to imitate, we are transformed in the course of our acculturation into a new sort of being, into rational animals. Whatever the exact details, the process must involve, I have suggested, three essentially different stages. It involves, first, the evolution of social practices through the ratchet effect that is made possible by our powers of imitation. Then, at the second stage, we are realized as players of socially instituted verbal games, where this depends also on our ability to cooperate and share goals. And finally there is the transformation—through the coalescence of loci of authority that is enabled by our ability to synthesize procedural knowledge into a single unified whole—of such players into properly rational beings capable of distinguishing in principle between how things seem and how things are. In the process, what at the second stage have the status only of moves in a kind of verbal game, that is, of socially significant game entries, game exits, and intra-game moves, come to be properly described as instances of (fully intentional) perception, action, and judgment. Perception, we have seen, is a prima facie entitlement, a fallible power to take in how things are. Action is the realization of a commitment freely undertaken, the fallible power to do as one wills. And judgment, finally, is, or at least aims to be, an acknowledgement of truth, at once an act of freedom and answerable to things as they are.

The world as it is revealed in and through natural language is inherently sensory. It is a world of objects, paradigmatically, living beings with their natures and characteristic powers. And we, as the rational animals we are, have the power to take in, in perception, as well as to say, how things are with such objects, and more generally what is happening. Perception, on this account is not, as action is, something we do but instead is an actualization with respect to which one is passive. One simply finds things to be thus and so in perception. Judgment, on our account, although not passive, is also not an action of the ordinary sort. It is not a move in a language game, or an act of assent to a contentful proposition; it is not a commitment that one forms. Instead it is, or aims to be, again, an acknowledgement of truth, an actualization of one's power to know that, when successful (because what is acknowledged is true and recognized as such), fully manifests that power in a cognitive relation of knowing to the thing known.

But not everything there is to be known about the world is available from the perspective afforded by natural language. Indeed, it can seem that nothing that is afforded by natural language properly counts as knowledge. Even to the ancient Greeks, our knowledge of the familiar everyday world of change and becoming seemed somehow second best, somehow partial, limited, and unsatisfyingly perspectival, when compared to knowledge of things such as mathematical objects that are grasped instead by the mind's eye and do not change. Although the paradigm of cognition for the ancient Greeks is our perceptual grasp of an object as what it is in its nature, and hence is essentially bodily and sensory, already they had a vision of a purely cognitive, purely rational and non-sensory grasp of what is. It is the subsequent unfolding of this marvelously seductive idea, beginning with the ancient practice of diagrammatic reasoning, that will occupy us for the remainder of this work.

2

Ancient Greek Diagrammatic Practice

Mathematics, the oldest and most venerable of all the sciences, begins first and foremost with the ancient Greeks. Although many mathematical discoveries were made also in ancient India and ancient China, it was the Greeks who realized mathematics as a systematic science within which to demonstrate an extraordinary range of properties and relations of mathematical entities from the very simple to the remarkably complex.[1] Our aim is to understand the practice of this science, in particular, the use of diagrams in ancient Greek mathematical practice. That is, we want not merely to understand the mathematical results but also, and primarily, the means by which those results are established in Greek diagrammatic practice, how the practice works as mathematics. We will be especially concerned, first, with the sort of generality that is involved in ancient Greek diagrammatic reasoning, and also with the very idea of diagrammatic reasoning, what it means to reason *in* a diagram as opposed to reasoning on the basis of a diagram.

It is often remarked that mathematical knowledge is cumulative in a way that empirical knowledge is not, that there are no revolutions in mathematics comparable to those that have occurred in the empirical sciences. But although revolutions perhaps do not occur in mathematics, at least as they do in the empirical sciences, nevertheless, as Crowe (1975, 19) remarks, "revolutions may occur in mathematical nomenclature, symbolism, metamathematics (e.g. the metaphysics of mathematics), methodology (e.g. standards of rigor), and perhaps even in the historiography of mathematics."[2] Although the mathematics itself may not change, nevertheless, our understanding of the mathematics can and does change, sometimes very radically.[3] This idea that the mathematics itself does not change is furthermore often formulated

[1] See Bashmakova and Smirnova (2000).

[2] Crowe (1975) argues that there are no revolutions in mathematics comparable to those in the natural sciences. Later, in Crowe (1988), he reverses his view. Crowe (1975) has generated a great deal of further work. For a recent overview see François and Van Bendegem (2010).

[3] Putnam (1972, 5) suggests something similar for the case of logic. As he writes, for instance, of the law of identity or of inference in *barbara*, "even where a principle may seem to have undergone no change in the course of the centuries... the *interpretation* of the 'unchanging' truth has, in fact, changed considerably."

in terms of the idea that the earlier conceptual framework is always translatable into the later one.[4] And at least in some cases, the translations seem not merely to preserve the mathematics but to *reveal* the mathematics in a way that the original formulations could not.[5] It was, for example, widely argued in the nineteenth century that ancient Greek mathematics is algebra in geometric dress, that is, that it is really algebra though the method is geometrical.[6] And there is a sense in which this is true: certain parts of Greek mathematics achieve a kind of closure when translated into the language of algebra. But although it may be correct to describe the relevant *mathematics* as algebra, to think of Greek mathematical *practice* as "algebra in geometrical dress" neglects the fact that, although its fruits can be translated into the language of algebra, Greek mathematical practice is not algebraic. Although the *mathematics* (in relevant instances) may be algebra, the *language* and *practice* of ancient Greek mathematics is geometry.[7]

If in the nineteenth century Euclid was read through the lens of the sort of algebra that was first made possible by Descartes, today it is more common to read Euclid through the lens of twentieth-century quantificational logic and current mathematical practice (or at least what many philosophers think of as current mathematical

[4] Friedman claims, for example, that "revolutionary transitions within pure mathematics . . . have the striking property of continuously (and, as it were, monotonically) preserving what I want to call *retrospective* communicative rationality: practitioners at a later stage are always in a position to understand and rationally to justify—at least in their own terms—all the results of earlier stages" (Friedman 2001, 96); "in pure mathematics . . . there is a clear sense in which an earlier conceptual framework (such as classical Euclidean geometry) is always translatable into a later one (such as the Riemannian theory of manifolds)" (Friedman 2001, 99). Stein (1988, 238) similarly remarks, "a mathematician today, reading the works of Archimedes, or Eudoxus's theory of ratios in Book V of Euclid, will feel that he is reading a contemporary."

[5] This is because, as Wilson (1995, 111) notes, "the 'proper' definition of a mathematical term should not rest upon the brute fact that earlier mathematicians had decided that it should be explicated in such-and-such manner, but upon whether the definition suits the realm in which the relevant objects optimally 'grow and thrive'." And this is true even in the case of mathematical constants. For example, we take the number π, that is, the ratio of the circumference of a circle to its diameter, to be a basic and important mathematical constant, but Palais (2001) persuasively argues that it makes more mathematical sense to use the ratio of the circumference to the *radius* as the basic and important constant. It would have been natural for the ancients (whether Greek or Chinese or Indian) to fix on the diameter given that for them a circle is an object, a kind of two-dimensional geometrical figure. But once one has achieved the modern notion of a circle as given by the familiar equation $x^2 + y^2 = r^2$, it is clear that what matters is not the diameter, but r, that is, the radius. See also Hartl (2010), who suggests that the constant ought to be given the symbol *tau*, τ.

[6] We find such arguments in, for example, Georg Nesselman's *Die Algebra der Griechen* (1847), Paul Tannery's "*De la solution géométrique des problèmes du second degree avant Euclide*," *Mémoires scientifiques* (1882), and Hieronymous Zeuthen's *Die Lehre von den Kegelschnitten im Altertum* (1886). The interpretation was taken up by Heath in his English translation of the *Elements* first published in 1908, and was further applied also to Babylonian mathematics in the 1930s by Otto Neugebauer. Bartel L. van der Waerden, following Neugebauer, argues in his influential *Scientific Awakening* (first published in Dutch in 1950, then in English in 1954) that the Greeks clothed their algebra in geometrical dress under the duress of the discovery of irrational quantities.

[7] See Szabó (1978), especially the Postscript, and Unguru (1979a) and (1979b). See also Høyrup (2004) for a thoughtful discussion of the ways in which "mathematical concepts and conceptual structures are formed in interaction with tools within a practice" despite there being no "clear one-to-one correspondences between practices and mathematical conceptual structures" (Høyrup 2004, 134).

practice), to take the system of mathematics presented in the *Elements* to be an (imperfectly realized) axiomatic system in which theorems are proven and problems constructed though chains of diagram-based reasoning about instances of the relevant geometrical figures. But this reading, as a reading of ancient Greek mathematical practice, is as misguided as the algebraic reading. The *Elements* is not best thought of as an axiomatic system in our sense but is more like a system of natural deduction; its Common Notions, Postulates, and Definitions function not as premises from which to reason but instead as rules or principles according to which to reason. Furthermore, and especially important to the historical developments that are of concern to us here, demonstrations in Euclid do not involve reasoning about *instances* of geometrical figures, particular lines, triangles, and so on, but are instead general throughout. Equally importantly, the chain of reasoning is not merely diagram-based, its moves, at least some of them, licensed or justified by manifest features of the diagram. The reasoning is instead diagrammatic. One reasons *in* the diagram in Euclid, or so it will be argued.

2.1 Some Preliminary Distinctions

In the science of mathematics, whether ancient or modern, truths are established by way of some sort of demonstration. If one does not have a demonstration then, however obvious it is that one's claim is true, one has only a conjecture, not mathematical knowledge.[8] A (written) mathematical demonstration is, furthermore, self-standing: by contrast with (written reports of) empirical findings, a mathematical proof *itself* carries evidentiary force; the intentions, reliability, and trustworthiness of its author are irrelevant to its cogency.[9] In mathematics one must be able, at least in principle, to see for oneself that things are as the author argues they are. (This is also the case in philosophy.) Mathematical knowledge is, in this sense, a priori; it is a priori insofar as it does not rely on empirical evidence, whether the evidence of one's own senses or the testimony of another. Mathematical knowledge relies instead on self-standing proof.

In mathematics one can see for oneself how it goes. But this can mean different things in different cases. Suppose, for example, that you are given the task of dividing eight hundred and seventy-three by seventeen. You might solve the problem by engaging in a bit of mental arithmetic that you could report as follows.

[8] Of course, proof in general requires that something be given at the outset, whether in the form of axioms or something else. Not *everything* can be proved, even in mathematics. For the moment, we leave this obvious fact aside.

[9] This feature of mathematical proof is sometimes described as its "transferability"; and it has been used by Easwaran (2009) to argue that probabilistic (mathematical) proofs are essentially different from standard mathematical proofs precisely insofar as they are not transferable but depend, as do reports of experimental results generally, on one's recognition of the (Gricean) intention of the author to convey the information reported.

$$
\begin{array}{r}
\underline{51} \\
17\surd 873 \\
\underline{85} \\
23 \\
\underline{17} \\
6.
\end{array}
$$

Figure 2.1 A paper-and-pencil calculation in Arabic numeration.

> A hundred seventeens is seventeen hundred; so fifty seventeens is half that, eight hundred and fifty. Add one more seventeen—so that now we have fifty-one seventeens—to give eight hundred and sixty-seven. This is six less than eight hundred and seventy-three. So the answer is fifty-one with six remainder.

Alternatively, you might do, or imagine yourself doing, a standard paper-and-pencil calculation in Arabic numeration the result of which will (if you were schooled in North America) look something like the array displayed in Figure 2.1. Either way, one gets the answer that is wanted. But the two collections of marks serve very different purposes. The written English, it seems fair to say, provides a *description* of some reasoning, in particular a description of the steps a person might go through in order to find, by mental arithmetic, the answer to the problem that was posed. The description does not give those steps themselves. To say or write that a hundred seventeens is seventeen hundred is not to calculate a product. Even more obviously, to say or write the words 'add one more seventeen' is not to add one more seventeen. It is to instruct a hearer or reader: at this point in their reasoning, assuming they are following the instructions, they are to add seventeen.

The paper-and-pencil calculation is radically different. It is manifestly not a *description* of a chain of reasoning one might undertake; rather it *shows* a calculation—at least it does so to one familiar with this use of signs to divide one number by another. (Notice that the paper-and-pencil calculation does not show the steps that are described in the first passage; calculating in Arabic numeration does not merely reproduce mental arithmetic but involves its own style of computing.) Working through this collection of signs in Arabic numeration, or performing the calculation from scratch for oneself, just *is* to calculate. Here we have not a *description* of reasoning but instead a *display* of reasoning. One calculates *in* the system of Arabic numeration in a way that is simply impossible in natural language. To one who is literate in this use of the system of written signs, the calculation shows the reasoning; it embodies it.

This distinction between *describing* or *reporting* a chain of reasoning in some natural language and *displaying* or *embodying* a chain of reasoning, for instance, in the system of Arabic numeration, is at once intuitively obvious and elusive insofar as it seems natural in a way to say that one reasons in natural language more or less as one reasons in, say, Arabic numeration. Certainly it is true that when I do mental arithmetic I in some sense do it in English as contrasted with Chinese or Italian. But one does not reason *in* natural language in the same sense in which one reasons in (say) Arabic numeration. One cannot divide the word 'sixty-nine' by three any more than one can bisect the word 'line', but one *can* divide the numeral '69' by three (in a calculation) as one can bisect a drawn line (in geometry). Arabic numerals are designed to be operated on; written words are not. The function of written words is, first and foremost, to record speech. I can tell, by speaking or writing, what happens or what is the case, how I have reasoned or what is true, but the reasoning itself is something different, as a different example may help to make clear: the proof, known already to the ancient Greeks, that there is no largest prime. Here it is.

> Suppose that there are only finitely many primes, and that we have an ordered list of all of them. Now consider the number that is the product of all these primes plus one. Either this new number is prime or it is not. If it is prime then we have a prime number that is larger than all those originally listed; and if it is not prime then, because none of the numbers on our list divide this new number without remainder (because it is the product of all those primes plus one), this new number must have a prime divisor larger than any of the primes on our list. Either way there is a prime number larger than any with which we began. Q.E.D.

This proof, like a bit of mental arithmetic, clearly does not rely on any system of written signs. It depends not on the capacity to write but on the capacity to reflect on ideas, and to think, that is, to reason or infer.[10]

A calculation in Arabic numeration is essentially written—though of course the writing can be merely imaginatively performed rather than actually performed. The proof that there is no largest prime is not. Although the words clearly do convey the line of reasoning, the proof is not *in* the words (whether spoken or written); it is not the words that one attends to in the proof that there is no largest prime, but instead the relevant ideas, central among them the idea of a number that is the product of a collection of primes plus one. The task of the proof is to think through what follows in the case of such a number. The words do not display the reasoning but only describe it.[11]

In the case of a calculation in Arabic numeration, a person proficient in the system literally sees how it goes; the reasoning is not merely reported but is itself embodied in the system of written signs. But not all systems of written signs support reasoning in the same way. In some cases, although one can reason *on* the signs (as we can put it),

[10] As Kant might think of it, whereas a calculation in Arabic numeration involves an intuitive use of reason, a construction (in pure intuition), the reasoning involved in the ancient proof that there is no largest prime instead makes a discursive use of reason directly from concepts. See Kant (1781), first section of the first chapter of the *Transcendental Doctrine of Method* (especially A712/B740–A723/B751). Kant's understanding of the practice of mathematics is taken up in Chapter 4.

[11] As we will see in section 6.4, the reasoning that is needed in proofs of theorems in current college-level textbooks is similarly described rather than displayed. This is furthermore characteristic of the reasoning that is involved in the mathematical practice that first emerged in the nineteenth century in Germany, and especially significant to our concerns in this work. We will need to understand why current mathematics has no (mathematical) language within which to work. We will also need to understand what such a language within which to display the sort of reasoning that is involved in contemporary mathematical practice might look like.

one cannot reason *in* the signs. This distinction is critical to our philosophical purposes, but also subtle. We will approach it by considering a range of cases.

Case One. I am a merchant selling eggs. I keep my eggs in boxes of a dozen each; that is, when I have twelve eggs I put them in a box. And when I have six boxes I put them in a crate for storage. Suppose now that I have three crates, five boxes, and eight loose eggs. I receive a delivery of two crates, two boxes, and seven eggs. I add the two crates to the three I already have and the two boxes to the five I already have. Because I now have seven boxes, I put six of them into a crate, which I then put with the other crates. Combining the seven newly delivered loose eggs with the loose eggs I already had, I see that I have enough to fill a box so I do that and put this new box with the one that was left over after I crated the six boxes, leaving me with three loose eggs. Looking around my shop I see that I now have six crates, two boxes, and three loose eggs.

Case Two. I am again an egg merchant with three crates, five boxes, and eight loose eggs, and again receive delivery of two crates, two boxes, and seven eggs. Because I have hired someone to do the menial labor around the shop, I do not now deal directly with the eggs. Nonetheless, I want to know how many I have and for this I have devised a system of written marks. Using strokes to stand for loose eggs, one for each, crosses to stand for boxes, one for each, and crosshatches to stand for crates, one for each, my record of what I have already in store looks like this:

♯♯♯ + + + + + ////////.

Now I add to my tally the appropriate signs standing for what has been delivered, namely, two crates, two boxes, and seven eggs:

♯♯♯♯♯ + + + + + + + ///////////////.

And much as before I did with the eggs and boxes, I now put twelve strokes "into" one cross and six crosses "into" one crosshatch. That is, I erase twelve strokes and add a cross, and similarly, replace six of the crosses with a crosshatch:

♯♯♯♯♯♯ + + ///.

Without going anywhere near my merchandize I see that I have six crates, two boxes, and three eggs.

Case Three. Having diversified my enterprise so that I now deal in eggs, various sorts of fruits, three kinds of grains, and a wide array of spices, I have given up trying to have different signs for all the different sorts of goods. I adopt instead a numeral notation that can be used for all, together with signs for each sort of foodstuff so as to be able to record, using separate sorts of signs, both how many and of what. I adopt, let us say, the system of Roman numeration: 'I' stands for one thing, 'V' for five, 'X' for ten, 'L' for fifty, 'C' for one hundred, 'D' for five hundred, and 'M' for one thousand. As it helps me to think of it, V is a bag of five things; two V-bags go in an X-box; five X-boxes make an L-crate; two L-crates go in a C-bin; five C-bins are a D-pallet; and two D-pallets form an M-store. As before, I have three crates, five boxes, and eight loose eggs. Each crate contains six times XII eggs: XXXXXXIIIIIIIIIIII, that is, LXXII eggs. So the number of eggs in three crates is that taken three times: LLLXXXXXXIIIIII or CCXVI eggs. Five boxes is XII taken five times: XXXXXIIIIIIIIII, or LX. I now can determine how many three crates, five boxes, and eight loose eggs is simply by putting all the signs together and replacing signs as necessary: CCLXXVVIIII, that is, CCLXXXIIII. The new delivery is, again, two crates, two boxes, and seven eggs. I know that one crate has LXXII eggs in it, so two crates is double that: LLXXXXIIII or CXXXXIIII. Two boxes is double one: XXIIII, and I get the total number of eggs delivered by putting together the numerals for crates, boxes, and eggs: CXXXXXXVIIIIIIIIII, that is, CLXXV. Adding that to my existing stock gives CCCLLXXXXXVIIII or CCCCLVIIII.[12]

Case Four. Having been taught the positional system of Arabic numeration together with the means of calculating in this system, and faced with the same problem of determining how many eggs I have in light of the new shipment, I now determine that my existing stock of three crates, five boxes, and eight eggs is 3 times 6 times 12, and 5 times 12, and 8. I know, because I have memorized it, that 3 times 6 is 18. To determine 18 times 12, I write '18' then just below it '12', and do what is to us a very familiar paper-and-pencil calculation that yields 216. A similar calculation shows me that 5 times 12 is 60. So I add, in the way we all learned as children, 216 and 60 and 8 to give 284. The new delivery is two crates, two boxes, and seven eggs: 2 times 6, that is, 12, times 12, plus 2 times 12, plus 7. A paper-and-pencil calculation tells me that 12 times 12 is 144 and that 2 times 12 is 24. The new delivery is then 144 plus 24 plus 7, which I add up the usual paper-and-pencil way to give 175. Now I need to add 284 and 175, which I do by more paper-and-pencil scribbling to give 459.

In the first case, in which I deal directly with eggs, boxes, and crates, I am clearly not calculating in any arithmetically interesting sense. I am working with stock, not numbers. In the second case, I am working with signs rather than with stock. The signs picture the stock and I can manipulate the signs much as before I had manipulated the stock. Because in the third case I do not directly picture eggs, boxes, and crates but instead use a multipurpose system of numeration, it takes a little more work to record what I have in the notation, but here again the marks I use provide a kind of picture of the stock, of how many eggs I have, and I can manipulate the signs as before I had manipulated the stock. Although I keep my eggs in boxes of twelve eggs and crates of six boxes, in my reckoning I think of them instead as in bags of five eggs, boxes of two bags, crates of five boxes, and so on. In all three cases I literally add things together (and would similarly literally take things away if instead

[12] As our example might suggest, it is very easy to make mistakes in this system, to miscount the various sorts of symbols or to add or remove too many or too few, which may help to account for the fact that numeration systems that picture collections of objects have not historically been used as we have used it here. Instead an abacus or counting board would have been used for the calculation.

of receiving a delivery I send off a shipment). To determine how many is three boxes I write the signs three times, and were I dividing, say, by three, I would look for three occurrences of a sign to separate into three. The notation thus has an immediacy and concreteness that makes it very intuitive and easy to learn.[13]

The same cannot be said of our last scenario involving the positional system of Arabic numeration. In this system, the signs do not simply stand in for objects and collections of objects that are put together to picture larger collections. The system is not additive: '475' does not mean four and seven and five. Not only the numerals but their positions in a given context of use together determine what number is meant. Of course this system is related to the earlier ones, and one might well try to explain how it works by talking of five units, seven tens, and four hundreds on the model of eggs, boxes, and crates. But it nonetheless works as a system of signs on very different principles from the straightforward picturing conventions of a numeration system such as the Roman system. One does not in the case of Arabic numeration operate on the system of signs in a way that mimics one's manipulation of the objects, and there is nothing particularly intuitive about the rules one does follow. One could know full well how to do a calculation in Arabic numeration without having any idea how or why it works, why doing things that way gives the right result. One could not, I think, in the same way know how to manipulate the signs of Roman numeration—how to put collections of signs together, or apart, how to pack and unpack signs by now putting a 'V' for five strokes and now five strokes for a 'V'—without at the same time seeing why it works. Knowing what the signs mean and how they function to stand for collections just is to be able to make sense of the manipulations with Roman numerals.

Over the past five and a half millennia more than a hundred different numeral notation systems, among them Roman numeration, have been developed for the purpose of recording how many. Indeed, "the primary function of numerical notation is always the simple visual representation of numbers. Most numeral notation systems were never used for arithmetic or mathematics, but only for representation" (Chrisomalis 2010, 30). Instead of using (say) Roman numerals to determine how many, as we did in the third case above, one would use, for instance, an abacus or a counting board; instead of manipulating the signs of Roman numeration, one would manipulate counters in a systematic and highly functional way to achieve the desired arithmetical results.[14] Here again, even more obviously than in the case of our manipulations of Roman numerals, one operates on the counters much as one would operate on the things themselves, and can do because the counters directly stand in for things and collections of things, that is, in effect, for eggs, boxes, and

[13] Lengnink and Schlimm (2010) provide some empirical evidence for this claim.

[14] In such a system, it is impossible to make some of the mistakes that are all too easy to make in manipulating the signs of Roman numeration because one is not rewriting but actually physically moving counters.

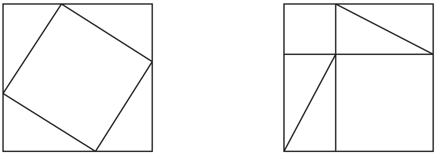


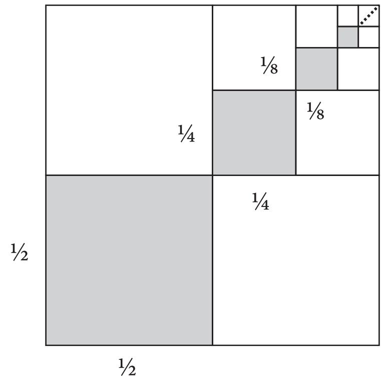
Figure 2.2 A picture proof of the Pythagorean Theorem.

crates. In all such cases, one operates *on* the system of signs rather than *in* it as one does in Arabic numeration.

Although it is possible, as Schlimm and Neth (2008) argue, to determine the answer to an arithmetical problem using Roman numeration, as it is of course possible to do this using Arabic numeration, it does not follow that one is in both cases *calculating* the answer, or at least calculating in just the same sense in the two cases. The fact that the Roman system directly pictures collections and so can be manipulated much as one manipulates objects such as eggs, boxes, and crates, whereas the Arabic does not in the same way directly picture collections and cannot be manipulated as one manipulates objects, matters crucially, at least for our philosophical purposes here. It is just this distinction that I aim to mark by saying that although one can reason *on* collections of Roman numerals, much as one reasons on an abacus or counting board, one cannot reason *in* that system as one does in Arabic numeration. And the same is true, for the same reason, of, for instance, representations of knots in knot theory that can be manipulated using Reidemeister moves. Because the representations directly picture knots, they can be manipulated much as the knots themselves can be. In our terminology, one operates on the system of signs but not in it. And the same would seem to be true of some picture proofs, for instance, the picture proof of the Pythagorean theorem shown in Figure 2.2. Here one directly pictures areas and imaginatively moves them around in a way that reveals that the square on the hypotenuse is equal in area to the sum of the squares on the other two sides. One reasons *on* the display. One of my principal aims will be to show that one does not in the same way reason *on* a diagram in Euclid. Instead one reasons *in* the diagram as one reasons in the written system of Arabic numeration.

Other picture proofs help to bring out another contrast that is relevant here, namely that between reasoning (generally one-step) that makes explicit something that is otherwise only implicit, and the sort of (multi-step) reasoning that one finds in Euclid and, it will be argued, does not merely make something explicit but instead actualizes a potential. Consider, for example, the familiar picture proof to show that ¼ + 1/16 + 1/32 + . . . + $1/4^n$ + . . . = 1/3 displayed in Figure 2.3. Here the display formulates the information in the problem—that one is to add one quarter and one sixteenth and . . . —in a way that enables one to see that in the limit one will have taken one third of the unit square. Looking at the display one way enables one to see it as picturing the infinite sum; looking at it another way one sees it as marking off

Figure 2.3 A picture proof showing that the series ¼ + 1/16 + . . . + $1/4^n$ + . . . sums to 1/3.



one third of the unit square. One can see the display either way and because one can, one comes to see that the equality given above is true. The signs flanking the equal sign express different senses but as the display shows, they designate one and the same thing. Here, it is the shift in one's perceptual focus that effects what we would otherwise think of as a step in reasoning, and what that shift serves to do is to make explicit information that is already implicit in the display.

Similarly in a Venn diagram, or an Euler diagram, one pictures the contents of the premises of a syllogism in a way that (again, by shifting one's perceptual focus) enables one to read off the valid conclusion if there is one.[15] In all three cases—the picture proof in Figure 2.3, a Venn diagram, and an Euler diagram—one pictures the given information in a way that serves implicitly to picture also the desired result. Shifting one's visual attention appropriately one makes that implicit content explicit and thereby sees that the result holds. Diagrammatic reasoning in Euclid, we will see, is not like this; a Euclidean diagram is not a picture proof.[16] Instead, much as a calculation in Arabic numeration does, a Euclidean diagram formulates content in a mathematically tractable way, in a way enabling one to reason in the system of signs in a step-wise fashion from the given starting point to the desired endpoint.

We have drawn three distinctions. The first was between describing or reporting some reasoning in (written or spoken) natural language as contrasted with displaying or embodying the reasoning in a written system of signs. The second distinction was that between reasoning *on* some collection of marks (or collection of counters) and

[15] For further discussion of these two systems, of their similarities and differences see Gurr, Lee, and Stenning (1998). For extensions of Venn-style diagrams to cases involving more than three, even indefinitely many, terms see Moktefi (2008) and Moktefi and Edwards (2011).

[16] We need, then, to distinguish between a broad sense of diagrammatic reasoning according to which it is any reasoning or problem solving involving pictorial representations—as in Chandrasekaran, Glasgow, and Narayanan (1995) and Larkin and Simon (1987)—and the narrower sense of diagrammatic reasoning that is characteristic of reasoning in Euclid and is our particular concern here.

reasoning *in* a system of signs. We saw that when a display directly pictures something, say, a collection of objects or a knot, it is possible to reason on the display as one might manipulate the relevant object or objects. One does not, in that case, reason *in* the system of signs. What is required in the case of a system of signs within which to reason remains to be clarified. Our last distinction was that between picture proofs, which for these purposes include Euler and Venn diagrams, on the one hand, and Euclid's diagrams, on the other. What is characteristic of picture proofs, at least those of concern here, is that they formulate information in a way that allows one to see in the display both the information with which one began and that one aims to discover. Looked at one way, one sees the starting point, and looked at another, one sees the desired result. The display encodes the starting point in a way that makes the result implicit in that encoding; to make it explicit one needs only to shift one's visual focus a bit. As we will soon see, Euclidean diagrams are much more complex, and fruitful, than any such display.

2.2 Euclid's Constructions

Euclid's geometrical practice involves the use of diagrams in proofs. Indeed Euclid's demonstrations involve diagrams even in cases, such as those in Books VII to IX regarding numbers, in which they play no argumentative role. And not only are diagrams used in Euclid's demonstrations, very often what is to be demonstrated just is that some figure can be drawn, some construction effected. Propositions in Euclid, that is to say, are of two sorts: problems in which it is shown how to construct something—say, an equilateral triangle on a given line (proposition I.1), or a point of bisection on a given line (proposition I.10)—and theorems in which it is shown that some claim, usually in the form of a generalized conditional, is true, for instance, that if two straight lines cut one another, they make the vertical angles equal to one another (proposition I.15). Furthermore, according to Proclus, head of the Academy in fifth-century Athens, such constructions are "rightly preliminary" to theorems. Euclid begins the *Elements*, for example, with three construction problems, among them that of constructing an equilateral triangle on a given straight line, and then turns, in proposition I.4, to a proof of a general theorem about triangles. Proclus explains why.

> Suppose someone, before these have been constructed, should say: "If two triangles have this attribute, they will necessarily also have that." Would it not be easy for anyone to meet this assertion with, "Do we know whether a triangle can be constructed at all?" . . . It is to forestall such objections that the author of the *Elements* has given us the construction of triangles . . . . these propositions are rightly preliminary [to the theorems about the congruence of triangles].[17]

Constructions must precede the demonstration of theorems about the entities constructed because otherwise it could be objected that we do not know whether the

[17] Quoted in Knorr (1983, 126).

objects involved can be constructed. But why is this an objection? What exactly is it that we do not know when we do not know how to construct a triangle? One very common answer is that we do not know, in that case, whether triangles exist.

It can seem obvious, at least to us, from the perspective of our current conceptions, that construction problems in Euclid serve as existence proofs, that they are included in order to show that the constructed object exists. Nevertheless, as Knorr and others have argued, the reading is anachronistic.[18] Although questions about existence did occasionally arise in Greek mathematics, "the thesis that the ancient constructions were intended as a form of existence proof accounts neither for the geometers' manner of treating problems of construction nor for their ways of handling issues of existence" (Knorr 1983, 135). For example, it is well known that many construction problems cannot be solved using only the resources provided by Euclid, among them, the problems of trisecting an angle and squaring a circle. The issue for the Greeks was not, however, whether trisecting an angle or squaring a circle is possible. It seemed to them obvious that the trisection or square existed; the problem was to find a method or procedure for constructing such a thing, "not to establish the *existence* of the solution (for that is simply assumed), but rather to discover the *manner* of its construction" (Knorr 1983, 140).[19] An illustrative analogy might be this. Suppose that you know, because you have counted them, that you have fifty-seven boxes, each containing twenty-three jars into each of which you have put exactly thirty-five olives. One would, in such a case, have no doubt that there is some number that is the total number of olives in all the boxes. Nevertheless, one would, in the absence of some means such as those provided in our examples above, be unable to *produce* the total number from the given information. (Of course one could just count the olives, but that is quite different from producing the number that is wanted from the given information.) Similarly, Greek mathematicians were in no doubt that there is, say, a square equal in area to a given circle despite having no way of producing, or constructing, that square from the circle given the resources Euclid provides.

As Knorr argues, construction problems do not function as existence proofs in Euclid. Might it nonetheless be the case that constructions are needed in the *course* of Euclidean demonstrations, whether of problems or of theorems, in order to show that the various entities required by the process of reasoning exist? Demonstrations in Euclid involve, in every case, both a *kataskeue* (construction of the diagram) and, following it, an *apodeixis* (proof).[20] Is the construction stage perhaps needed because,

[18] See Knorr (1983), also Lachterman (1989), and Harari (2003). Saito (2008, 808–9) also briefly discusses this issue.

[19] Knorr (1983, 132) further quotes Philoponus, who writes in his commentary on Aristotle's *Posterior Analytics* I 9 that "those who square the circle did not inquire whether it is possible that a square be equal to the circle, but by supposing that it can exist they thus tried to produce (*γεννᾶν*) a square equal to the circle." As Knorr also notes in this context, Eutocius took it to be obvious that there is a straight line that is equal in length to the circumference of a circle, despite the fact that no one knew how to construct such a line.

[20] The translations are standard (see Heath, 1956, vol. I, 129); nevertheless, they may be misleading. On the question of the translation of '*kataskeue*' in particular, see Harari (2003, 2), also Lachterman (1989, 57).

as Mueller (1981, 15) suggests, in a system such as Euclid's "the existence of one object is always inferred from the existence of another by means of a construction"? Friedman makes a very similar claim.

In Euclid existence assumptions are represented not via propositions formulated in modern quantificational logic but rather by constructive operations—generating lines, circles, and so on in geometry—which can then be iterated indefinitely. Such indefinite iteration of constructive operations takes the place, as it were, of our use of quantificational logic, and it is essentially different, moreover, from the inferential procedures of traditional syllogistic logic. (Friedman 1992, xiii–xiv)[21]

On this reading, Euclid's demonstrations, whether of problems or of theorems, are not strictly deductive, logically valid because they require the use of postulates, rules of construction, that cannot be formulated as axioms without the resources of the full polyadic predicate calculus. Lacking those resources, Euclid could not include among his axioms that, say, between any two points there is a third and therefore could not deduce the existence of the points and lines that are needed in the course of various proofs. The existence of such points and lines is instead established by constructions according to the rules laid out in the postulates. The constructions are needed because the logic is monadic; given a more powerful logic, one could reason deductively throughout.

On Friedman's account, Euclid's constructions serve to insure the existence of the points and lines that are needed in Euclid's demonstrations. Friedman explicitly connects the idea to the familiar objection that Euclid merely assumes, for example, in proposition I.1, that there exists a point of intersection where two lines cross. The complaint, Friedman suggests, is misguided insofar as Euclid does not *assume* that the relevant point exists but instead *constructs* it—or, as Friedman (1992, 61) says, "generates" it. Clearly, a point that is given at the crossing of two lines is not constructed in the sense in which, say, a triangle or even a point of bisection is constructed using Euclid's procedures; it could not be a *problem* of Euclidean geometry to construct a point at the intersection of two lines. That there is a point at the intersection of two lines is not something that could be demonstrated in a Euclidean construction because only a whole of parts in relation can be constructed and the point at the intersection of two lines is not a whole of parts in relation. Nor even is such a point *generated*, that is, produced, brought into existence, shown to exist, by the intersection of lines, as Friedman suggests. That two crossed lines have a point of intersection is not, and could not be, *justified* by the construction but must instead be presupposed by it. Euclid simply assumes without comment, perhaps in light of continuity considerations, that there is a point at the crossing of two lines (Knorr 1983, 133). Even within a construction, then, the aim cannot be to establish the existence of the points and lines that are needed.

[21] See also Friedman (1992, ch. 1).

Ancient Greek mathematical practice suggests that neither construction problems nor the constructions that are appealed to in the course of demonstrations in Euclid's *Elements* serve to establish the existence of something. Indeed, problems regarding existence do not in general have the significance in ancient thought that they would come to have in the modern period. As Burnyeat (1982, 32) argues,

> Greek philosophy is perfectly prepared to think that reality may be entirely different from what we ordinarily take it to be.... But all these philosophers, however radical their scrutiny of ordinary belief, leave untouched—indeed they rely upon—the notion that we are deceived or ignorant about *something*. There is a reality of some sort confronting us; we are in touch with something, even if this something, reality, is not at all what we think it to be.

Roughly speaking, whereas modern philosophy (beginning with Descartes) takes essence (what a thing is) to precede existence (that it is), for the ancients, it does not. One cannot, on the ancient view, ask what something is, for example, what a triangle is—what it is to be a triangle (its essence)—unless one knows that triangles exist. And this is because, for the ancients, existence and essence are inextricably combined: to be is to be something in particular, that is, some sort of thing, something with a nature, form, or essence; and conversely, only what is has an essence, a nature or form. Whereas we find it natural to distinguish logically between the question what it is to be, say, a triangle or a circle, and the question whether there exist in reality any triangles or circles, no such distinction is made in ancient Greek thought: "the notions of existence and predication, which we distinguish as two separate logical or linguistic functions, are conceived in Greek as two sides of a single coin" (Kahn 1981, 123).[22] Thought, on such a view, is inextricably related to something that exists independent of it; it is inevitably directed on things without the mind. Indeed, this is just what we should expect given the intentional directedness that is enabled by natural language. As we have already seen, natural language is by its nature object oriented, and because it is, it will take a radical transformation, a *metamorphosis* in our mode of intentional directedness, for it to come to be so much as *intelligible* that, as Descartes suggests, the world without the mind might not exist.[23]

Much as mathematicians, both ancient and modern, distinguish between knowing that something is true and being able to prove it, so ancient geometers distinguish between knowing that something exists and being able to construct it. Indeed, they seem to have been primarily interested in constructions—that is, in finding ways of producing geometrical figures of mathematical interest beginning only with circles, points, and lines—insofar as, Knorr (1983, 140) suggests, in some cases theorems were proven only because they were needed to enable constructions. According to

[22] See also Owen (1965).

[23] Although skepticism about the world without the mind might have been conceivable to the ancient Greeks as an abstract or academic possibility, as a kind of limit case of sensory illusion, it could not, for reasons that will become clear in Chapter 3, actually be made intelligible before Descartes' revolutionary discoveries in mathematics.

Knorr (1983, 139), the solution of construction problems "constitutes in effect what the ancients *mean* by mathematical knowledge." Any adequate account of ancient Greek diagrammatic practice must be able to make sense of this.[24]

2.3 Propaedeutic to the Practice

The *Elements* opens with a series of definitions, postulates, and common notions. These are not, we will see, the starting points for the mathematics to follow, not if by that we mean the first, foundational truths from which other truths will be derived. Instead, they function as a propaedeutic to the practice of geometry. They belong not to geometry itself but only to the antechamber of geometry. They provide a "preamble" to the actual work of mathematics (Burnyeat 2000, 23),[25] one that amounts to "a constitution for Euclid's subject matter" (Reed 1995, 21) and enables one thereby "to 'read' or interpret diagrams which relate the parts of figures to one another" (Reed 1995, 52).[26]

Consider, first, the definitions. In current mathematical practice, definitions are understood as stipulations that fix the meaning of some newly introduced sign by setting out in the primitive (and perhaps previously defined) signs of the language what the newly introduced sign is to mean. Such definitions function, as the axioms of the system do, to provide premises for inferences.[27] One defines, for instance, the concept of a group using various primitive notions, and then one proves theorems about groups on the basis of that definition. The primitive notions cannot, of course, be similarly defined; and yet they must be understood. Instead they are elucidated in prefatory remarks, that is, in remarks that belong not to the system itself, in which starting points are set out and inferences drawn from them, but in the preparation for setting out the starting points. Euclid's definitions, we will see, function as elucidations in this sense. They belong to the preparation rather than to the system itself.

[24] Grosholz (2007), section 2.1.1, also emphasizes the importance of, as she puts it, the analysis of intelligible things in Euclid, "what makes a shape the shape it is" (Grosholz 2007, 36), and highlights as well the fact that in such an analysis the whole is not reduced to its parts: "no part has a relation to another part that is not mediated by the whole to which they belong." Nevertheless, it is important to recognize that although regarded one way the whole is prior to the part, regarded another the part is intelligible independent of the whole. Because the side of a triangle is a line, it is intelligible independent of the triangle, despite the fact that regarded as a side it is intelligible only in relation to the triangle as a whole. Such a whole is, in other words, neither an essential unity, as a living body is, the parts intelligible *only* in relation to the whole, nor an accidental unity, the whole *reducible* to the parts in relation. It is an *intelligible unity* of parts within the whole, neither of which is prior to the other.

[25] As Netz (1999, 95) puts it for the case of Euclid's definitions in particular, they are "simply part of the introductory prose... Before getting down to work, the mathematician describes what he is doing—that's all."

[26] Reed (1995, ch. 1) explains in detail what this might mean.

[27] Perhaps it will be objected that definitions are merely stipulations about abbreviations, that they have no essential role to play in the reasoning analogous to that played by the axioms. This is the standard view of philosophers. It is also, we will eventually see, mistaken.

As we now understand them, definitions concern words, or other signs; a definition sets out the meaning of a word or newly introduced sign. According to the ancient Greeks, definitions are not of words (or signs) but of what words name. That is, as Aristotle explains in the *Metaphysics* VII.4, "we have a definition not where we have a word and a formula identical in meaning . . . but where there is a formula of something primary [i.e., substance]" (1030a7–10).[28] "Only substance is definable" (1031a1). And only substance is definable because, as Aristotle explains in *Posterior Analytics* II.3, "definition is of the essential nature or being of something" (90b30); "definition reveals essential nature" (91a1). On Aristotle's view, which is that afforded by natural language, what most fundamentally exist are substances, that is, beings with natures, paradigmatically, living things. Again, to be is to be some particular sort of thing, something with a nature. Definitions as understood by the ancient Greeks set out what it is to be for such beings.

It is furthermore clear, as already indicated, that on Aristotle's view only what exists has such a nature: "he who knows what human—or any other—nature is, must know also that men exist; for no one knows the nature of what does not exist—one can know the meaning of the phrase or name 'goat-stag' but not what the essential nature of a goat-stag is" (*Posterior Analytics* 92b4–7). We know that nothing is a goat-stag because such a thing would have to be at once a goat and not a goat (because a stag) and nothing could be that. Because there is (demonstrably) nothing that can correctly be called a goat-stag, the term 'goat-stag' is not really a *name*. And hence, although the word has a perfectly clear meaning, no definition can be given of a goat-stag. Existence is in this way a precondition of essence, and hence of definition. But if so, then the definitions with which Euclid begins are not of words and do not leave it open whether the defined entities exist. Their existence is presupposed. What the definitions provide is only an account of what it is to be this or that geometrical entity, that is, their essential natures.[29]

According to Aristotle, definition is of the essential nature of (existing) things. It sets out what a thing most fundamentally is, its nature. In the case of a living being nature is given by reference to a form of life, the form of life of a rational animal, say. In the case of the elements, that is, earth, air, fire, and water, nature is given by certain sensory properties, the hot and the cold, and the wet and the dry: earth is cold and dry, fire hot and dry, and so on. In the cases of concern to Euclid, nature is given by parts in relation. In these cases, then, the definitions set out what parts in relation are required if something is to be a particular geometrical object, or a part thereof, what parts in relation are required for a particular property or relation of geometrical

[28] See also Le Blond (1979).

[29] This is not to say that there were not dialectical challenges to the idea that the objects of mathematics exist. There were. (See, for instance, Mueller 1982.) Such challenges were, however, of no concern to the practicing mathematician. And of course there were known cases in which alleged mathematical entities, such as the numerical ratio of the diagonal to the side of a square, demonstrably do not exist. These were, however, exceptions to the rule in Greek mathematics.

entities to obtain. We are told, for example, that a triangle is a rectilineal figure contained by three lines, rectilineal, figure, and line all having previously been defined. Similarly we are told that a number is a multiple composed of units, a unit having been previously defined as that in accordance with which each of the things that exist is called one. Definitions in terms of relations of parts are given of properties: we are told, for example, what it is for a line to be straight (it is a line that lies evenly with the points on itself), and for a number to be prime (it is a number that is measured only by a unit). And definitions in terms of relations of parts are provided, finally, of what it is to bear some particular relation, for instance, of magnitudes what it is to have a ratio one to another (just in case they are capable, when multiplied, of exceeding one another), or of circles what it is to touch one another (just in case they meet but do not cut one another). Because it is in the nature of such entities to be wholes of parts in this way, these sorts of things are, as Kant was perhaps the first to note explicitly, constitutively such as to be iconically representable. Drawings of geometrical entities "show in their composition the constituent concepts of which the whole idea . . . consists" (Kant 1764, 251; AK 2:278). The drawings like the things drawn are wholes that are made up of parts in relation. As Kant also sees, such wholes can then further be combined to show "in their combinations the relations of the . . . thoughts to each other" (Kant 1764, 251; AK 2:278).[30] In geometry—indeed, Kant thinks, in mathematics generally—one combines in a diagram the wholes that are created out of simples into larger wholes that exhibit relations among them. We will return to this.

In Book I of the *Elements*, after the definitions have been set out, Euclid lists some postulates and common notions. We know already from the definitions that, for example, the extremities of a line are points; now it is postulated "to draw a straight line from any point to any point." We know already that when a straight line is set up on a straight line so as to make the adjacent angles equal then those equal angles are right; now it is postulated that all right angles are equal to one another. And we know already that parallel lines are straight lines in a plane that never meet no matter how much they are extended; now it is postulated that if the interior angles on the same side of a transverse line are both acute, that is, less than a right angle, then the two lines crossed by the transverse line will meet if extended far enough. It is also postulated that a straight line can be extended and a circle described given a center and distance. The common notions then set out fundamental features of equality, for instance, that things equal to the same are equal, that things coinciding with one another are equal, and that the whole is greater than the part. What is at issue is whether these postulates and common notions function as premises *from which* to reason or instead as rules or principles *according to which* to reason.

[30] Kant is in fact describing what the words of natural language that are used in philosophy cannot do. It is clear that he means indirectly to say what the marks used in mathematics can do.

In an axiomatic system, a list of axioms is provided (perhaps along with an explicitly stated rule or rules of inference) on the basis of which to deduce theorems. Axioms are judgments furnishing premises for inferences. In a natural deduction system one is provided not with axioms but instead with a variety of rules of inference governing the sorts of inferential moves from premises to conclusions that are legitimate in the system. In natural deduction, one must furnish the premises oneself; the rules only tell you how to go on. The question whether Euclid's system is an axiomatic system or not is, then, a question about how the postulates, and common notions that are laid out in advance of Euclid's demonstrations actually function, whether as *premises* or as *rules* of construction and inference. Do they function to provide starting points for reasoning or do they instead govern one's passage, in the *kataskeue*, from one construction to another, and in the *apodeixis*, from one judgment to another? Inspection of the *Elements* strongly suggests the latter. In Euclid's demonstrations, the common notions and postulates are not treated as premises; instead they function, albeit only implicitly, as rules constraining what may be drawn in a diagram and what may be inferred given that something is true.[31] They provide the rules of the game, not its opening positions.

Consider, for example, the first three postulates. They govern what can be drawn in the course of constructing a diagram: (1) if you have two points then a line (and only one) may be produced with the two points as endpoints; (2) a finite line may be continued; and (3) if you have a point and a line segment or distance then a circle may be produced with that point as center and that distance as radius. In each case, one's starting point—points and lines—must be supplied from elsewhere in order for the postulate to be applied. And nothing can be done in constructing a diagram, at least at first, that is not allowed by one of these postulates. But once they have been demonstrated in problems, various other rules of construction can be used as well. For instance, once it has been shown, using circles, lines, and points, that an equilateral triangle can be constructed on a given finite straight line (proposition I.1), one may in subsequent constructions immediately draw an equilateral triangle, without any intermediate steps or constructions, provided that one has the appropriate line segment. Propositions such as I.1 that solve construction problems function in Euclid's practice as derived rules of construction. Once they have been demonstrated, they can be used in the construction of diagrams just like the postulates themselves, as rules governing those constructions.

Euclid's common notions, and again most obviously the first three, also govern moves one can make in the course of a demonstration, in this case in the course of the *apodeixis*. They govern what may be inferred: (1) if two things are both equal to a third then it can be inferred that they are equal to one another; (2) if equals are added to equals then it follows that the wholes are equal; (3) if equals be subtracted from equals,

[31] As we will see, Euclid in fact almost never invokes his definitions, postulates, and common notions in the course of a demonstration. They are nevertheless readily identifiable as warranting the moves that are made.

then the remainders are equal.[32] These common notions manifestly have the form of generalized conditionals; that is, they have the form of rules of inference.[33] Furthermore, in this case as well, theorems, once demonstrated, can function in subsequent demonstrations as derived rules of inference. Once it has been established that, say, the Pythagorean theorem is true (I.47), one may henceforth infer directly from something's being a right triangle that the square on the hypotenuse is equal to the sum of the squares on the sides containing the right angle. Euclid's *Elements* provides in this way the elements, the rules of construction (in its demonstrations of problems) and inference (in its demonstrations of theorems), for more advanced mathematical work.

But not everything that happens in the course of a Euclidean demonstration is governed by a stated rule, whether primitive or derived. There are two sorts of cases. First, Euclid draws (explicitly or implicitly) various obviously valid inferences, such as that two things are not equal given that one is larger than the other, despite the fact that the rule governing the passage is nowhere explicitly stated. Because rules such as this do not belong to mathematics in particular, but are simply a part of our understanding of natural language, no special mention is made of them.[34] The second sort of case is more interesting. It is well known that in order to follow a demonstration in Euclid, one must read various things off the relevant diagrams. For example, as we have already seen, given two lines that intersect in a diagram, Euclid assumes that there is a point at their intersection. The point of intersection seems simply to "pop up" in the diagram as drawn, and is henceforth available to one in the course of one's reasoning.[35] This happens not once but twice in the very first proposition of Book I of the *Elements*, to construct on a given finite straight line an equilateral triangle.

The demonstration begins with the *ekthesis* (setting out): let there be a straight line, AB.[36] A statement of what is to be done follows: to construct an equilateral triangle on AB. Then the *kataskeue* is given:

> [C1.] With center A and distance AB let the circle BCD be described. [This is licensed by the third postulate, though Euclid does not mention this.]
> [C2.] With center B and distance BA let the circle ACE be described. [Again, the warrant for this, the third postulate, is not mentioned.]
> [C3.] From point C, in which the circles cut one another, to the points A, B let the straight lines CA, CB be joined. [This is implicitly licensed by the first postulate on the assumption that there is such a point C.]

[32] These are, of course, not formally valid rules of inference; they are instead what Sellars has taught us to call materially valid rules.

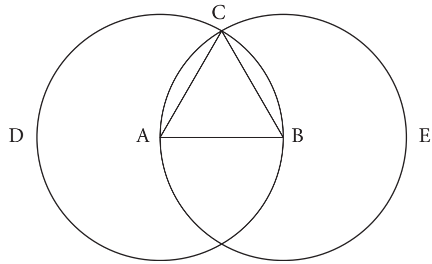
[33] As Ryle (1950) argues, rules of inference are inherently conditional in form and essentially general.

[34] Why, then, does Euclid include among his common notions that the whole is greater than the part? Is this not also something one learns as one learns the language one speaks? The answer may be that Zeno had by then rendered the relation of whole and part more problematic in mathematics than natural language usage might suggest.

[35] I borrow this use of the expression "pop up" from Kenneth Manders, to whom I am indebted for helping me to appreciate just how important is this feature of Euclidean diagrams. See Manders (1996) and (2008).

[36] I here follow Netz (1999, 43–4).

Figure 2.4 The diagram of proposition I.1 of Euclid's *Elements*.



The diagram that results is shown in Figure 2.4. And the *apodeixis* then follows:

[A1.] Given that A is the center of circle CDB, AC is equal to AB. [This is licensed, without mention, by the definition of a circle.]
[A2.] Given that B is the center of circle CAE, BC is equal to BA. [This again is implicitly licensed by the definition of a circle.]
[A3.] Given that AC equals AB and BC equals BA, we can infer that AC equals BC because what are equal to the same are equal to each other [that is, Common Notion 1].
[A4.] Given that AB, BC, and AC are equal to one another, the triangle ABC is equilateral. [This is warranted by the definition of equilateral triangle, on the assumption that there is such a triangle ABC.]
This triangle was constructed on the given finite straight line AB as required, and so we are done.

In the course of this demonstration, first a point pops up at the intersection of the two drawn circles, and then later a triangle pops up, formed from the radii of the two circles. This sort of thing is, furthermore, ubiquitous in ancient Greek geometrical practice. One simply reads the relevant geometrical objects off the diagrams, apparently without any explicitly stated warrant for doing so. Although nothing can be put into a diagram that is not licensed by one of the given postulates or by a previous construction, there seem to be no stated rules governing what, in the way of pop-up objects, can be taken out of it.[37]

We have seen that Euclid's definitions are not of words but of what words name, and that they function more like our elucidations of primitive expressions than like our definitions. And Euclid's common notions and postulates function, we have seen, essentially like rules of a natural deduction system. They are not axiomatic starting

[37] Could Euclid's definitions serve this purpose? Can it be inferred, for example, from the fact that a line is a breadthless length that at the cut of two lines there is a point (i.e., something that has no parts)? Perhaps, but more would need to be said.

points for inference but instead rules governing one's passage from the starting points given by the proposition to be demonstrated to the desired conclusion. But although both common notions and postulates are rules on this view, they are rules governing two very different sorts of activities. Postulates, at least the first three, govern constructions, what can be put into a diagram given the starting points provided by the proposition in question. Common notions instead govern one's reasoning in the course of the demonstration, what may be inferred given something one has already established. Problems and theorems function, respectively, as derived postulates and common notions so conceived. Once a problem has been demonstrated, the relevant construction can immediately be effected if one has the appropriate starting point; once a theorem has been demonstrated, the relevant inference can immediately be drawn given the relevant premise. But, as we also have seen, not everything that is inferred in the course of a Euclidean demonstration is governed by a rule. We have yet to understand the nature and legitimacy of pop-up objects in Euclid. We will return to them when we take up again the Kantian idea that in a diagram parts are related in wholes that are in turn combined in larger wholes.

2.4 Generality in Euclid's Demonstrations

Euclid's demonstration that an equilateral triangle can be drawn on a given finite straight line begins with a "setting out" (*ekthesis*): let there be a straight line, AB. And this is standard in Euclid's demonstrations. Although what is to be demonstrated is something wholly general, the demonstration proceeds, at least in many cases, by way of such a setting out. To anyone familiar with proofs in standard quantificational logic, it is very easy to take this setting out as an analogue of the rule of Universal Instantiation. In quantificational logic, in order to prove that (say) all A is C given that all A is B and that all B is C, one first turns to an instantiation of the premises in order that the rules of the propositional calculus may be applied. One reasons, in effect, about a particular case, and then at the end of the proof one takes what has been shown to apply generally on the grounds that no inference was drawn in the course of the proof that could not have been drawn were any other instance to have been considered instead. We tend to assume that demonstrations in Euclid work the same way.[38]

There is, however, little reason to think that the ancient Greeks understood generality as it is understood in quantificational logic.[39] Although they did

[38] We read, for instance in Russell (1908, 64) that "the general enunciation tells us something about (say) all triangles, while the particular enunciation takes one triangle and asserts the same thing of this one triangle. But the triangle taken is any triangle, not some one special triangle; and thus although, throughout the proof, only one triangle is dealt with the proof retains its generality." Netz (1999, 246) also takes this view: what is to be demonstrated is general but the demonstration itself (that is, the setting out, construction, and *apodeixis*) is particular; its generalizability is "a derivative of [its] repeatability." See also Netz (1999, 262).

[39] The notion of a quantifier, and more generally the quantificational conception of generality, are explored in some detail in section 6.1.

distinguish between general sentences about all or some objects of some sort, and sentences about particular objects, this distinction was not for them, as it is for us, a *logical* distinction. Aristotle's logic reflects this. It is a term logic in which no logical distinction is drawn between referring and predicative expressions (required for quantificational logic's distinction between singular and general sentences). Terms as Aristotle understands them have both referential and predicative aspects; they are essentially object involving (unlike the predicative expressions of standard logic), and also predicative (as referring expressions, as conceived in standard logic, are not). Terms in Aristotle's logic are, as we might think of it, what things are *called*, for instance, 'Socrates', 'man', 'snub-nosed', and so on. It is for precisely this reason that it is legitimate in traditional logic, though not in standard quantificational logic, to infer, on the basis of the fact that all (no) S is P, that some S is (not) P. Again, the ancients did recognize terms, such as 'goat-stag', that nothing is called, but such terms are, for just that reason, not really names. (There are no judgments that can be made about goat-stags.) Ancient Greek thought is inherently world directed; lacking the essentially modern notion of a concept as predicative rather than referential, the Greeks had no way of asking, in general, whether anything answering to one's concepts exists, no way of calling into question the existence of the "external" world.

According to the quantificational reading, a Euclidean demonstration proves something general by proving it for the particular instance that is introduced in the setting out portion of the text with which the demonstration begins. The setting out is essential on this reading insofar as the only way to prove something general in quantificational logic is by way of an instantiation. The diagram, as contrasted with the (textual) setting out, is not required in the same way. The diagram is needed, when it is, only to ensure the existence of the various points, lines, and so on, that are required for the cogency of the course of reasoning. And yet, in the *Elements*, diagrams are invariably included even in cases in which no construction is carried out on them, as in the demonstrations regarding numbers. Proposition VII.4, that "any number is either a part or parts of any number, the less of the greater," is a case in point. The text of the demonstration is this (Euclid 1956, I, 303):

> Let A, BC be two numbers, and let BC be the less; I say that BC is either a part, or parts, [i.e., a submultiple, or proper fraction] of A. For A, BC are either prime to one another or not. First, let A, BC be prime to one another. Then, if BC be divided into the units in it, each unit of those in BC will be in some part of A; so that BC is parts of A. Next, let A, BC not be prime to one another; then BC either measures, or does not measure, A. If now BC measures A, BC is a part of A. But, if not, let the greatest common measure D of A, BC be taken; and let BC be divided into the numbers equal to D, namely BE, EF, FC. Now, since D measures A, D is a part of A. But D is equal to each of the numbers BE, EF, FC; therefore each of the numbers BE, EF, FC is also a part of A; so that BC is parts of A. Therefore, etc. Q.E.D.

The "diagram" that is provided is shown in Figure 2.5. There is in this case no construction beyond the diagrammatic "setting out" of the numbers A, BC, and D;

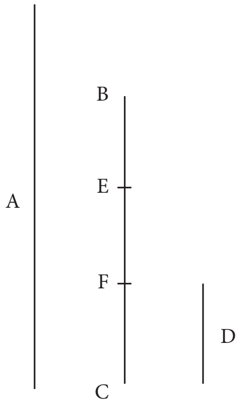


Figure 2.5 The diagram of proposition VII.4 of Euclid's *Elements*.

nothing further is added to the diagram, and nothing is inferred on the basis of it. And yet the diagram is included in this, and in every other, case.

Diagrams are inevitably included in Euclid's demonstrations, even in cases in which they seem to play no role in the demonstration. Surprisingly enough, at least from the quantificational perspective, the setting out is *not* invariably included. In some demonstrations, there is simply no moment of "universal instantiation." In proposition IV.10, for example, there cannot be any such moment because nothing is given as the basis on which to carry out the needed construction. One is merely to construct, out of thin air as it were, an isosceles triangle of a certain sort. The construction is given, and the fact that the triangle so constructed meets the requirement set is shown. But there is no logical moment of setting out; one simply starts with a drawn line and goes from there.[40]

A Euclidean demonstration apparently does not need to have a setting out; but it must have a diagram, and it must have one even in cases in which the diagram seems to serve no purpose in the demonstration. Both features of such demonstrations are anomalous on a quantificational interpretation of them; on the quantificational interpretation it is the setting out, universal instantiation, that is essential and the diagram that is optional, needed only in certain cases. It is furthermore worth noting that in Aristotle's term logic one infers *directly* from the fact that all A is B together with the fact that all B is C to the conclusion that all A is C. The reasoning, that is to say, is general throughout, though not quantificationally general. Perhaps, then, the same is true in the case of reasoning in Euclid. Grice's analysis, in "Meaning" (1957),

[40] Mendell (1998, 179) notes this case and others like it. A different sort of case in which the setting out is absent, noted also by Mendell, is that in which it is to be shown that an object lacks a certain property, for instance, that a circle does not cut a circle at more than two points (III.10). Because the reasoning in this case is by reductio, there is no setting out; one just starts with the needed construction, and thereby with the supposition that a circle can cut a circle at more than two points. This example will be discussed in greater detail in section 2.5.

of the distinction between (as Grice puts it) natural and non-natural meaning can help us to understand how this might work.[41]

Grice points out that there is an intuitively clear distinction between, for example, the sense in which a certain sort of spot on one's skin can mean measles and the sense in which three rings on the bell of a bus can mean that the bus is full.[42] The task is to provide an analysis of the difference between the two senses of 'means', natural and non-natural, respectively; and one of Grice's examples to that end is particularly revealing for our purposes. Grice (1957, 282–3) asks us to compare the following two cases:

(1) I show Mr. X a photograph of Mr. Y displaying undue familiarity with Mrs. X.
(2) I draw a picture of Mr. Y behaving in this manner and show it to Mr. X.

He immediately remarks:

> I find that I want to deny that in (1) the photograph (or my showing it to Mr. X) meant$_{NN}$ [that is, non-naturally] anything at all; while I want to assert that in (2) the picture (or my drawing and showing it) meant$_{NN}$ something (that Mr. Y had been unduly familiar), or at least that I had meant$_{NN}$ by it that Mr. Y had been unduly familiar. What is the difference between the two cases? Surely that in case (1) Mr. X's recognition of my intention to make him believe that there is something between Mr. Y and Mrs. X is (more or less) irrelevant to the production of this effect by the photograph.... But it will make a difference to the effect of my picture on Mr. X whether or not he takes me to be intending to inform him (make him believe something) about Mrs. X, and not to be just doodling or trying to produce a work of art.

Although the photograph can serve to convey to someone the fact that Mr. Y is unduly familiar with Mrs. X independent of anyone's intending that it so serve, the drawing cannot. To take the drawing as conveying a message about Mr. Y's behavior, rather than as a mere doodle or as a work of art, essentially involves taking it that someone produced it with the intention of conveying such a message, and that that person did so with the intention that that intention be recognized (and play a role in the communicative act). Only in that case does the drawing mean (non-naturally) that Mr. Y is unduly familiar with Mrs. X.

A photograph has natural meaning in virtue of a (causally induced) resemblance between the image in the photograph and that of which it is a photograph. A drawing, Grice argues, can in certain circumstances have instead non-natural meaning. That is, it can have the meaning or content that it does in virtue of one's intending that it have that meaning or content (and intending that that intention be recognized and play a certain role in the communicative act). So, we can ask, does a drawn figure in Euclid, a triangle, say, have Gricean natural meaning or instead

[41] See also Dipert (1996).
[42] In the opening paragraphs of his essay, Grice lists five different ways that the use of 'means' differs in the two cases.

Gricean non-natural meaning?[43] If it is a drawing of an instance, a particular geometrical figure, then it has natural meaning. It is in that case a semantic counterpart.[44] It is the thing, say, the particular triangle ABC, that is referred to in the text of a demonstration when one judges of triangle ABC that it is thus and so. But perhaps the drawing, like Grice's drawing, instead has non-natural meaning. Perhaps it is not (say) a circle but instead, as Leibniz (1969, 84) says, is taken for one.[45] If so, then the Euclidean diagram can mean or signify some particular sort of geometrical entity only in virtue of someone's intending that it do so and intending that that intention be recognized. One's intention in making the drawing—an intention that can be seen to be expressed in the setting out (in those cases in which there is one) and throughout the course of the *kataskeue*—is, in that case, indispensable to the diagram's playing the role it is to play in a Euclidean demonstration.

A Euclidean diagram, more exactly, a figure that is discerned within such a diagram, can be interpreted either as having natural or as having non-natural meaning in Grice's sense. If it has natural meaning then it does so in virtue of being an instance of a geometrical figure. But if it has non-natural meaning then it can be inherently general, a drawing of, say, an angle without being a drawing of any angle in particular. The geometer draws some lines to form a rectilineal angle, in order, say, to show that an angle can be bisected. The geometer does not mean or intend to draw an angle that is right, or obtuse, or acute. He means or intends merely to draw *an angle*. Hence he says, for example, in proposition I.9, that the angle he draws is "the given rectilineal angle," not also that it is right, or obtuse, or acute. That which he draws, regarded as something having natural meaning, will necessarily be right, or acute, or obtuse; but regarded as having non-natural meaning, it will be neither right nor acute nor obtuse. It will simply be *an angle*.[46] One can no more infer on the basis of the drawing so conceived that the angle in question is (say) obtuse, based on how it looks, than in Grice's case one could infer (say) that Mr. Y has put on a little weight recently, based on how the drawing of him looks. A drawing of Mr. Y could have as its non-natural meaning that he has put on weight recently, just as in the context of a Euclidean demonstration a drawing of an angle can have as its non-natural meaning that it is (say) obtuse, but that would require, in both cases, that quite different intentions be in play.

[43] We need to distinguish here between the role of Gricean intentions in someone's coming to believe something and the role of such intentions in what is meant, whether or not it is believed. Because proofs in mathematics are self-standing, in the sense explicated in section 2.1, Gricean intentions play no role in one's coming to accept a mathematical proof. They can nonetheless play a role in what is meant. It is only the latter idea that is invoked here.

[44] The terminology is Manders' (1996, 391).

[45] Quoted in Manders (1996, 391).

[46] The use of lines in Books VII–IX for numbers reinforces the point. Had, say, collections of points or strokes been used instead, the generality of the demonstration would have been lost insofar as any collection of points or strokes must involve some number of points or strokes in particular. By using lines for numbers, it can be left unspecified what the unit is that measures the line. The line so understood gives a number but no number in particular. The demonstration is general throughout.

If the figures drawn in a Euclidean diagram have non-natural rather than natural meaning, then they can, by intention, be essentially general.[47] It seems furthermore clear that they would in that case function as icons in Peirce's sense, rather than as symbols or indices, because they would in some way *resemble* that which they signify.[48] But the resemblance is *not*, at least not merely and not directly, a resemblance in appearance. As Peirce (1932, 159) notes, "many diagrams resemble their objects not at all in looks; it is only in respect to the relations of their parts that the likeness consists." We can, then, think of a drawn figure in Euclid as an icon that (though it may also resemble its object in appearance) signifies by way of a resemblance, or similarity, in the relations of parts, that is, in virtue of a homomorphism. On such an account a drawn circle serves as an icon of a geometrical circle not in virtue of any similarity in appearance between the two but because there is a likeness in the relationship of the parts of the drawing, specifically in the relation of the points on the drawn circumference to the drawn center, on the one hand, and the relation of the corresponding parts of the geometrical figure, a circle, on the other.[49]

A drawn circle is roughly circular; it looks like a circle just as a dog looks like a dog. But a dog looks like a dog because it *is* a dog, that is, a particular instance of dog nature. A drawn circle, I have suggested, can look like a circle for *either* of *two* reasons. It can look like a circle for the same reason that a dog looks like a dog, namely, because it is a circle, a particular instance of circle nature. Or it can look like a circle because it is an icon with non-natural meaning that is intended to resemble a circle first and foremost in the relation of its parts. Because what it is an icon of is circle nature, and because what is essential to a circle's being a circle is (as Euclid's definition reveals) that all points on the circumference are equidistant from the center, and it is this relationship of parts that is to be iconically represented, the icon itself comes to look roughly circular. The appearance of circularity is induced in this case by the intended higher order resemblance rather than being something that is there in any case (as circularity is there in any case in a drawing of a particular instance of a circle).

It is a familiar fact that Euclid never mentions any tools to be used in the constructions that are needed for his demonstrations. If drawings in Euclid provided

[47] Saito (2009, 821) makes a similar point using a numerical example, proposition IX.36.

[48] Peircean icons can have either natural or non-natural meaning. In particular, individual instances of geometrical figures can be icons of the relevant sorts of things; they have in that case natural meaning. A drawn circle regarded as an instance of a circle is an icon of a circle that has natural meaning because it so functions independent of anyone's intention that it do so. But a drawn circle can also function as an icon with non-natural meaning. In that case it can be essentially general, an icon of a circle not further specified.

[49] Consider also the fact that in the case of the geometry of three-dimensional objects what is needed is not a perspectival drawing but something more schematic, something "suggesting objective geometrical relations rather than subjective optical impressions" as Netz (1999, 17, n. 24), following Burnyeat, puts it. As Netz also remarks (1999, 18, n. 28), "Greek diagrams are . . . 'graphs' in the mathematical sense [though not in the sense of graph theory, as he notes (1999, 34)]. They are not drawings." Nevertheless, as already indicated, Netz himself does not seem to consider the possibility that the demonstration is constitutively general.

instances of various figures, however, this is just what one would expect.[50] If one wishes to draw a particular straight line then the best way to do that is with a straightedge, and similarly in the case of a circle: if one wants to draw a particular circle then one is best off using a compass. If, however, one's aim is to draw something with non-natural meaning, in particular, an icon of, say, a line or circle (that is, something essentially general, merely *a line* or *a circle*), then all that matters is that one's drawing is able to produce the desired effect, to convey one's intention. In this case there is no reason at all to mention some particular means of producing the drawing precisely because the drawing, to serve the purpose it is to serve, need not *look* very much like that for which it is an icon. So long as it serves the role it is intended to play then the resemblance (with respect to the relevant relations of parts) is good enough. This is not true of an instance: an instance ought as far as possible to look like what it is. It follows that instances are harder to draw than icons with non-natural meaning, and this is generally true. It is, for example, much easier to draw stick human figures and "smiley" faces, which are of course inherently general, than it is to draw someone in particular, the way some particular person actually looks. Even very small children can do the former; most of us even as adults cannot do the latter very well at all.[51] There is no need for mechanical aids in drawing Euclidean diagrams if those diagrams function as has been suggested here, as essentially general icons with non-natural meaning.

The idea that diagrams are iconic and general by intention can also explain why ancient Greek mathematicians almost never make the sorts of mistakes they would be expected to make if their reasoning were based instead on an instance. If one were reasoning about an instance, particularly about a drawn instance, it would be critical to distinguish carefully between what can and cannot be inferred on the basis of one's consideration of that instance. And this ought, in principle, to be quite difficult. It is, for instance, much harder to learn what a particular person looks like (so as to be able to re-identify that person) from a chance photograph than it is to find this out from, say, a caricature, because in the caricature the work of discovering what are the salient and characteristic features of the person's appearance has already been done. Similarly, if a Euclidean drawing were an instance (with natural meaning), it would be hard to distinguish between what Manders (1996, 392–3) has called co-exact features, ones on the basis of which inferences can be made, and exact features, which have no implications for one's course of reasoning.[52] But in fact, all the evidence suggests that this is not hard at all. As Mueller (1981, 5) remarks regarding the familiar diagram-based "proof" that all triangles are isosceles,

[50] Compare Lachterman (1989, 71).

[51] Vygotski (1978, 112) describes children's drawings as "graphic speech": "The schemes that distinguish children's first drawings are reminiscent in this sense of verbal concepts that communicate only the essential features of objects."

[52] See also the discussion in Greaves (2002, ch. 3).

perhaps a "pupil of Euclid" might stumble on such a proof; but probably he, and certainly an interested mathematician, would have no trouble in figuring out the fallacy on the basis of intuition and figures alone. And in the history of Euclidean geometry no such fallacious arguments are to be found. There are indeed many instances of tacit assumptions being made, but these assumptions were always true. In Euclidean geometry . . . precautions to avoid falsehood are really unnecessary.

This is unsurprising if the figures in the diagram function not as instances but as icons with non-natural meaning that is inherently general. The features that Manders identifies as exact are no more there in the diagram than information about the relative size of human limbs is there in a stick figure.[53]

We noted above that it is in the nature of the objects Euclid defines to have parts in relation and so to be iconically representable. Furthermore, we have seen, such icons can be essentially general, of the concepts themselves, what it is to be, say, a circle, as they contrast with instantiations of those concepts. Drawings of geometrical figures conceived as icons with non-natural meaning can resemble particular instances in appearance in virtue of the fact that there is a resemblance in the relation of the parts. A drawn circle, intended as an icon of a circle, can look very much like a particular instance of a circle. It is easy, then, based on such an appearance, to think that the drawing is such an instance, not such an icon at all. There is good reason nonetheless for thinking that in Euclidean demonstrations the diagram functions iconically (i.e., non-naturally) rather than to provide an instance about which to reason. First, as Manders (1996, 391) has noted, the latter idea "seems incompatible with the use of diagrams in proof by contradiction." To demonstrate, for instance, that a circle does not cut a circle at more than two points, one first sets out in a diagram that two circles do cut at more than two points, say, at four. This is not a situation that can obtain; there is no instance to be drawn. And yet the diagram is drawn—as we will see. If the diagram is read instead as a Peircean icon with non-natural meaning there is no difficulty. What the diagram means non-naturally, the content it exhibits, is exactly what one aims to show is impossible, that a circle cuts a circle at more than two points.

Nor is this the only sort of case in which it is impossible to draw an instance. We know, because Euclid tells us in the opening section of the *Elements*, that a point is that which has no parts, and that a line segment is a length that has no breadth (the extremities of which are points). Such entities are clearly not perceptible; there is nothing that a thing with no parts, or a length with no breadth, looks like. It follows directly that there is no way to draw an instance of either a point or a line. But the concepts of such things, and their relations one to another, can be iconically

[53] Kant offers an interesting variant on the idea in the first *Critique*. According to him, although the geometer constructs an instance (in pure intuition), the construction nonetheless enables a priori knowledge because nothing is ascribed to the figure "except what follows necessarily from what he [the geometer] himself put into it in accordance with its concept" (B xii). Although it is an instance that is drawn, according to Kant, it seems to be an arbitrary one insofar as it has only the properties that are common to all such entities. On the notion of an arbitrary object see Fine (1985).

represented. A drawn line length, for example, can formulate (be intended iconically to display) the content of the concept of a line with endpoints. A drawn dot can formulate (be intended iconically to display) the content of the concept *point*. A drawn circle is, again, a slightly different case because drawn circles do look roughly circular; that is, there is a look that geometrical circles can be said to have. But as in the case of Grice's drawing, the role of a drawn circle in the context of a Euclidean demonstration is not that of an instance but instead that of an icon with non-natural meaning, one that is intended to formulate the content of the concept *circle*, that is, the relation of the points on the circumference to the center, the fact that those points are equidistant from the center (whether or not in the figure as drawn the points on the circumference *look* equidistant from the center). All that is formulated in a Euclidean straight line (conceived as an icon) is a breadthless length lying evenly with the points on itself, that is, a certain relationship between the line and the points that may be found on it; and similarly, all that is formulated in a Euclidean circle (similarly conceived) is the relation of the points on its circumference to the center. In each case, what is important for the cogency of the demonstration is not what the figure looks like but instead what is intended by it whether as set out in Euclid's definitions and postulates, or as stipulated by the particular problem or theorem in question. The similarity in appearance (say, between the icon of a circle and an actual instance of something circular) does help to convey the intended meaning, just as it does in the example of Grice's drawing; nevertheless, what is meant is carried by the intention, not by the similarity in appearance.

A final indication that the object of investigation is not the individual drawn instance, which is a sensory object grasped in perception, is the fact that in the drawing of (say) the diagonal of a square, conceived as a drawing of an instance, the side and the diagonal of a square will not be incommensurable but instead commensurable. That is, as Mueller (1980, 115) has noted, that the diagonal of the square is incommensurable with its side is, in the case of any actually drawn instance, "always disconfirmed by careful measurement." Ancient Greek mathematicians do talk about their diagrams, but as Plato has Socrates remark in *Republic* (510d 7–8), "they do this *for the sake of* the square itself and the diagonal itself," that is, invariant being, however precisely the being of such being is to be understood.[54]

What one draws in Euclidean diagrams are not pictures or instances of geometrical objects but the relations that are constitutive of the various kinds of geometrical entities involved. A Euclidean diagram does not *instantiate* content but instead *formulates* it.[55] Obviously, then, the *ekthesis* does not provide an instance falling under a concept; the letters that it introduces are neither variables of quantification nor names of particular objects. They are nothing more than a means by which the various parts of the diagram can be referred to in the written text. Were the

[54] The translation is Burnyeat's in (1987, 219). [55] See Harari (2003).

demonstration live, that is, actually given by one geometer to another, no letters would be needed. One would in that case merely point as needed now to this bit of the diagram and now to that. The *sumperasma* in which the conclusion is stated is, correlatively, not inferred from a particular case. As Mueller (1974, 42) argues, although "in ancient logic the *sumperasma* is the conclusion of an argument," in Greek geometry, "the *sumperasma* is not so much a result of an inference as a summing up of what has been established." "The word *sumperasma* can . . . mean 'completion' or 'finish' . . . the *sumperasma* merely sums up what has taken place in the proof. . . . It merely completes the proof by summarizing what has been established" (Mueller 1981, 13). The move in a Euclidean demonstration from a claim involving letters to the explicitly general claim is not an application of the rule of universal generalization but instead a move from an implicit generality, such as that a horse is warm-blooded (inferred, say, from the claim that a horse is mammal together with the fact that mammals are warm-blooded), to an explicit generality, say, that all horses are warm-blooded. The demonstration is general throughout.

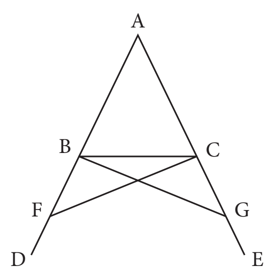
2.5 Diagrammatic Reasoning in the *Elements*

We have seen that Euclid's system is best thought of on analogy with a system of natural deduction as it contrasts with an axiomatization. And I have argued that figures in Euclidean diagrams do not provide instances but instead should be read as having (Gricean) non-natural meaning and to function, in particular, as icons formulating the contents of the concepts of concern to Euclid. Diagrams so conceived are constitutively general. What remains to be shown is that the reasoning involved in a Euclidean demonstration is not merely diagram-based but instead diagrammatic.

It is clearly true that in a Euclidean demonstration at least some steps in the chain of reasoning are in some way licensed by the diagram. Consider, for example, proposition I.5, that in isosceles triangles the angles at the base are equal to one another, and if the equal straight lines be produced further, the angles under the base will be equal to one another. The diagram is given in Figure 2.6.

It is then argued that because AF is equal to AG and AB to AC, the two sides FA, AC are equal to the two sides GA, AB, and they contain a common angle FAG. This

Figure 2.6 The diagram of proposition I.5 of Euclid's *Elements*.



inference, from the two equalities, AF to AG and AB to AC, to the double conclusion, first that the two sides FA, AC are equal to the two sides GA, AB, and also that both those same pairs of sides contain the angle FAG, might be thought to be diagram-based in the following sense. Having, first, drawn an isosceles triangle with sides AB and AC equal, and then having produced the straight lines BD and CE by extending (respectively) AB and AC, and having taken F on BD at random and cut AG off from AE to equal AF, one has thereby inevitably produced not only FA, AC equal to GA, AB, which is already more or less given, but also the angle FAG, which, as inspection of the diagram shows, is identical both to the angle FAC and to the angle GAB. It is no more possible to construct the required diagram without realizing this identity of angles than it is possible to embed one circle in a second circle and the second in a third without thereby embedding the first in the third—which is of course the principle behind Euler diagrams used to exhibit the validity of various valid syllogistic forms of reasoning, here *barbara*. Just as one can infer on the basis of an Euler diagram that some conclusion follows, so one can infer on the basis of the above diagram that the relevant claims are true. The diagram-based inference is, in both cases, valid, that is, truth-preserving, and it is truth-preserving precisely because there exists a homomorphism, a higher level formal identity, that relates relations among elements in the diagram with relations among the entities represented by those elements.

Reasoning using Euler diagrams provides a paradigm of what we might mean by diagram-based inference. In that case, one draws icons of things of a given sort, say, circles to represent collections of objects, and draws them in spatial relations that are homomorphic to relations of inclusion and exclusion over collections. If, for example, all dogs are mammals then one draws the circle representing the collection of dogs inside the circle representing the collection of mammals. If no fish are mammals then one draws the circle representing the collection of fish wholly separate from that representing the collection of mammals, without any overlap. Then one can read off the diagram what the relationship is between the collection of dogs and the collection of fish: the relevant icons are wholly disjoint so one infers that no dogs are fish. The inference, the passage from the premises about dogs, mammals, and fish, to the conclusion about dogs and fish is licensed by the diagram one draws. It is diagram-based. Do diagrams in Euclidean demonstrations function in the same way as the basis for an inference? We will see that although they can, in general they do not.

The first indication that diagrams in Euclid function differently from Euler diagrams is the fact that although Euler diagrams can make something that is implicit in one's premises explicit, they cannot in any way extend one's knowledge. Euclidean demonstrations, by contrast, do seem clearly to be fruitful, real extensions of our knowledge. And the reason they are is connected to the fact that objects can pop up in a Euclidean diagram. We find out that an equilateral triangle can be constructed on a straight line only because a triangle pops up in the course of the construction. Before that point there is nothing whatever about any triangles in anything we have to work with in the demonstration. Quite simply, there is no sense in which an equilateral triangle is *implicit* in a line, even given Euclid's axioms, postulates, and definitions. Nevertheless, as I.1 shows, such a triangle *is* there *potentially*: given what Euclid provides us, we can construct an equilateral triangle on a given straight line. The triangle just pops up when we draw certain lines. And this is true generally in ancient Greek mathematics: one achieves something new largely in virtue of this pop-up feature of its diagrams.[56] To understand the role of diagrams in Euclidean demonstrations, then, we need to understand these pop-up objects.

In a Euclidean demonstration, what is at first taken to be, say, a radius of a circle is later in the demonstration seen as a side of a triangle. But how *could* an icon of one thing become an icon of another? How could an icon of, say, a radius of a circle *turn into* an icon of a side of a triangle? Certainly nothing like this happens in the course of reasoning about an Euler diagram. The iconic significance of the drawn circles is fixed and unchanging throughout the course of reasoning. If the lines one draws in a Euclidean diagram had this same sort of iconic significance, the reasoning would clearly stall because in that case no new points or figures *could* pop up. It is also obvious that nothing could possibly be *both* an icon of (say) a radius of a circle and an icon of a side of a triangle at once, because these are incompatible. An icon of a radius essentially involves reference to a circle; radii are and must be radii of circles. An icon of a side of a triangle makes no reference to a circle. So nothing could at once be an icon of both. But one and the same thing *could serve* now (at time t) as an icon of a radius, and now (at time $t^* \neq t$) as an icon of a side. The familiar duck/rabbit drawing is just such a drawing; it is a drawing that is an icon of a duck (though of course no duck in particular) when viewed in one way and an icon of a rabbit (no one rabbit in particular) when viewed in another.

Some drawings, such as the duck/rabbit, can be seen in more than one way. It would seem, then, that we can say that in the diagram of proposition I.1 one sees certain lines now as icons of radii, as required to determine that they are equal in length, and now as icons of sides of a triangle, as required in order to draw the conclusion that one has drawn an equilateral triangle on a given straight line.[57] The cogency of the reasoning clearly requires both perspectives. But if it does then the Euclidean diagram is functioning in a way that is very different from the way an Euler diagram functions. Much as in the case of the duck/rabbit drawing, and by

[56] What then should we say about, for example, the ancient Greek demonstration that there is no largest prime? Here there is no diagram and there are no pop-up objects, and yet it seems right to say that the demonstration, like properly diagrammatic demonstrations in Euclid, extends our knowledge. As we will eventually see, understanding the ancient demonstration that there is no largest prime, which is a matter of deductive reasoning from concepts, can be understood only at the end of our story, in light of Frege's analysis of the nineteenth-century practice of reasoning from the contents of concepts. Only when mathematics came, in the nineteenth century, to be primarily a practice of deductive reasoning from concepts could the resources be developed that would enable us to understand such a practice.

[57] Euclid's definitions would seem to play a crucial role here: only if one can see in the diagram that the definition is satisfied can one find the relevant object in it.

contrast with an Euler diagram, various collections of lines and points in a Euclidean diagram are icons of, say, circles, or other particular sorts of geometrical figures, *only when viewed a certain way*, only when, as Kant would think of it, the manifold display (or a portion of it) is synthesized under some particular concept, say, that of a circle, or of a triangle. The point is not that the drawn lines underdetermine what is iconically represented, as if they had to be supplemented in some way. It is that the drawing as given, as certain marks on the page, has (intrinsically) the *potential* to be regarded in radically different ways (each of which is fully determinate albeit general, again, as in the duck/rabbit case). It is just this potential that is actualized in the course of reasoning, as one sees lines now as radii and now as sides of a triangle, and which begins to explain the enormous power of Euclidean diagrams as against the relative sterility of Euler diagrams.

In Euclidean diagrams, geometrical entities—points, lines, and figures—pop up as lines are added to the diagram. Cut a line AB with another line CD and up pops a point E as the point of intersection, as well as four new lines AE, BE, CE, and DE. Take the diagonal of a square and up pop two right triangles, and so on. We do not find this surprising in practice; indeed, one need have no self-conscious awareness that it is happening as one follows the course of a Euclidean demonstration. Nevertheless, as we have seen, it clearly *is* happening, and the cogency of the demonstration essentially depends on it. Such pop-up objects, I have suggested, depend in turn on our capacity perceptually to "re-gestalt" various collections of lines, to see them now one way and now another. And what this shows is that it is not the lines themselves that function as icons (even in light of one's intention that they be so regarded) but only the lines *when seen from a particular perspective*, when viewed one way rather than another equally possible way.

Consider again our two crossed lines AB and CD that cut at E, and suppose that they are functioning as icons independent of any perspective taken on them. One could then argue that the point E can belong to only one of the four segments AE, BE, CE, or DE, leaving the other three without an endpoint at one end. This would be a perfectly reasonable inference if we were dealing with a simple icon, one whose significance was fixed independent of any perspective taken on it, because it is true that if you divide a (dense) line by a point then that point can be the endpoint of only one of the two line segments, leaving the other to approach it indefinitely closely (because the line is dense), without having it as its endpoint. Suppose now that we offered this argument to an ancient Greek geometer. How would he respond? He would laugh us away, much as (according to Plato, *Republic* 525e) he laughs away those who would "[attempt] to cut up the 'one'" by pointing out that the line drawn to iconically represent the unit can surely be divided. The geometer laughs because one is in that case taking the diagram the wrong way, because one fails to understand how it works as a diagram in mathematical practice.

When two lines AB and CD cut at E, only one point pops up and it is the endpoint of all four lines AE, BE, CE, and DE, which also just pop up with the cut. And they can do because E is the endpoint of any one of these lines only relative to a way of regarding it, much as certain lines in the duck/rabbit drawing are ears, or a duckbill, only relative to a way of regarding those lines. Another example makes the same point. Suppose that I claimed on the basis of the drawing of a straight line segment ABCD that, contrary to what Euclid holds, two straight lines *can* have a common segment: AC and BD have BC in common. Again the geometer would laugh—and rightly so. Of course one can see the drawn line as iconically representing AC, or BD, or AD, or BC, and indeed one may need so to regard it in the course of a demonstration; but that does not show that the lines themselves have a common segment. This would be required only if the drawn line functioned iconically to represent these possibilities independent of any perspective that was taken on the drawing. Between two points there is only one straight line, not many; two lines can seem to have a common segment (as above) only if one, perversely, mistakes the way the diagram functions.

The lines in a Euclidean diagram, like the lines in a duck/rabbit drawing, function as icons of various sorts of geometrical objects only relative to a perspective that is taken on them. But Euclid's diagrams have a further feature as well, one that is not found either in the duck/rabbit drawing or in an Euler diagram: they have, as Kant already saw, three levels of articulation. Not only are icons of various geometrical figures constructed out of parts. Those icons are combined in turn in larger wholes. At the lowest level, then, are the primitive parts, namely, points, lines, angles, and areas, and their corresponding icons. At the second level are the (concepts of) geometrical objects we are interested in, those that form the subject matter of geometry, all of which are wholes of those primitive parts (and similarly for their icons). At this level we find points as endpoints of lines, as points of intersection of lines, and as centers of circles; we find angles of various sorts that are limited by lines that are also parts of those angles; and we find figures of various sorts. A drawn figure such as, say, a square has as parts: four straight line lengths, four points connecting them, four angles all of which are right, and the area that is bounded by those four lines. Of course, in the figure as actually drawn, the lines will not be truly straight or equal, and they will not meet at a point; the angles will not be right or all equal to one another. But this does not matter because the drawn square is not a picture or instance of a square but instead an icon of a square, one that formulates certain necessary properties of squares. At the third level, finally, is the whole diagram, which is not itself a geometrical figure but within which can be discerned various second-level objects depending on how one configures various collections of drawn lines within the diagram.

A Euclidean diagram is a whole of (intermediate) parts that are themselves wholes of (primitive) parts, and because it has this structure, one can reconceive parts of intermediate wholes in new wholes and discover thereby something new. It is furthermore this feature of the diagram that distinguishes it from simple picture proofs (such as that of the Pythagorean theorem given in section 2.1) and from representations of knots in knot theory and from Roman numerals by which to record how many. A Euclidean diagram is not merely a picture or record of something; instead it formulates the contents of concepts of various geometrical figures in a structured array that enables not merely reasoning *on* the diagram but instead reasoning *in* it. The three levels of articulation enable one to reconfigure parts of intermediate wholes into new (intermediate) wholes in the course of a Euclidean demonstration, and thereby demonstrate significant and often surprising geometrical truths.

We have seen that a Euclidean demonstration comprises a diagram and some text, and in particular, the *kataskeue* and the *apodeixis*. The *kataskeue* provides information about the construction of the diagram and is governed by what can be formulated in the diagram as legitimated by the postulates and any previously demonstrated problems. The *apodeixis*, which is governed by what can be read off the diagram as legitimated by the definitions, common notions, and previously demonstrated theorems, is generally taken to be the proof. This, as we will see in more detail below, is a mistake. The *apodeixis*, that is, that particular bit of the text in a (written) Euclidean demonstration, should be read, on our account, not as the proof but instead as a piece of text that provides instructions regarding how various portions of the constructed diagram are to be read, construed, or analyzed, that is, how they are to be carved up in the course of the demonstration. It is the *diagram* that is the site of reasoning, on our account, not the accompanying text.

Consider one last time the first demonstration in Euclid, that on a given straight line an equilateral triangle can be constructed. The diagram, once again, is shown in Figure 2.7. According to reasoning already rehearsed, we know that AC = AB because A is the center of circle CDB, and that BC = BA because B is the center of circle CAE, hence that AC = BC = AB, on the basis of which it is to be inferred that ABC is an equilateral triangle constructed on the given line AB, which was what we were required to show. Our task concerning this little chain of reasoning is to understand how, based on a claim about radii of circles, one might infer, even given the diagram, something about a triangle. Were the reasoning merely diagram-based, that is, were it reasoning *in* (natural) language but, at least in some cases, *justified by* what is depicted in the diagram then the problem seems intractable. No mere diagram of

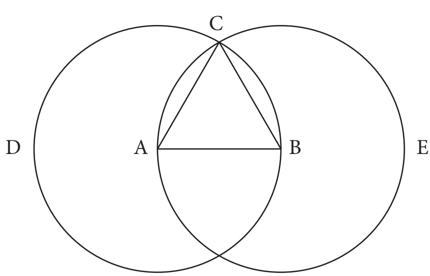


Figure 2.7 The diagram of proposition I.1 of Euclid's *Elements*.

some drawn circles, however iconic, can justify an inference from a claim about the radii of circles to a claim about a triangle. If, on the other hand, we take the diagram not merely as a collection of icons of various geometrical entities but instead as a display with the three levels of articulation outlined above, and so as a collection of lines various parts of which can be configured and reconfigured in a series of steps (as scripted by the *apodeixis*), the solution to our difficulty is obvious. One considers now one part of the diagram in some particular way and now another in some (perhaps incompatible, but perfectly legitimate) way as one makes one's passage to the conclusion.[58] In particular, a line that is at first taken iconically to represent a radius of a circle is later taken iconically to represent a side of a triangle. It is only because the drawing can be regarded in these different ways that one can determine that certain lines are equal in length, because they can be regarded as radii of a single circle, and then *conclude* that a certain triangle is equilateral, because its sides are equal in length. The demonstration is fruitful, a real extension of our knowledge, for just this reason: because we are able perceptually to take a part of one whole and combine it with a part of another whole to form an utterly new and hitherto unavailable whole, we are able to discover something that was simply not there, even implicitly, in the materials with which we began.

Consider now the Euclidean demonstration of proposition II.5: if a straight line be cut into equal and unequal segments, the rectangle contained by the unequal segments of the whole together with the square on the straight line between the points of section is equal to the square on the half.[59] The diagram, which we letter for ease of reference in our written discourse (were the presentation oral we could just point), is shown in Figure 2.8.

To understand this diagram as intended, that is, to know how to "read" it, how to configure its parts (at least to begin with), we need to know the provenance of its various parts, that is, the intention with which it is drawn. We are given that line AB is straight, and that it is cut into equal segments by C and into unequal segments by D. We know by the construction that CEFB is a square on CB, which we know from proposition I.46 is constructible, and that DG is a straight line parallel to CE and BF, KM parallel to AB and EF, and AK parallel to CL and BM, shown to be constructible in proposition I.31. That is, already we see here the various ways the drawn lines are taken to figure in various iconic representations of geometrical figures. CB, for example, is

[58] This idea, that a system of written marks might function to designate various entities only relative to a particular way of regarding it, will emerge again, most notably in our discussion of Frege's language *Begriffsschrift*. We can, then, put the point we have made here about how a Euclidean diagram functions in a demonstration in Fregean terms. Independent of a particular way of regarding collections of them, the drawn points lines, angles, and areas of a Euclidean diagram only express Fregean senses. It is only relative to a way of regarding a collection of them that the various signs designate anything.

[59] Szabó (1978, 334) persuasively argues that, though we might naturally interpret this theorem algebraically (see the second paragraph of Chapter 3), for the Greeks it "is a purely geometrical lemma needed for the solution of a purely geometrical problem . . . Proposition II.14 . . . to construct a square equal to a given rectilineal figure."

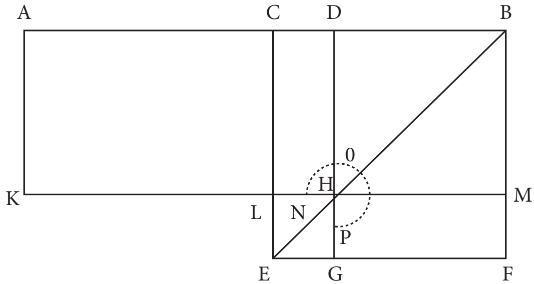
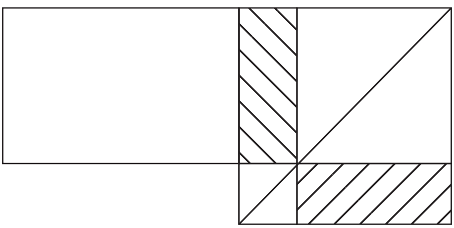


Figure 2.8 The diagram of proposition II.5 of Euclid's *Elements*.

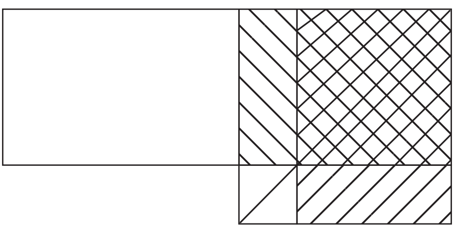
first taken to be the half of line AB, but then as a side of a square. BM is a line length equal in length to DB but also a part of the line BF, which is another side of that same square. It is in virtue of these various relations of parts iconically represented in the drawn diagram, and of these reconfigurings of parts, that the diagram can show that the theorem is true. It does so in a series of six steps scripted by the *apodeixis*.

1.



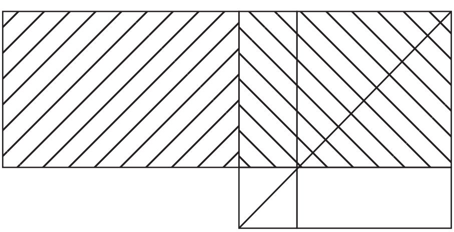
These differently shaded areas are equal, as is shown by prop. I.43: the complements of a parallelogram about the diameter are equal to one another.

2.



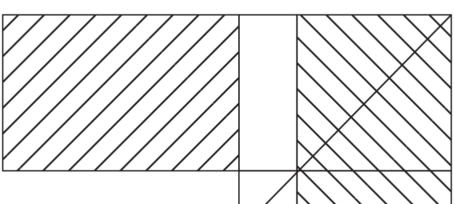
It follows from (1) that these areas are equal, because the same has been added to the same.

3.

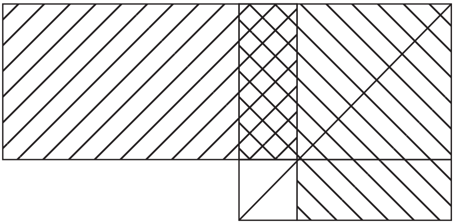


These areas are known to be equal on the basis of what we know about the relationships that obtain among the lines that form the boundaries of the two shaded areas.

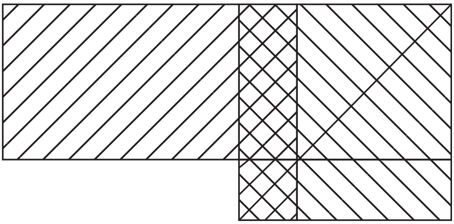
4.



It follows from (2) and (3) that these areas are equal because things equal to the same are equal to each other.

5. 

It follows from (4) that these areas are equal because the same has been added to equals.

6. 

It follows from (5) that these areas are equal because the same has been added to equals.

But that is just what we wanted to show: that the square on the half is equal to the rectangle contained by the unequal segments plus the square on the line between the points of section. By construing various aspects of the diagram in these various ways in the appropriate sequence one comes to see that the theorem is, indeed, must be, true. But if that is right, then (again) it is the diagram that is the site of reasoning in Euclid, not the text.[60] The text of the *apodeixis* is merely a script to guide one's words, and thereby one's thoughts, as one walks oneself through the demonstration in the diagram. Reasoning in Euclid is in this regard quite like doing a calculation in Arabic numeration, say, working out the product of thirty-seven and forty-two. In this latter case, one first writes the two Arabic numerals in a particular array, one directly above the other, then, in a series of familiar steps performs the usual calculation. As one writes down the appropriate numerals in the appropriate places, one may well talk to oneself ('let's see; two times seven is fourteen, so . . .'). Nevertheless, the calculation is not in the words one utters—if one does utter any words, even silently (and one need not)—but in the written notation itself. One calculates *in* Arabic numeration. In much the same sense, one reasons *in* a Euclidean diagram. The demonstration is not in the words of the *apodeixis*; it is in what those words help one to see in the diagram.[61] (This would perhaps be even more obvious if we considered someone

[60] Compare Peirce's remark (1976, 236) that "they [Greek writers] took it for granted that the reader would actively think; and the writer's sentences were to serve merely as so many blazes to enable him to follow the track of that writer's thought."

[61] That the words of the demonstration in Euclid are merely a record of the speech of someone presenting the demonstration to an audience is also indicated by the way the written text is presented in Euclid, and in ancient Greek generally: "unspaced, unpunctuated, unparagraphed, aided by no symbolism related to layout." "Script," in Euclid, "must be transformed into pre-written language" (Netz 1999, 163).

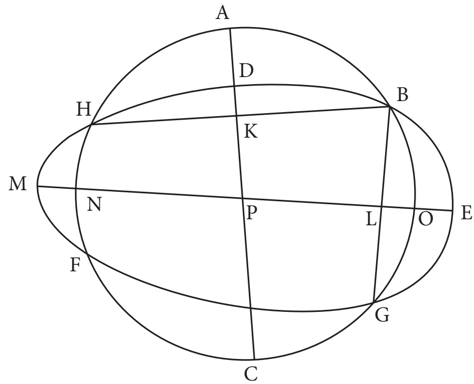
trying to discover a demonstration of some proposition. The inscribing would not be of words but of diagrams.)

As the demonstration of proposition II.5 shows, in order to follow (that is, to understand) a Euclidean demonstration, one must be able to see various drawn lines and points now as parts of one iconic figure and now as parts of another. The drawn lines are assigned very different significances at different stages in one's reasoning; they are *interpreted* differently depending on the context of lines they are taken, at a given stage in one's reasoning, to figure in. It is just this that explains the fruitfulness of a Euclidean demonstration, the fact that its conclusion is, as Kant would say, synthetic a priori. In Euclid, the desired conclusion is contained in the starting point, not merely implicitly, needing only to be made explicit (as in a deductive proof on the standard construal or in an Euler diagram), but instead only potentially. The potential of the starting point to yield the conclusion is made actual only through the construction of the diagram and the course of reasoning in it, that is, by a series of successive refigurings of what it is that is being iconically represented by various parts of the diagram. Parts of wholes must be perceptually taken apart and combined with parts of other wholes to make quite new wholes. And this is possible, first, in virtue of the three levels of articulation in the diagram, and also because the various parts of the diagram signify geometrical objects only relative to ways of regarding those parts. A given line must actually be construed now as an iconic representation of (say) a part of another line and now as an iconic representation of a side of a square, if the demonstration is to succeed. The diagram, more exactly, its proper parts, must be actualized, now as this iconic representation and now as that, through one's construal of them as such representations, if the result is to emerge from what is given. Only a course of constructing and thinking through the diagram can actualize the truth that it potentially contains.[62]

Another example, this time of the reductio proof of III.10, that a circle does not cut a circle at more points than two, reinforces the point. The diagram (again lettered to enable us to explain in the written text what it involves) is shown in Figure 2.15. The following is formulated iconically in the diagram. First, we have by hypothesis (for reductio) that the circle ABC cuts the circle DEF at the points B, G, F, and H; and here (again) it is especially obvious that we do not *picture* the hypothesized situation, which is of course impossible, but instead formulate in the diagram the (conceptual) content of that hypothesis. BH and BG are drawn (licensed by the first postulate) and are then bisected at K and L respectively. (We know that we can do this from I.10.) KC is then drawn at right angles to BH, and LM at right angles to BG. (We know from I.11 that we can do this.) Both KC and LM are extended, KC to A and LM to E, as permitted by the second postulate. What we have then are, first, two straight lines BH and BG, both of which are chords to both circles; that is, we can see BH as drawn

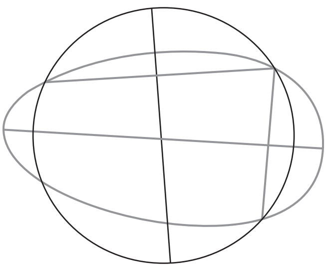
[62] We might, then, translate '*apodeixis*' not as 'proof' but as 'reasoning'.

Figure 2.9 The diagram of proposition III.10 of Euclid's *Elements*.



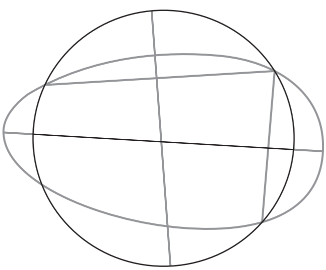
through ABC or as drawn through DEF, and similarly for BG. And we also know that AC bisects HB and is perpendicular to it, and similarly for ME and BG. Now the reasoning begins.

1.



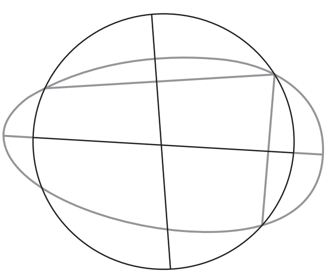
The center of the highlighted circle is on the highlighted bisecting line, by III.1 porism.

2.



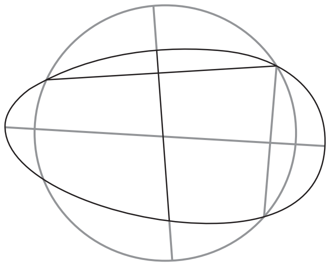
The center of the highlighted circle is on the highlighted bisecting line, again by III.1 porism.

3.



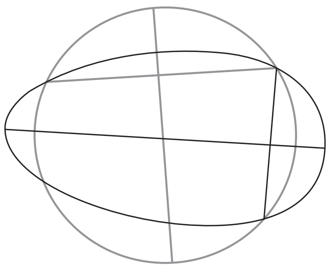
As the diagram shows, the two bisecting lines meet at only one point; so this point must be the center of the circle (because it is the only point that is on both lines).

4.



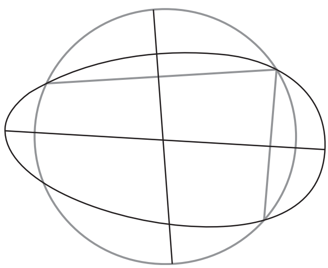
The center of the highlighted circle is on the highlighted bisecting line, by III.1 porism.

5.



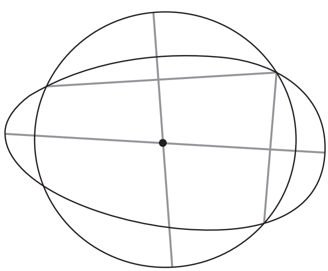
Again, the center of the highlighted circle is on the highlighted bisecting line, by III.1 porism.

6.



The two bisecting lines meet at only one point and so that point must be the center of the circle (because, again, that is the only point on both lines known to contain the center).

7.



The two circles have the same center because one and the same point that was shown first (3) to be the center of the circle ABC was shown also (6) to be the center of DEF.

But we know from III.5 that if two circles cut one another then they will not have the same center. Our hypothesis that a circle cuts a circle at four points is false. Furthermore, because the demonstration involves only three of the four points presumed to cut the two circles, it clearly is cogent no matter how many points one supposes two circles to cut at. Any number greater than two will lead to contradiction. It follows that a circle does not cut a circle at more points than two.

In this demonstration we are concerned with lines, circles, more exactly, the circumferences of circles, and their interrelationships. All that we assume about the various figures is what we are told about them in the construction, and all that we infer at each stage is what we know follows from what we know about the figures that are taken at that stage to be iconically represented in the relevant part of the diagram. By focusing on the various parts of the diagram, now this part and now that, and conceptualizing these parts appropriately, then applying what one knows to the relevant figure thereby iconically represented, one can easily come to realize that, given what is known and assumed, the two circles have one and the same center. But this is absurd given that they cut one another. The assumption that a circle can cut a circle at more than two points must be rejected. It is in just this way that one reasons by reductio in a Euclidean diagram, by formulating in a diagram what one aims to show is false and then showing by a chain of strict and rigorous reasoning in the diagram that that assumption leads to a contradiction.

We have seen that diagrams in Euclid are not merely images or instances of geometrical figures but are instead icons with Gricean non-natural meaning. As such, they are inherently general. But this alone is not sufficient to explain the workings of a Euclidean demonstration. In particular, Euclidean reasoning cannot be merely diagram-based, even where the diagram is conceived iconically, because if it were merely diagram-based, no account could be given of the pop-up objects that are essential to the cogency of Euclidean demonstrations. Euclidean reasoning is instead properly diagrammatic; one reasons *in* the diagram in Euclidean geometry, actualizing at each stage some potential of the diagram. This is furthermore made possible, we have seen, by the nature and structure of the signs that make up that system, and in particular by the fact that collections of signs can be seen now this way and now that within a diagram that has what we have described as three levels of articulation. Because a Euclidean diagram is a collection of primitive signs with three levels of articulation, the parts can be variously construed in systematic ways to mean, iconically represent, now this (sort of) geometrical figure and now that. The drawn diagram, then, is the site of reasoning but does not by itself give the desired result. The steps of the demonstration must actually be taken to realize that result. It is in just this sense that one reasons *in* the diagram in Euclid, that is, through lines, *dia grammon*, just as the ancient Greeks claimed. Not the words but "the construction... is the vehicle for the execution of the proof" (Knorr 1975, 74).[63]

2.6 Ancient Greek Philosophy of Mathematics

I have argued that diagrammatic demonstration in Euclid works in virtue of very distinctive features of the notation, in particular, the fact that there are three levels of

[63] See also Knorr's (1975, 72–4) discussion of various sources. As Netz (1999, 36, n. 61) has put this same point, "modern mathematicians prove with axioms; Greek mathematicians proved with lines."

articulation in the drawn diagram that enable various parts of the diagram to be regarded now as an icon of one geometrical figure and now as an icon of another. The diagram formulates content and does so in a way enabling reasoning in the system of signs. This reasoning is furthermore essentially general throughout. The diagram does not instantiate such content in an individual instance, but instead iconically exhibits *what it is to be*, say, a circle or triangle. The content that is formulated in the diagram is *conceptual* content. Nevertheless, because the intentionality of ancient thought is that enabled by natural language, and hence is constitutively object involving, ancient diagrammatic practice was understood by ancient Greek mathematicians and philosophers as similarly object involving. It was assumed that one was arguing about particulars of some sort.

This fact, that the intentionality of ancient Greek thought is constitutively object oriented, explains why we find diagrams even in those cases in which the diagram does no work, for instance, in the demonstration that there is no largest prime. Lines are drawn to be the numbers about which one reasons, and although the reasoning does not rely on the lines one draws, as it does in the cases we have discussed, still the lines are needed to give one objects about which to reason. These objects are not, in intention, particular numbers any more than the triangle one draws for the purposes of a demonstration is some particular triangle, but they are numbers and that about which one reasons. The lines give one a subject matter for one's demonstration. Again, one does not need a setting out in every case (though that is what one would expect if the generality involved in the demonstration were quantificational) but one does need a diagram in every case, even in those cases in which the reasoning is merely reported and is not diagrammatic (as in the demonstration that there is no largest prime), and this is explained by the fundamental object orientation of the intentionality of ancient Greek thought.

Mathematics is a science, and for the shape of spirit we find in the ancient Greeks this means that it is an inquiry into a domain of objects; insofar as it aims at the discovery of truths, mathematics aims at the discovery of truths about objects of various sorts. "All parties to the debate agree that mathematics is true. All parties are therefore committed to accepting that mathematicals exist. The dispute . . . is about their manner of existence" (Burnyeat 1987, 221). Furthermore, as Klein (1968) emphasizes, generality in the ancients' understanding of it is very different from the sort of generality that would come to characterize the essentially symbolic thinking of the early moderns. It is in terms of the *ancient* understanding of generality that we need to understand the philosophical problems they faced regarding the practice of mathematics as they knew it: "The problem of the 'general' applicability of method is therefore for the ancients the problem of the 'generality' (καθόλον) of the mathematical objects themselves, and this problem *they* can solve only on the basis of an ontology of mathematical objects" (Klein 1968, 122–3). The problem that divides Plato and Aristotle concerns the being of mathematical objects, the triangles, lines, and numbers that the mathematician studies. Mathematical truths clearly are not unqualifiedly true of everyday sensible objects. A drawn circle is not perfectly circular; the diagonal of a drawn unit square is not incommensurable with the side; and any physical object that is counted as one, a unit, can be divided. Physical objects do not have the mathematical properties and relations that are of concern to the mathematician, and do have properties, such as mutability and perishability, that proper objects of the understanding do not have. Furthermore, we know this; we know that the drawn circle is imperfectly circular. What is the nature of this knowledge? And what are the objects of mathematical science that will explain it? Are the objects of the science of mathematics ordinary sensory objects conceived in a particular way, as Aristotle thought, or are they peculiar, distinctively intelligible objects grasped with the eye of the mind, as Plato held?

According to Plato, mathematics, although needing to be completed by dialectic, is nonetheless a kind of paradigm of knowledge in large part because it is of what is, timeless and unchanging. It is through the study of mathematics that one's soul is first turned away from sensory things towards intelligible things, through the study of mathematics that one first comes to realize that the ordinary objects of everyday sensory experience and knowing cannot be the only or even the primary objects of knowledge there are. For, Plato argues, the being and intelligibility of the sensory realm of becoming is dependent on the being and intelligibility of the non-sensory realm of being. Our capacity to count sensory things, for instance, can be explained only by our prior and independent grasp of numbers as pure monads (Klein 1968, 71). Socrates in *Republic* tries to tell us a little about the nature of these other objects of knowledge—though of course he cannot say very much. What we learn is that they are intelligible rather than sensory, and that they depend in some way on the Forms. As to the Forms themselves we also know very little. The Form is that in virtue of which things appear similar, and it is by reflecting on the practice of mathematics that we come to expect that Forms are distinct from the sensible things of everyday experience. Drawn circles are roughly circular, none of them perfectly instantiating what it is to be a circle, namely, a plane figure all points on the circumference of which are equidistant from a center. Yet we do know what it is to be a circle, and what we grasp in such a case is, Plato thinks, the Form, Circle itself, something that is exactly what a circle is insofar as it is a circle, nothing more and nothing less.

In the imagery of the Line in *Republic* VI, Plato divides the intelligible realm into two sections. In the first, lower section one proceeds by means of images and assumptions; in the second, higher section one instead proceeds from assumptions to a "beginning or principle that transcends assumptions," making no use of images, "relying on ideas only" (510b). The first is the way of the mathematician, the second the way of the philosopher. That is, as Socrates explains,

> students of geometry and reckoning and such subjects first postulate the odd and the even and the various figures and three kinds of angles and other things akin to these in each branch of science, regard them as known, and, treating them as absolute assumptions, do not deign to

render any further account of them to themselves or others, taking it for granted that they are obvious to everybody. (510d)

And as Klein (1968, 73) points out, this "procedure by 'hypothesis' stressed by Plato is *not* a specifically 'scientific' method but the original attitude of human reflection prior to all science which is revealed directly in speech as it exhibits and judges things." The postulation that is involved in ancient mathematical practice is not the same as that we find in early modern scientific practice but is instead continuous with our everyday understanding, which always already knows a great deal about how things are and how they work.

Plato continues:

They take their start from these, and pursuing the inquiry from this point on consistently, conclude with that for the investigation of which they set out.... And... they further make use of the visible forms and talk about them, though they are not thinking of them but of those things of which they are a likeness, pursuing their inquiry for the sake of the square as such and the diagonal as such, and not for the sake of the image of it which they draw. And so in all cases. The very things which they mold and draw, which have shadows and images of themselves in water, these things they treat in their turn only as images, but what they really seek is to get sight of those realities which can be seen only by the mind. (510d)

Mathematicians use visible forms and talk about them; that is, they point to various parts of the drawn diagram, to lines, angles, areas, and figures, and say things about them, just as we have seen. But they are not, Plato holds, thinking about the drawn diagram "but of those things of which they are a likeness," of things that are more properly mathematical, things that can be grasped only by the mind's eye. And this notion of the mind's eye, of seeing with the mind, is no mere manner of speaking. As Frede (1987) explains, in Plato's later dialogues the verb '*aisthanesthai*' although usually translated as sense perception, often means more generally awareness of something, perhaps by way of sense perception but also perhaps by the mind, and the use of the verb in the latter case is not metaphorical:

It, rather, seems that all cases of becoming aware are understood and construed along the lines of the paradigm of seeing, exactly because one does not see a radical difference between the way the mind grasps something and the way the eyes see something. Both are supposed to involve some contact with the object by virtue of which, through a mechanism unknown to us, we become aware of it. (Frede 1987, 378)

Again, what we know first and foremost are not, for the ancient Greeks, facts (as would come to seem obvious with the advent of modernity) but objects. To know is to know things as they are, according to their natures, in some cases with the power of sight of the eyes and in others with the power of the sight of the mind.

These things that on Plato's account the mathematician thinks about, grasps with the eyes of the mind, are not themselves Forms. There is, for instance, only one Form Square, but in mathematical practice, we need to appeal to squares that are, however qualitatively similar, nonetheless numerically distinct. The point is perhaps easiest to appreciate in the case of numbers. According to the ancients numbers are collections, for instance, of eggs or boxes, or of pure units in the science of mathematics, collections that can be added to and subtracted from, taken many times, or divided. But for this one needs, for instance, many collections of five (given that five added to five is ten, that five times five is twenty-five, and so on). Each collection of five is five, but the different collections are numerically distinct. The Form is simply Five itself. The Five that is a Form is not a collection of units that can be combined with another number (collection of units) or divided into two and three. It is just Five. Exactly the same is true of triangles, circles, lines, and so on in geometry. To do geometry one needs to be able to call on many different squares, circles, and lines, all of which are ideal instances of Forms, but not themselves Forms. As Aristotle reports in *Metaphysics* I.6: "besides sensible things and Forms he [Plato] says there are the objects of mathematics, which occupy an intermediate position, differing from the sensible things in being eternal and unchangeable, and from Forms in that there are many alike, while the Form itself is in each case unique" (987b 14–18). One simply cannot do mathematics with Forms.[64] Nor, Plato seems to think, is the knowledge that results from mathematical inquiry knowledge about Forms, and for much the same reason. One cannot express mathematical knowledge by appeal only to Forms because, again, often one needs to invoke numerically distinct instances of one and the same Form. What one knows is, for instance, that a circle does not cut a circle at more than two points, and to know this, one might easily think, one needs to have in mind not one but two circles, however arbitrary, and not one but at least two (arbitrary) points.

Although Plato takes both the Forms and the mathematicals, that is, the ideal circles and points that are the objects of mathematical study, to exist separately from sensory objects, to be intelligible rather than sensory objects, Aristotle does not. According to Aristotle, mathematicians work with perceptible things but not as perceptible. Much as the perceptible property of a thing, say, the redness of the apple I see or the sound of the wind chime I hear, has no existence independent of the apple or sounding chime, so the triangularity of the triangle I draw or the fiveness of the fingers on my hand cannot exist independent of the drawn triangle or my hand (Klein 1968, 101). One studies, for instance, the drawn triangle but not *qua* drawn or perceptible; one studies it instead *qua* triangle. That is, as Aristotle explains in *de Anima* III.7:

> The so-called abstract objects the mind thinks just as, if one had thought of the snub-nosed not as snub-nosed but as hollow, one would have thought of an actuality without the flesh in which it is embodied: it is thus that the mind when it is thinking the objects of Mathematics thinks as

[64] See Burnyeat (1987, 229–36).

separate, elements which do not exist separate. In every case the mind which is actively thinking is the objects which it thinks. (431b 13–18)

One talks about (mathematical) triangles as if they were separable, indeed, as if they are separate and unchanging.[65] But in fact, according to Aristotle, they are not. As Burnyeat argues, this position that Aristotle defends is outlined, together with Plato's view, already in *Republic*.

What is at issue is, again, the ontological status of the objects of mathematical inquiry, and in the imagery of the cave, Burnyeat argues, we know both that the puppets on the wall, which are the second thing the prisoner sees (the first being shadows cast by those puppets), and the divine reflections, that is, the images that are the first to be seen outside the cave, are mathematicals.

But with the divine reflections, the escaped prisoner is apparently aware that he is looking at images of things that he cannot yet see directly (516ab); with the puppets, the opposite is the case (515d). Some awareness of a dependence of mathematicals on Forms is part of the decisive transition from inside to outside the cave. (Burnyeat 1987, 227)

To rest content with C2 [the puppets] would be to accept, as Aristotle does, that a philosophical account of mathematics need probe no further than the diagram on the page and what the practicing mathematician does with it. To move out of the cave to C3 [the divine reflections] is to come to regard mathematicals no longer as abstractions from the sensible world but as things which exist independent of it. (Burnyeat 1987, 229)

And here we come to an aporia that will dog the philosophy of mathematics for millennia. Plato is right: no adequate account of mathematical *truth* can be achieved unless we recognize that mathematicians in their practice somehow transcend the sensory world of ever-changing things. But so is Aristotle: no adequate account of mathematical *knowledge* can be achieved by talk of mathematicians transcending, in their practice, the sensory world of becoming. The problem of truth and knowledge in mathematics, made familiar recently by Benacerraf (1973), is as old as the practice of mathematics itself. It will finally be resolved only when, through the course of our intellectual history, the power of reason is fully realized as a power of knowing.

2.7 Conclusion

A Euclidean demonstration, as we have come to understand it, is not a proof in the standard sense; it is not a sequence of sentences some of which are premises and the rest of which follow in a sequence of steps that are deductively valid, or diagram-based. Indeed, the demonstration does not lie in *sentences* at all. The demonstration consists in a certain activity, in a course of reasoning that at least in some cases, those

[65] See Mueller (1979), Lear (1982), and Mendell (1998).

we have been concerned with here, is focused directly on a diagram. One does not merely *report* the reasoning, as one does in the demonstration that there is no largest prime, but nor does one display the reasoning as one does in a calculation in Arabic numeration. What is displayed in ancient Greek geometry is a Euclidean diagram that must be reasoned through in a series of steps scripted by the *apodeixis*.

One reasons *in* the diagram rather than merely *on* it in Euclidean diagrammatic practice. And one *can* reason in the diagram because the diagram does not merely picture some objects or some state of affairs, as, we saw, a Roman numeral or picture proof does. Because the diagram formulates the *contents of concepts* in the way that it does, namely, by combining primitive parts into wholes that are themselves parts of the diagram as a whole, various parts of the diagram can be conceived now this way and now that in an ordered series of steps. One does not in this case merely shift one's gaze in order to see explicitly something that was already implicit in the diagram as drawn, as is the case in an Euler or Venn diagram and in some picture proofs. Because what is displayed are the contents of concepts the parts of which can be recombined with parts of other concepts, something new can emerge that was not there even implicitly in that with which one began—though, of course, it had the potential to be discovered. Because parts of different wholes can be perceptually combined in new ways, the demonstration can reveal hitherto unknown relations among the figures of interest to Greek geometry.

Diagrams serve in ancient Greek geometrical practice as a medium of reasoning by enabling the display of the contents of the concepts of various sorts of geometrical figures as they matter to diagrammatic reasoning. To demonstrate a truth, or a construction, in Greek mathematical practice just is to find a diagram, constructed according to the rules set out in the postulates and any previously demonstrated problems, that provides a path from one's starting point to the desired endpoint. To discover such a diagram is to reveal a connection between concepts that is made possible by the definitions, postulates, and common notions that Euclid sets out but is not already there, even if only implicitly, in those definitions, postulates, and common notions. It is only the diagram, itself fully actualized as the diagram it is as one reasons through it, regarding aspects of it now this way and now that as scripted by the *apodeixis*, that actualizes the potential of Euclid's starting points to yield something new.

Proclus claims that problems are preliminary to theorems in geometry, that one needs to know that, say, a triangle can be constructed before one is in a position to know theorems about attributes of triangles. What Proclus does not explain is why that is, what precise role such constructions play in Greek geometrical practice. We now have an answer. Constructions encode or formulate information about the essential natures of geometrical entities in a form that is usable in diagrammatic reasoning. To know how to construct a particular figure is to know how to formulate the content of the concept of that figure, as set out in a definition, in a diagram; to know how to construct figures in Euclidean geometry is to be able to *display* the contents of mathematical concepts in written marks in a way that allows one to reason rigorously in the system of signs. Knowing how to construct something given Euclid's primitive resources is, in other words, quite like knowing how to write a number in Arabic numerals. It is to be able to formulate in a visual display content as it matters to inference. Much as Arabic numeration is a language within which to calculate in arithmetic, so constructions in Euclid provide a language within which to reason in geometry. Only what can be constructed in a Euclidean diagram can figure in demonstrated theorems precisely because and insofar as the diagram is the site of reasoning.

Because a construction displays content in a mathematically tractable way, in a way enabling mathematical reasoning regarding that content, to know how to construct a given figure is to be able to reason about such figures in Euclidean geometry. But that alone does not explain the full significance of constructions in ancient Greek geometry. First, much as an axiomatization sets out a small collection of primitive truths on the basis of which to derive all the other truths in some domain of knowledge, and a small collection of primitive concepts can provide the basis on which to define more complex concepts, so constructions show one how to produce complex geometrical figures on the basis of a few primitive ones, more exactly, how to display the contents of complex concepts given that one knows how to display the contents of primitive ones. And much as in the case of an axiomatization or definition, to have a construction of some complex figure given only a few primitives is to achieve an important kind of systematic knowledge. Furthermore, because these constructions display *what it is to be* a Euclidean geometrical figure, by exhibiting its parts in relation, and knowledge just is, for the ancient Greeks, of things as what they most essentially are, in their natures, to know how to construct some geometrical figure *is* to know it in its nature. It is for precisely this reason that, as Knorr (1983, 139) points out, knowledge of constructions "constitutes in effect what the ancients *mean* by mathematical knowledge."

Constructions are the starting point for reasoning in ancient diagrammatic practice. It is the construction, not the definition, that formulates the contents of the concepts of geometry in a mathematically tractable way, in a way enabling one to reason rigorously in the system of signs to conclusions about those concepts. And the construction can do this because it exhibits in a way that words cannot the natures of the figures involved, what it is to be an equilateral triangle, say, or the bisection of an angle. In sum, the language of Greek mathematical practice is not natural language but constructions—drawn Euclidean diagrams. It is by means of constructions, diagrams, that, as ancient Greek mathematicians realized, it is possible to discover and explore the myriad necessary relationships that obtain among geometrical concepts, from the most obvious to the very subtle. It was an extraordinary achievement, one that would not be surpassed for nearly two millennia.

3

A New World Order

Although it demonstrates a priori, timeless truths winning thereby the title of *mathesis*, ancient mathematical practice operates within the horizon afforded by natural language. The concepts of ancient mathematics are concepts of objects with their characteristic natures in virtue of which they have properties and relate to one another in discoverable ways. But not all concepts of interest to mathematicians are concepts of objects together with their properties and relations; and not all geometrical problems that mathematicians can formulate can be solved diagrammatically. In Descartes' *Geometry* (1637) mathematical practice is transformed through the achievement of a distinctively symbolic language, and the achievement thereby of both a new mode of intentional directedness and a new understanding of the reality we seek to know.[1] Our task is to understand the emergence of this new mathematical practice, the conception of being it embodies, and its relationship to Descartes' radically new metaphysics.[2]

Where an ancient Greek geometer demonstrates by means of a diagram, Descartes computes in the symbolic language of elementary algebra.[3] For example, Euclid uses a diagram to show that if a line be cut into equal and unequal segments then the rectangle contained by the unequal segments of the whole together with the square on the line between the points of section is equal to the square on the half.[4] Descartes, and we following him, would instead approach the problem algebraically. We are given a line AB that is cut into equal segments at C and unequal segments at D as shown in Figure 3.1. We first assign names to the three lengths, say, a to AC, b to CD, and c to DB. We know, then, that $a = b + c$; that is, we interpret the claim that a line is

[1] Here I reverse the order suggested by Klein (1968) insofar as he holds that it was the new mode of intentionality that made possible the new form of mathematics. Where I can and do wholly concur with Klein is in his claim (Klein 1968, 121) that "the nature of the modification which the mathematical science of the sixteenth and seventeenth century brings about in the conceptions of ancient mathematics is *exemplary* for the total design of human knowledge in later times"—or at least, we will see, until the nineteenth century.

[2] The discussion of the significance of Descartes' mathematics in this chapter has been helped not only by Klein (1968) but also by Lachterman (1989). A good introduction to the mathematics of Descartes' *Geometry* can be found in Mancosu (1992).

[3] This is an oversimplification but for present purposes close enough. Later we will see in some detail the nature of Descartes' mathematical practice.

[4] This is proposition II.5 of Euclid's *Elements*. We considered its demonstration in section 2.5.

Figure 3.1 A line cut into equal and unequal segments.

cut into equal and unequal segments as a claim about an arithmetical relationship between the lengths of the three segments that are generated by the two cuts. What is to be shown is similarly interpreted. The idea of a rectangle contained by the unequal segments is interpreted as $(a + b)c$; the square on the line between the points of section becomes b^2, and the square on the half is a^2. What is to be shown, then, is that $(a + b)c + b^2 = a^2$, given that $a = b + c$. This is easily done: simply replace all occurrences of 'a' in what is to be shown by '$b + c$', and "do the math," that is, perform the appropriate symbol manipulations until the expressions on both sides of the equal sign are identical.

But how is it that we come to interpret an expression such as "the rectangle contained by the unequal segments" as '$(a + b)c$'? A rectangle is an *object*, a geometrical figure with a nature and a characteristic look. How does such an object come to be transformed into or conceived as something expressible using the language of elementary algebra? As obvious and natural as it may seem to us, this use of symbols was not at all easy to achieve.[5]

As we have noted already, the symbolic languages of mathematics are quite unlike natural languages. Neither narrative nor sensory (at least not in the way natural language is sensory), they are special purpose instruments designed for particular purposes and useless for others. They are not constitutively social and historical, and they have no inherent tendency to change with use. Unlike natural languages, they also can (at least in some cases) be used merely mechanically, without understanding, used, that is, not as languages at all but as useful calculating devices. The positional Arabic numeration system, for example, was used merely as a tool, as a device for solving arithmetical problems, for centuries after its introduction around the tenth century. Although used throughout the first half of the second millennium as an alternative technology to the counting board in the performance of calculations, Arabic numeration was not at first used to record the results of calculations. Records were instead kept in Roman numeration. Only well into the sixteenth century did shopkeepers and merchants begin to keep their records in Arabic numeration; and universities and monasteries continued to use Roman numerals for record keeping even after that (Ball 1889, 7).[6]

[5] Compare this remark of Thom (1971, 74): "geometry is a natural and possibly irreplaceable intermediary between ordinary language and mathematical formalism, where each object is reduced to a symbol and the group of equivalences is reduced to the identity of the written symbol with itself. From this point of view the stage of geometric thought may be a stage that it is impossible to omit in the normal development of man's rational activity."

[6] For further discussion of the use of Roman and Arabic numeration in Europe in the first half of the second millennium see also Swetz (1987) and Chrisomalis (2009, 504).

The significance of this fact, that for centuries both merchants and academics were using paper-and-pencil calculations in Arabic numeration to solve arithmetical problems but would nonetheless record their results in Roman numeration, should not be underestimated. In particular, the fact that even merchants, who surely have little concern for theoretical questions about what numbers (really) are, engaged in such a time-consuming and apparently unmotivated practice, and not merely for a few generations but for centuries, cannot be set aside as a mere historical curiosity. Rather it suggests that they simply could not see Arabic numerals as signs for numbers at all.[7] While it is surely correct to distinguish between everyday practice with numbers, a practice that does not distinguish between the unit, one, and the other numbers, and a philosopher's theoretical account of what numbers "must" be (namely, collections of units, in which case one is not a number),[8] here it is the practice that suggests something fundamental about the pre-modern conception of number. Here it really does seem that there was something they could not do that we can. Why after centuries of recording their results in Roman numeration did the fashion suddenly begin to change in the late sixteenth century? The answer, I think, can only be that the most basic understanding of what numbers are was changing towards the middle of the second millennium, first for practical purposes, for instance, in the introduction of negative numbers in the recording of debts, and finally, after Descartes, in theory as well.[9]

For centuries after its introduction, Arabic numeration was conceived *only* as an instrument, a tool that although useful for calculating results could not be employed in the statement of those results. The signs of Arabic numeration were not conceived as representatives of numbers, as a perspicuous notation of how many. And nor could they be so long as numbers were conceived in terms of the question 'how many?', that is, as collections of units. Similarly, we will see, although François Viète devised a notation suitable for algebraic manipulations, that notation had for him merely instrumental value. It was useful, but not itself a language within which to express mathematical ideas. Only with Descartes would we acquire the eyes to read the symbolism of arithmetic and algebra as itself a language, albeit one of a radically new sort.

Descartes' new mathematical practice and transformed vision both of ourselves as knowers and of the reality known, fundamentally changed who we are. But of course

[7] This may also help to explain the otherwise surprisingly heated debates between abacists and algorismists that persisted even into the seventeenth century (Chrisomalis 2009, 509–10).

[8] As Høyrup (2004, 143) remarks, "if it was necessary to explain so often that unity was no number, then the temptation must have been great to see it as one." "The conceptual otherness that is reflected in the sermons about the nature of number is not caused by any inability to think otherwise; the sermons censure an ever-recurrent tendency to neglect in mathematical practice taboos resulting from philosophical critique" (Høyrup 2004, 144).

[9] The invention of double-entry bookkeeping and the replacement of Roman numeration by Arabic numeration happened almost simultaneously during the fourteenth and fifteenth centuries (Urton 2009, 30–1).

Descartes did not appear out of nowhere. As just indicated, Viète had already developed a notation for basic algebra, and already European culture had become, at least in some respects, "modern." Already nature was coming to be conceived as a kind of clockwork, that is, a mechanical device rather than a living being.[10] These earlier developments, briefly considered in the next two sections, will serve to highlight both the respects in which Descartes continues the tradition he inherits and the extent to which he completely transforms our most fundamental understanding of the being of things in the world and of our cognitive relationship to them.

3.1 The Clockwork Universe

Due in part to the recovery of various classics of ancient Greek philosophy, mathematics, and science, the intellectual culture of Europe was fundamentally transformed over the four centuries preceding Descartes' birth in 1596. With the development of musical notation beginning in the thirteenth century, and the construction of the first clocks, time would, by the fifteenth century, come to be seen as something in its own right, independent of motion, and as itself measurable or quantifiable. The "quantification" of space is similarly manifested in the development of the renaissance conception of what constitutes a "faithful" depiction of things in space, both in the new perspective drawings and paintings, and in the new sorts of maps that began to appear in the fifteenth century. With this quantification of space and time came, finally, the new Galilean conception of motion as at once measurable and subject to laws.[11] What did not change amidst all these developments was our most fundamental conception of being. A grid was being laid over all reality; everything, it was coming to seem, can be measured and counted, or at least ordered, in tables, columns, and graphs. Not living things but clockworks, mechanical devices, were coming to be seen as the paradigm of being. The problem was that no one before Descartes really understood just what that might mean.

In our everyday lives we keep track of where we are both in space and in time. One is in the marketplace or at home, in town or at the seaside; it is morning or night, summer or winter. But although the further idea of measuring spaces, that is, lengths, surfaces, and volumes, is quite natural to us, the further idea of measuring times is not. Time is most naturally understood in terms of motion, for instance, the motion of the sun from its rising to its setting. Because motions can be faster or slower, so, it at first seemed, time can go more or less quickly, the hours themselves expanding and contracting as the seasons change. And the first mechanical clocks did nothing to change this conception. Early clocks were a means of tracking time, the changing

[10] Compare Lachterman's (1989, 125) suggestion that "the advent of radical modernity, at least in its Cartesian figure, might be characterized by this pairing of two tactics for outwitting 'Nature', mechanization and symbolization." See also Mahoney (1998) and Shapin (1996, ch. 1).

[11] And there were other important developments as well. See Crosby (1997), also Foucault (1970).

hours and days, seasons and years, but not yet a mechanism by which to *measure* time. They were nothing more than mechanical reproductions of the motions of the sun and passing of the seasons. The idea of measuring time, as opposed merely to keeping track of it, began to be developed only in the thirteenth century, and only in Europe. And it was made possible at all because musicians got the idea of writing music in addition to performing it.[12]

The earliest musical notation was devised for Gregorian chant, a form of singing that is monophonic and rhythmic without being metrical; in Gregorian chant, all singers sing the same notes at the same time, and although tones can be longer or shorter, they are not measured against any independently fixed temporal unit. There is no measure, no beat, only the longer and shorter tones themselves. As the number of these chants grew, and with them the time needed to learn them, the idea of trying to record the chants suggested itself. Much as written natural language records the sounds speakers make, so the sounds singers make could be recorded, not as in a written language such as English that uses arbitrary marks to stand for sounds, but by the relative placement of marks, higher or lower corresponding to the higher or lower pitch of the notes sung. The result was a two-dimensional display of the notes to be sung according to their pitch along the vertical and in their order along the horizontal. This notation hastened in turn the development of polyphonic music in which different singers sing different notes with different durations, and the development of notations adequate to record this new sort of song.

Monophonic music requires only that all singers keep time with one another; it is not essentially written. Polyphonic music, because it involves different singers singing various notes with different durations, requires that time itself be measured, that is, that a musical notation be developed in which time is tracked independently of the notes that are to be sung. Polyphonic music needs, in other words, a time standard, a unit that is the measure of a duration, and with it a sign for a "rest," that is, the temporal duration within which a singer is to remain silent. Indeed, "the symbols of rests are nothing if not instructions to measure time intervals independently of anything else" (Szamosi 1986, 105). (The symbol for zero is essentially similar, an instruction to conceive numbers independently of counting and things counted—something no one in Europe seemed able to do before the sixteenth century.)

We are so familiar with the idea that time can itself be measured, that it marches on in measured steps, that it can be hard for us to imagine what it was like to live in a world that knows no measured time. We do not in the same way need to use our imaginations to know how things seemed before we achieved the idea of measured space. We have early maps and pictures to show us.

Measuring spaces, the lengths, areas, and volumes of things is as natural as counting, and is common already in various ancient cultures. But this is not yet the

[12] I here follow Szamosi (1986, ch. 5). But see also Crosby (1997, ch. 8).

idea of measuring space itself. So, for instance, we find that thirteenth-century European maps are more like pictures than maps, and peculiar pictures at that. They are "a non-quantificational, non-geometrical attempt to supply information about what was near and what was far—and what was important and what was unimportant" (Crosby 1997, 40; see also Harvey 1993). Pre-Renaissance paintings similarly depict what is more important, say, a deity or an emperor, literally as larger and more central than what is less important, for instance, a peasant. Things are not placed in space in such pictures; they are merely placed. To depict things in space requires a means of depicting the spatial analogue of a musical rest, that is empty space, and only in the fourteenth century did painters begin to consider how to do this. They began to paint not merely spatial things but things in space, that is, as near or far (see Crosby 1997, ch. 9). But the development of perspective painting requires much more than painterly insight. Like the development of a notation for polyphonic music, it requires both a theory (in this case, of optics) and a carefully articulated, step-wise method of depiction.

We say that perspective painting is more realistic than its predecessors, and this is often taken to mean that such paintings depict what one actually sees rather than, say, how what one sees is interpreted. The fact that such works require both a theory of optics and explicitly stated rules governing their production, together (often) with special apparatuses or mechanical aids to help one to see in this way, indicate that this is a mistake. What is true is that such paintings, like fifteenth-century maps, are geometrically accurate; they encode, often with great accuracy, quantitative information about the relative placement and size of things. Much as a fifteenth-century map, unlike earlier maps, accurately depicts relative size and location as if seen from above, so a renaissance perspective painting accurately depicts relative size and location as if seen from in front. In both cases, what is depicted is quantitative information, measurable features of things.

With these developments in the arts came as well the "profoundly unmedieval idea" of progress in human affairs, and self-conscious efforts to cast off old ways in order to develop something new (Crosby 1997, 155). Copernicus's *De revolutionibus* (1543) was to have much the same effect in the sciences. If Copernicus was right, the scholastic Aristotelian conception of the world with its concentric crystalline spheres, at the center of which was the earth, was wrong; and it was shown to be wrong by careful observations, measurements, and reflection. This new account furthermore suggested that the relationship between what we observe and reality is more complex than one might naturally think, that one cannot merely read how things are directly off their appearance to one. By the sixteenth century, it was becoming clear that a general reform of all knowledge and all learning was needed, that a new beginning, a "great instauration," as Bacon would put it, had to be made, one that would "try the whole thing anew upon a better plan, and to commence a total reconstruction of sciences, arts, and all human knowledge raised upon the proper foundations" (Bacon

1620, 66). The idea of modernity, "decisively and *irreversibly* novel in comparison with the past in its entirety," was being born (Lachterman 1989, 126).[13]

But although the Copernican picture of the relative motions of the earth and the heavenly bodies provided, relative to the available data, an intellectually satisfying account, it also posed a serious problem: if the earth moves, why do we not feel it or see it in, say, the displacement of falling objects? Galileo (1564–1642) provided the answer, and thereby solved as well the ancient problem of the motion of projectiles. His use of diagrams in solving these problems is an important precursor to Descartes' use of them.

According to the Aristotelian conception, what we today call motion is only one instance, namely, local motion, of a much wider conception that includes all manner of change. For Aristotle, natural motions, which comprise all the ways a thing changes over time as the sort of thing it is, belong to the essence of a thing. The ripening of fruit, the education of youths, the rolling of a rock down a hill, all are conceived as motions appropriate to the natures involved. They belong to various sorts of things as the kinds of things they are; things move according to their natures. And all such motions naturally come to an end. But there are also unnatural motions, for instance, the motion of something pushed or pulled and the motion of projectiles. The first sort of unnatural motion was easily explained by appeal to the agent doing the pushing or pulling because as soon as the agent stops pushing or pulling the motion itself stops. Projectile motion was much more puzzling. Why do projectiles keep moving for as long as they do given that nothing seems to be making them move after they are released? Aristotle did come up with an account of sorts involving the air as the pusher but it was not very plausible, and in the fifth century John Philoponus suggested instead that when a projectile is set into motion it acquires some kind of motive force or impulse that keeps it moving, at least for a while. (Buridan would suggest something similar in the fourteenth century.) Galileo would finally solve the puzzle and thereby the problem of the moving earth as well.[14]

Careful observation and measurement of a ball rolling down a slightly inclined plane reveals that the ball accelerates at a uniform rate. A ball rolling up such a plane likewise decelerates at a constant rate. It stands to reason, then, that a ball rolling on a horizontal surface will neither accelerate nor decelerate, that is, that it will roll along at the same speed forever. But if so then Aristotle had been asking the wrong question. What needs to be explained is not why projectiles move but why they

[13] This may not be entirely true. According to Klein (1968, 120), the new science "conceives of itself as again taking up and further developing Greek science, i.e., as a recovery and elaboration of 'natural' cognition. It sees itself not only as the science of *nature*, but as '*natural*' science—in opposition to *school* science."

[14] Early intimations of the notion of inertia are found already in the *Mechanical Problems* of pseudo-Aristotle, which Winter (2007) ascribes to Archytas of Tarentum. See also Laird (2001) on the place of the *Mechanical Problems* in Galileo's thinking. Apparently Galileo lectured on the *Mechanical Problems* in Padua in 1598.

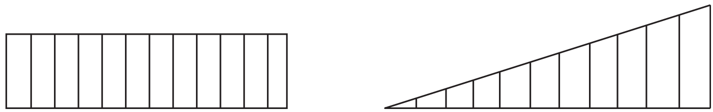


Figure 3.2 Oresme's time-speed graphs of uniform speed (left) and uniform acceleration from rest (right).

ever stop moving having once been set into motion. And as Galileo saw, if that is the right question to ask then a body must be indifferent to its motion. Motion does not belong to a thing in virtue of its nature, its being as the sort of thing it is; and a body can be moving even at a great speed without that motion being apparent to it. Only changes in motion (or rest now conceived as the limit of motion) need a cause, the force of which can be felt. Furthermore, because a body in motion is indifferent to that motion, different motions can be combined in one moving object, for instance, in a projectile, which combines vertical and horizontal motion. And even an object dropped from some height above the earth forms a kind of projectile: because it is already moving with the earth it continues that motion combined with its downward acceleration until it lands directly below the point at which it had been released—not somewhere behind that point as had seemed to be predicted by the Copernican idea of a moving earth.

Like Copernicus's discovery of the movement of the earth relative to the sun and other heavenly bodies, Galileo's discovery of the law of inertia combined careful observation and measurement, in this case of distance traveled per unit of time, with thoughtful reflection on what those observations and measurements revealed. His analysis of projectile motion involved in addition a very novel use of Euclidean principles. Already in the fourteenth century Oresme had taken the idea of a time-pitch graph from music and applied it to the case of motions. The progression of times was exhibited in a horizontal line and relative speed in a series of vertical lines along the horizontal. A depiction of uniform speed over time thus forms a rectangle while uniform acceleration from rest forms a triangle. (See Figure 3.2.) And other cases similarly generate other shapes. Galileo would combine this basic idea of mapping speed over time with that of measure to realize a radically new form of argument.

To show, for example, that the time taken to traverse a given distance by a body starting at rest and uniformly accelerating is the same as that in which the distance is traversed by a body traveling at a constant speed equal to half the maximum speed achieved by the accelerating body, Galileo appeals to the diagram shown in Figure 3.3.[15] AB represents the time the accelerating body takes to travel the distance

[15] Grosholz (2007, sec. 1.1) provides an insightful discussion of this and other examples from Galileo's *Discorsi*. As she notes (2007, 9), Galileo turns Oresme's time-speed graph on its side so that it can more directly be applied to the case of free fall.

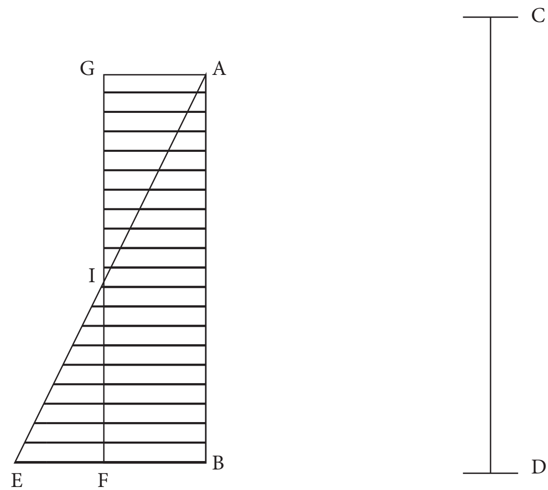


Figure 3.3 Galileo's diagram for Theorem I, proposition I of his *Discorsi*, Third Day.

CD. BE represents the maximum speed achieved by that body, and the lines parallel to BE, that is, the lines drawn from AB to AE, represent the increasing speeds from rest at A to the maximum at BE. F is the midpoint of EB and FG is drawn parallel to AB. The parallelogram AGFB is constructed. Galileo then argues that because the area ABE, which represents the distance traveled by the accelerating body, equals the area AGFB, which represents the distance traveled by the body in uniform motion, the time taken by a body traveling at constant speed half the maximum of a uniformly accelerating one is the same as that taken by the accelerating body to come the same distance.[16] What is crucial here is the fact that although distance traveled is most intuitively represented by a line, such as CD above, Galileo sees that because distance is a function of time and speed at a time, it can also be represented as an area, for instance, as the area of a triangle or of a rectangle. Galileo uses lines and areas not iconically to formulate the contents of concepts as ancient Greek geometers did, but instead to represent or stand in for that which is measurable, such as time, speed, and distance traveled, and the diagram enables him to formulate in a visual display arithmetical relationships among these measurables. The notion of a dimension is thus loosened from its association with the three dimensions of experiential space and is well on its way to becoming, as Descartes thinks of it in the *Regulae*, as "a mode or aspect in respect of which some subject is considered

[16] Notice that in this argument lines serve indifferently as sides of geometrical figures and as representations of arbitrary numbers. Both uses of lines are found already in Euclid's *Elements*, but in Euclid they are scrupulously distinguished and never combined. In Euclid, geometrical lines are utterly different from numbers, despite the fact that numbers can be expressed in lines, as in Books VII–IX. In Euclid, lines (in the relevant demonstrations) do not merely represent or stand for numbers. They *are* numbers, collections of units, albeit arbitrary ones insofar as the unit of measure is not given.

measurable. Thus length, breadth and depth are not the only dimensions of a body: weight too is a dimension—the dimension in terms of which objects are weighed. Speed is a dimension—the dimension of motion; and there are countless other instances of this sort" (CSM I 62; AT X 447[17]).

The idea of tracing in a visual display a phenomenon that is not itself visual is manifested already in written natural language. In the simplest musical notation that same idea recurs but in a form that is essentially two-dimensional and iconic. Instead of a series of arbitrary symbols standing for sounds, musical notation uses spatial position to track not only order but also pitch. Beginning with the simplest time-pitch graphs of Gregorian chants, through those involving a time measure that are needed for writing polyphonic music and Oresme's time-speed graphs, Galileo comes to use Euclidean diagrams and principles together with careful observation and measurement to analyze motion itself. This new form of representation is like a picture, map, or traditional Euclidean diagram, and unlike written natural language, insofar as it signifies a thing (or in the case of Euclid, a content) directly, not as mediated by the sounds we make in talking about it. But like written language, and unlike a picture or map, it depicts something that is not intrinsically visual. We can, of course, see things move, and often we can see as well the traces that can be left by moving things, but motion itself is not visual in the sense that, say, the shape of a thing (that is, parts in spatial relation) is visual. It cannot be pictured and yet, as Galileo realized following Oresme, it can be depicted in a two-dimensional array. Even more important, one can use such arrays to discover relations among the various notions—speed, distance, and time—that are involved in any analysis of motion.

We have seen that for Aristotle the paradigm of a substance, of what is, is a living thing, an organic unity with a characteristic form of life that has the form of a narrative or story with a beginning, a middle, and an end. For the Aristotelian scholastics, reality itself unfolds in a narrative; it takes the form of a story that can be illustrated in pictures but is itself something to be told—spoken and heard. Galileo's work on motion typifies the new sensibility that had developed by the sixteenth century, a sensibility according to which reality is conceived not narratively, as a story to be told, but visually, as something to be mapped in two-dimensional space. To discover how things are, it was coming increasingly to seem, is to turn one's back on the stories we tell, to confront nature directly through careful observation and measurement. Nature, it was coming to seem, does not address us in our natural languages; it does not speak. Rather it is, Galileo suggests, "written in the language of mathematics, and its characters are triangles, circles, and other geometrical figures, without which it is humanly impossible to understand a single word of it; without

[17] All references to Descartes' works, save for the *Geometry*, are to the three volume edition of Cottingham, Stoothoff, and Murdoch (CSM) together with reference to the standard twelve-volume *Oeuvres de Descartes*, edited by Ch. Adam and P. Tannery (AT), revised edition, (Paris: Vrin/C.N.R.S., 1964–76). All references are by volume and page.

these, one is wandering about in a dark labyrinth" (Galilei 1623, 184). Nature is written; it is a book. But unlike any ordinary book, it is not written to communicate something to someone in a natural, primarily aural and narrative, language. It is written in the constitutively visual language of (Euclidean) mathematics.[18] Mathematics, which for the ancient Greeks was irrelevant to the science of nature insofar as mathematics concerns what is unchanging and the science of nature that which changes, has become nature's own language.

The idea that reality is to be mapped and measured in diagrams and graphs rather than told in stories furthermore helps to explain why ancient atomism came now to grip the imagination of not only Galileo but also, for instance, Hobbes, Mersenne, Gassendi, and even, at least for a time, Descartes. What for Aristotle had seemed to be merely two sorts of sensory entities, the proper sensibles such as color, taste, odor, and sound, which can be sensed by only one sense organ, and the common sensibles such as shape, number, and motion, which can be both seen and felt, came now to be conceived as two radically different aspects of reality. And they did so because only Aristotle's common sensibles can be mapped and measured, written in the language of mathematics. On this new conception, the natural world, as contrasted with our experience of it, contains no colors, odors, tastes, or sounds, but only that which has shape, size, number, and motion.

For a sentient animal making its way through its environment, the sensory features of things, their colors, odors, taste, and sounds, are crucial guides to what is to be pursued and avoided. Nor in our own everyday experience of things can such features be separated from their shapes, sizes, number, and motions, except in an act of thought. And yet Galileo suggests that his mind "feels no compulsion" to take the former qualities to be essential to things in the way the latter are. As he writes in *The Assayer*:

> upon conceiving of a material or corporeal substance, I immediately feel the need to conceive simultaneously that it is bounded and has this or that shape; that it is in this place or that at any given time; that it moves or stays still; that it does or does not touch another body; and that it is one, few, or many. I cannot separate it from these conditions by any stretch of my imagination. But that it must be white or red, bitter or sweet, noisy or silent, of sweet or foul odor, my mind feels no compulsion to understand as necessary accompaniments. Indeed, without the senses to guide us, reason or imagination alone would perhaps never arrive at such qualities. For that reason I think that tastes, odors, colors, and so forth are no more than mere names so far as pertains to the subject wherein they reside, and that they have their habitation only in the sensorium. Thus, if the living creature (*l'animale*) were removed, all these qualities would be removed and annihilated. (Galilei 1623, 309)

> I do not believe that for exciting in us tastes, odors, and sounds there are required in external bodies anything but sizes, shapes, numbers, and slow or fast movements; and I think that if

[18] Thus early modern thought is sometimes described as distinctively visual, a matter of a new way of seeing. According to Crosby (1997, 132), for example, "the shift to the visual" is the catalyst of the quantification of reality.

ears, tongues, and noses were taken away, shapes and numbers and motions would remain but not odors or tastes or sounds. (Galilei 1623, 311)

The proper sensibles, the color, taste, odor, and sound of a thing, are inconceivable independent of our experiences of them, of what it is like to see, taste, smell, and hear them. This is not true in the same way of the common sensibles insofar as they can be both seen and felt. The common sensibles, that is, the shape, size, number, and motions of things, thus can come to seem to belong to things independent of our perceptual experiences of those things. Only in these cases is there a resemblance between the properties of things and our experience of these properties. Our understanding of mechanical contrivances such as clockworks further reinforces the idea. The workings of a mechanism can be fully understood by appeal only to the shapes, sizes, number, and motions of its parts. Colors, sounds, tastes, and odors have no role to play in an account of the workings of a mechanism. Hence, it was coming to seem, they have no place in nature either but only in our experience of nature. Natural changes were one and all to be reduced to the motions and impacts of atoms.

The new mathematical physics that was being developed by Copernicus, Kepler, Galileo, and others provided an intoxicating vision of precise mathematical descriptions of all the phenomena of nature. But it was as yet only a vision. The idea of modernity, of science raised upon its proper foundation, of a new form of explanation to replace that of Aristotelian substantial forms, remained to be fully realized. The idea of nature as a clockwork, a mere mechanism, needed to be combined with that of a properly symbolic language.

3.2 Viète's Analytical Art

Between the thirteenth and the sixteenth centuries, developments in music, painting, astronomy, and physics had begun radically to alter our understanding of nature. But mathematics too was changing. By the thirteenth century, the algebra of Al Khwarizmi (d. *c.*850) and Omar Khayyam (*c.*1040–1123), which dealt with linear, quadratic, and cubic equations (expressed in natural language), was widely known in Europe; and by the sixteenth century this work had been extended to quartic equations.[19] Various abbreviations and symbols were introduced for powers and other mathematical operations culminating in the literal notation, developed by François Viète (1540–1603), in which two sorts of letters, one for the unknown and the other for the known parameters of a problem, are employed in its solution. Viète is often regarded

[19] It is often assumed that "a single line of development led from the Latin presentations of the subject [that is, of Arabic *al-jabr*] (the translations of al-Khwārizmī and the last part of Fibonacci's *Liber abbaci*) to Luca Pacioli, Cardano, and Tartaglia" (Høyrup 2006, 5). Høyrup (2006) argues, on textual grounds, that a more complex account is needed.

as the first modern mathematician for just this reason.[20] In fact, we will see, Viète's most fundamental orientation remains essentially pre-modern. Only with Descartes is early modern mathematical practice fully realized.

According to a familiar account due to G. H. F. Nesselmann (*Versuch einer kritischen Geschichte der Algebra. Vol. 1: Die Algebra der Griechen* first published in 1842), algebra was at first rhetorical, that is, written in natural language, then syncopated insofar as it began to involve the use of signs or ligatures in place of some words, and finally properly symbolic, all words having been replaced by written signs of some sort. But as Heeffer (2009) argues, there are a number of difficulties, both historical and conceptual, with the account. Most important for our purposes, Nesselmann's account fails to distinguish in any significant way between *symbols* and shorthand notation, that is, abbreviations of various sorts (Heeffer 2009, 5). But as we will see, even this distinction between the use of signs as abbreviations and the use of signs as symbols does not cut finely enough. Viète does not use signs as abbreviations for words but nor, it will be argued, does he use them as symbols in the sense that Descartes does, in accordance with a properly symbolic way of thinking.

We have already noted that although paper-and-pencil calculations using Arabic numeration existed alongside the use of a counting board throughout the first half of the second millennium, they seem to have been understood merely instrumentally. This, I suggested, is unsurprising if numbers were understood as collections of units, and hence as properly expressed not in the positional decimal notation of Arabic numeration but instead in an additive notation such as Roman numeration. Nevertheless, it is remarkable that one can by paper-and-pencil manipulations *work out* the solution to an arithmetical problem that is otherwise worked out either "in one's head," by mental arithmetic, or by moving counters on a counting board. By the fifteenth century, the contrast between "symbolical" (*figuratamente*) and "rhetorical" (*per scrittura*) methods for solving problems had been made explicit (Heeffer 2009, 5–6). This distinction is one we have already appealed to (in the introductory section of Chapter 1). Leaving aside the technology of the counting board, which seems clearly to be neither symbolical nor rhetorical in the relevant sense, one can solve a problem in arithmetic either by way of a paper-and-pencil calculation (*figuratamente*) or by working it out through a chain of reasoning in mental arithmetic, that is, by reflecting on the relevant mathematical ideas and what they entail. Such a chain of reasoning can then be reported in natural language, whether written or spoken (*per scrittura*).

As Heeffer (2009) notes, a sign such as, for example, the familiar '+' for addition can function either as a mere abbreviation for a word of natural language or as a symbol properly speaking, and only the context of use can determine which way it is being used.

[20] See for example, Bos (2001, 154), Mahoney (1980, 144), Bashmakova and Smirnova (1999, 261), and Klein (1968, 183).

When printed in an early fifteenth century arithmetic book, the + sign in '3 + 5 makes 8' would be interpreted as a shorthand for 'and', meaning the addition of five to three. Here, 'plus' describes an operation, a mental or even physical action. There is some temporal element present in the description '3 + 5 makes 8'. First you have three; after adding five, you find out that you have eight. The + sign in this context is thus a direct representation of the action of adding things together. (Heeffer 2009, 11)

In this context the sign is merely shorthand for a description of something to be done on numbers conceived as collections of units that can be combined or put together. Correspondingly, Heeffer claims, properly symbolic thinking is possible not only in symbols but also in words, as, for example, in Pacioli's *Summa de arithmetica geometria proportioni* of 1494 where we find in words various rules of signs, for instance, these: Dividing a positive by a negative produces a negative (*a partire pui per meno neven meno*) and dividing a negative by a positive leads to a negative (*a partire meno per pui neven meno*) (Heeffer 2009, 14). As Heeffer (2009, 15) notes, "the formulation of the rules does not refer to any sorts of quantities, integers, irrational binomials or cossic numbers. The rules only refer to 'the negative' and 'the positive'." Hence, he concludes, "despite the absence of any symbolism, we consider this an early instance of symbolic reasoning." But as in the case of Arabic numeration, we need to ask whether the reasoning involved is conceived merely instrumentally or whether it is taken somehow to embody properly mathematical content.

This question is further complicated, for us, by the fact that we inherit a nineteenth-century tradition that is explicitly formalist in algebra. We read, for example, in an essay by George Peacock published in 1833, that we must distinguish between arithmetical and symbolic algebra.[21] As Peacock explains:

The first of these sciences would be, properly speaking, *universal arithmetic*: its general symbols would represent numbers only; its fundamental operations, and the signs used to denote them, would have the same meaning as in common arithmetic; it would reject the *independent* use of the signs + and −, though it would recognize the common rules of their incorporation, when they were preceded by other quantities or symbols: the operation of subtraction would be *impossible* when the subtrahend was greater than the quantity from which it was required to be taken, and there the proper *impossible* quantities of such a science would be the *negative* quantities of *symbolical* algebra; it would reject also the consideration of the multiple values of simple roots, as well as of the negative and impossible roots of the second and higher degree: it is this species of algebra which alone can be legitimately founded upon arithmetic as its basis.[22]

On this account, arithmetical algebra is a perfectly meaningful science "concerning which questions of truth and falsity are significant"; symbolical algebra is not

[21] According to Heeffer (2009, 8), Peacock's *A Treatise on Algebra* (1830) marks the first use of the term 'symbolic algebra'.

[22] "Report on the Recent Progress and Present State of Certain Branches of Analysis," *Report of the British Association for the Advancement of Science* 3 (1833), p. 189; quoted in Nagel (1935, 180).

meaningful—"questions of truth are meaningless"—but instead merely formal (Nagel 1935, 180). On this "fundamental point", Nagel (1935, 182) furthermore holds, Peacock "saw clearly and truly: the possibility of interpreting extrasystemically the symbols of an algebra [that is, the symbols of a symbolical algebra] in no way affects the validity of equivalences established by means of the formal rules of the combination of symbols which characterize that algebra".[23] But this, I will argue, is not a historically adequate account insofar as in the history of mathematics, and in particular in mathematics as it was developed by Descartes in the seventeenth century, the symbolism of algebra is not merely formal but serves rather to introduce a *new* subject matter for mathematics. Although, as we will soon see, algebraic symbolism is for Viète merely formal in something very like Peacock's (nineteenth-century) sense of formality insofar as it is an uninterpreted system of signs, algebraic symbolism is not at all formal/uninterpreted in Descartes' employment of it.[24]

Viète's Analytical Art comprises three stages. At the first stage, *zetetics*, a problem, whether of arithmetic or geometry, is translated into Viète's newly created symbol system or *logistice speciosa* in the form of an equation. At the second stage, *poristics*, the equation is transformed according to rules until it achieves canonical form. At the third and last stage, *exegetics*, a solution to the problem is found on the basis of the derived equation. As Viète himself emphasizes, at this third stage the analyst turns either geometer, "by executing a true construction," or arithmetician, "solving numerically whatever powers, whether pure or affected, are exhibited" (Viéte 1646, 29). That is, one begins with a problem, either arithmetical or geometrical, that is then formulated in Viète's *logistice speciosa* and operated on according to rules; at the third and final stage, one returns either to arithmetic or to geometry to complete the problem. Viète teaches the Art in eight essays first published individually between 1591 and 1631, then brought together in a single volume, the *Opera Mathematica*, in 1646.[25]

Both in the opening paragraph of the Introduction to *The Analytical Art* and in the Dedication that precedes it, Viète emphasizes the close connections between his art

[23] Nagel immediately continues: "It is this basic doctrine which is at the heart of modern logical theories, and which is the despair of their hostile critics." And this may well be right insofar as both Peacock's work and "modern logical theories" follow in the wake of Kant. But Kant, we need to remember, is responding to a *philosophical* difficulty raised by early modernity, and doing so with a philosophical *theory*. We will not be able so much as to understand the philosophical difficulty to which he is responding if we read back into developments in mathematics *before* Kant philosophical views that Kant developed in response to those difficulties. We will examine in Chapter 6 some of the sources of the formalism of "modern logical theories."

[24] Becoming clearer about this history, and in particular the transformed understanding of the nature and subject matter of mathematical practice that Descartes achieves, may help to explain why, for instance, the identity sign first introduced by Robert Recorde in 1557 "was not universally accepted for another century" (Heeffer 2009, 22). As long as the most fundamental orientation of mathematicians was that of the ancient Greeks, the theory of equations independent of any reference to numbers that might be equated had inevitably to be treated as somewhat anomalous.

[25] The *Opera Mathematica*, edited with notes by Frans van Schooten, was published in Leyden. A facsimile reprint has been issued by Georg Olms Verlag, Hildesheim (1970).

and the work of the Greeks.[26] Three themes from the then newly rediscovered Greek mathematical tradition are especially relevant: Pappus's conception of the analytic method in geometry as outlined in the seventh book of his *Mathematical Collection*,[27] Diophantus's treatment of arithmetical problems using letters for the unknown and for powers of the unknown in his *Arithmetica*,[28] and Eudoxus's general theory of proportions as set out in the fifth book of Euclid's *Elements*.[29] In the spirit of Eudoxus's general theory, Viète aimed to provide a general method for the solution both of the sorts of arithmetical problems Diophantus had considered and of the sorts of geometrical problems Pappus discusses. The method itself was that of analysis, a method Pappus describes as making "the passage from the thing sought, as if it were admitted, through the things which follow in order [from it], to something admitted as the result of synthesis."[30] Diophantus's treatment of arithmetical problems provides an instructive illustration.

Diophantus's *Arithmetica* is a collection of arithmetical problems involving determinate and indeterminate equations together with their (reasoned) solutions. The text is remarkable along a number of dimensions. First, unlike earlier Babylonian and Egyptian texts dealing with similar sorts of problems, Diophantus's *Arithmetica* refers not to numbers of cattle, or sheep, or bushels of grain, but to numbers of pure monads (or unknown numbers of monads, or powers of unknown numbers of monads); and it aims to provide not merely rules for the solution of problems but a demonstration, of a sort, to show why the rule is a good one. The *Arithmetica*, in other words, is a scientific or theoretical work at least as much as it is a practical manual in the art of solving problems. It (or possibly only a later copy[31]) is also remarkable in employing abbreviations—for the unknown and for powers of the unknown (up to the sixth), for the monad, and so on—all of which are explicitly introduced at the beginning of the work, and in providing explicit rules for the transformation of equations (by adding equal terms to both sides and reducing like terms).[32] The letter 'ς' (from *arithmos*, number) is used to signify the unknown, 'Δ^{υ}' (from *dunamis*, power or square) is employed for the square of the unknown, and

[26] As Heeffer (2009, 7) notes, "the humanist project of reviving ancient Greek science and mathematics played a crucial role in the creation of an identity for the European intellectual tradition. Beginning with Regiomontnus' 1464 lecture at Padua, humanist writers distanced themselves from 'barbaric' influences and created the myth that all mathematics, including algebra, descended from the ancient Greeks." As has already been noted, Klein (1968, 120) makes a related point for the case of *natural* science as contrasted with the school science of the Scholastics.

[27] A Latin translation of the *Collection* (fourth century A.D.) appeared in 1588/9. It is likely that Viète had access to the recovered original before then. See Klein (1968, 259, n. 214).

[28] The first six books of the *Arithmetica* (third century A.D.) were rediscovered in 1462 and published in Latin around 1560.

[29] The *Elements* was first printed (in Latin) in 1482.

[30] I here follow Mahoney's (1968, 322) translation.

[31] Heeffer (2009, 3) provides evidence to suggest that this use of signs in the extant text of Diophantus dates back only to the ninth century and not to Diophantus himself in the third. But see also Netz (2012).

[32] See Bashmakova (1997).

'K^{v}' (from *cubos*) for the cube of the unknown.[33] The fourth power is a square-square, the fifth a square-cube, and the sixth a cube-cube. The monad (unit) is abbreviated 'M^{o}'. Negative numbers are conceived in terms of missing or lacking and are signaled by a special sign (an inverted 'ψ').[34] Diophantus uses Greek alphabetic numerals, and he indicates addition by concatenation.

A simple problem illustrating his analytic method is to divide a given number into two numbers with a given difference. Diophantus turns immediately to a particular instance: the given number is assumed to be, say, one hundred, and the difference forty, units.

> Let the less be taken as $\varsigma\alpha$ [one unknown]. Then the greater will be $\varsigma\alpha M^{\text{o}}\mu$ [one unknown and forty units]. Then both together become $\varsigma\beta M^{\text{o}}\mu$ [two unknowns and forty units]. But they have been given as $M^{\text{o}}\rho$ [one hundred units]. $M^{\text{o}}\rho$ [one hundred units], then, are equal to $\varsigma\beta M^{\text{o}}\mu$ [two unknowns and forty units]. And, taking like things from like: I take $M^{\text{o}}\mu$ [forty units] from the ρ [one hundred] and likewise μ [forty] from the β [two] numbers and μ [forty] units. The $\varsigma\beta$ [two unknowns] are left equal to $M^{\text{o}}\xi$ [sixty units]. Then, each ς [unknown] becomes $M^{\text{o}}\lambda$ [thirty units]. As to the actual numbers required: the less will be $M^{\text{o}}\lambda$ [thirty units] and the greater $M^{\text{o}}o$ [seventy units], and the proof is clear.[35]

Rather than merely telling us what to do to find the desired answer as his predecessors had done (say: take forty from one hundred to give sixty, then divide by two to give thirty; the two numbers, then, are thirty and thirty plus forty, or seventy), the results of which can then be checked against the original parameters of the problem, Diophantus works the problem out arithmetically. Because he has a sign for the unknown, Diophantus can treat it as if it were known and proceed, through a series of familiar operations, to the answer that is sought. This analytical treatment of arithmetical problems through the introduction of signs for the unknown and its powers, together with Eudoxus's treatment of proportions—where a proportion, according to Viète (1646, 15), is "that from which an equation is composed"—provides Viète with the crucial clues to his Analytical Art.

Although Diophantus's method for solving a problem is clearly meant to be a general one for problems of the relevant type, his solutions are always of particular numerical problems. He has a general method but no means of expressing it in its full generality. Viète resolves the difficulty by appeal to the distinction between vowels and consonants: unknown magnitudes are to be designated by uppercase vowels and given terms by uppercase consonants, all of which are to be operated on as Diophantus operates on his signs for the unknown and its powers.[36] Viète also greatly

[33] As Klein (1968, 146) notes, these are likely merely word abbreviations and ligatures.

[34] On the question of the notion of negative number in play in Diophantus, see Bashmakova (1997, 5–6).

[35] Quoted in Klein (1968, 330–1, n. 22).

[36] Where this difference does not need to be marked, as in setting out the rules governing addition, subtraction, multiplication, and division of "species," Viète uses the two sorts of letters indifferently.

simplifies matters by designating the unknown and its powers not by using a variety of different signs (as Diophantus, and the cossists, had) but by using one sign, a vowel, for the unknown, followed by a word ('*quadratum*', '*cubum*', and so on) to indicate the power of the unknown. More significantly, he also generalizes Diophantus's method to apply not only to the sorts of arithmetical problems Diophantus considers but also to well-known geometrical problems. It is this dimension of generalization, we will see, that provides the key to an adequate understanding of Viète's *logistice speciosa*, his symbolic language or algebra.

Perhaps the first person to recognize the fundamental connection between Euclidean geometry and the new algebra, or art of the coss, was Petrus Ramus (1515–1572), the influential French pedagogue and author of textbooks of mathematics. It was Ramus who first gave the sort of algebraic reading of the *Elements*, in particular, of Books II and VI, that would become standard with Zeuthen and Tannery.[37] But it was Viète who would realize Ramus's ambitions, both mathematical and pedagogical, by showing that algebra, or as he preferred to call it, analysis, provides a general method for the solution of problems whether geometrical or arithmetical. The aim of Viète's Analytical Art, following Ramus, is to teach this method in a pedagogically effective fashion, that is, in a way that will enable students systematically to solve mathematical problems. (See Mahoney 1973, 32–3.)

As already noted, the *logistice speciosa* that Viète introduces in the Analytical Art uses two different sorts of uppercase letters—vowels and consonants—for unknown and known parameters of a problem. The various species (or powers) of unknowns are designated by a vowel followed by a word marking the power to which it is raised, for example, 'A *cubum*' (sometimes 'A *cub.*') or 'E *quadratum*' ('E *quad.*'). It is clear that these expressions are comparable to our 'x^3' and 'y^2' at least in this regard: in Viète's system, if one multiplies, say, A *quadratum* by A, the result is A *cubum*. In such expressions 'A' designates not the value but only the root. Signs for Viète's known parameters, although they too take the form of a letter followed by a word indicating the species (e.g., 'B *plano*' or 'Z *solido*'), do not function in the same way. In a sign such as 'B *plano*' of the *logistice speciosa*, it is the sign 'B' alone that designates the known parameter; '*plano*' merely annotates the letter.[38] As required by the law of homogeneity according to which "homogeneous terms must be compared with homogeneous terms," it serves to remind the analyst that if, at the last stage in solving a problem, he turns geometer (rather than arithmetician), he must put for 'B' something of the appropriate "scale."[39] If the problem is arithmetical, any number can be put for B because among numbers there is no difference in scale,

[37] See Mahoney (1980, 148).

[38] This is widely, if at times only implicitly, recognized. See, for example, Bos (2001, 151), Mahoney (1973, 149), Boyer (1989, 305), and Smith (1925, 449 and 465).

[39] Viète introduces the law of homogeneity in chapter III of Viète (1646) claiming that "much of the fogginess and obscurity of the old analysts is due to their not having been attentive" to it and its consequences.

all being measured by the unit; but if the problem is geometrical then only a plane figure (for instance, a square or a rectangle) can meaningfully be assigned to B. Where Viète wishes to indicate the known parameter raised to a power, say the second, he writes 'B *quad.*'; if he wishes to indicate that a root raised to a power (say, the second) is planar, he writes 'E *plani-quad*'.[40]

Viète's two different sorts of letters, uppercase vowels for unknowns and uppercase consonants for known parameters of a problem, function in his symbolic language in essentially different ways. Vowels signify roots the powers of which are then indicated by the word that follows the vowel. Consonants signify the known parameter itself. The word that follows the consonant (e.g., '*plano*' or '*solido*') serves only to indicate the sort of figure that can be put for the letter at the last stage of the art in the case in which the problem is geometrical. The *logistice speciosa* serves in this way as a symbolic language that can be applied to both arithmetical and geometrical problems. It is, in this regard, quite like Eudoxos's general theory of proportions as developed in Book V of the *Elements*—though, we will see, with one essential difference.

Eudoxos's theory is general in the sense of applying generally to numbers and geometrical figures. It concerns itself not specifically with ratios of numbers or ratios of geometrical figures but more generally with ratios of any sorts of entities that can stand in the relevant relationships; it concerns numbers but not qua numbers because it applies equally to figures and motions, and it concerns figures and motions but not qua figures or motions because it applies equally to numbers. The theory is concerned with such objects insofar as they fall under a "higher universal," one that "has no name" (Aristotle, *Posterior Analytics* I.5). That is, it applies to such objects insofar as they belong to some genus, which has no name, of which *number* and *geometrical figure* are species much as a theory of mammals applies to cats and cows (among other things) insofar as they belong to a genus—one that does have a name, *mammal*—of which *cat* and *cow* are species.

Viète's *logistice speciosa* is not general in the way that Eudoxos's theory of proportions is general. Though it does in a way apply generally to both numbers and geometrical figures, Viète's *logistice speciosa* also "generalizes" over two very different sorts of operations. Whereas in Eudoxos's theory, the notions of ratio and proportion are univocal—precisely the same thing is meant whether it is a ratio or proportion of numbers or of geometrical figures that is being considered—in the Analytical Art, the notions of addition, multiplication, and so on, are not univocal: the arithmetical operations that are applied to numbers in the Analytical Art are essentially different from those applied to geometrical figures. In arithmetic, as Viète understand arithmetic, one calculates with numbers, each calculation taking numbers to yield numbers; in geometry (again, as Viète understand it) one constructs

[40] See, for example, the first of the "Two Treatises on the Understanding and Amendment of Equations," chapter 12. (In Theorem II, 'B *plani-quad*' should be 'E *plani-quad*'; the error is corrected in Witmer's translation (Viète 1646, 192).)

using figures, and in the cases of multiplication and division, and in the geometrical analogue of the taking of roots, the result of a construction is a different sort of figure from that with which one began (or even a different sort of entity altogether, namely, a ratio). Furthermore, in arithmetic the result of an operation can be merely determinable, as it is in the case of the root extraction of, say, two; in geometry, all results are fully determinate. There is in Viète's Analytical Art, by contrast with what we find in Eudoxos's theory, no genus to which numbers and figures belong such that they can be, for instance, added, multiplied, or squared. How, then, are we to read an expression of Viète's *logistice speciosa* such as 'A *quadratum* + B *plano*' given that there is no genus relative to which the mathematical operations (here, addition) can coherently be applied?

For Viète, as for the ancient Greeks, relations depend essentially on the objects that are their relata; there are no relations independent of the objects they relate. It follows that Viète can have no generic notion of an arithmetical operation, say, addition, that serves as the genus, as it were, of which arithmetical and geometrical addition are species. A sign such as '+' in Viète's *logistice speciosa* cannot signify either arithmetical addition or geometrical addition to the exclusion of the other; it cannot be merely equivocal or ambiguous; and there is no genus that might be signified instead. The only plausible reading of Viète's *logistice speciosa* is, then, a reading of it as a formal theory or uninterpreted calculus. The first stage of the Analytical Art, *zetetic*, takes one out of a particular domain of inquiry, either arithmetical or geometrical, into a purely formal system of uninterpreted signs that are to be manipulated according to rules laid out in advance, and only at the last stage, *exegetics*, are the signs again provided an interpretation, either arithmetical or geometrical. As Mahoney (1973, 39) explains,

> the elevation of algebra from a subdiscipline of arithmetic to the art of analysis deprives it of its content at the same time that it extends its applicability. Viète's specious logistic, the system of symbolic expression set forth in the Introduction is, to use modern terms, a language of uninterpreted symbols.

Bos (2001, 148) makes essentially the same point:

> While considering abstract magnitudes Viète could obviously not specify how a multiplication (or any other operation) was actually performed but only how it was symbolically represented. Thereby the "specious" part of the new algebra was indeed a fully abstract formal system implicitly defined by basic assumptions about magnitudes, dimensions, and scales . . . and by axioms concerning the operations . . . [of] addition, subtraction, multiplication, division, root extraction, and the formation of ratios.

Viète's *logistice speciosa* is not, then, a language properly speaking at all. It is an uninterpreted calculus, a tool that is useful for finding solutions to problems but within which (that is, independent of any interpretation that might be given to it) neither problems nor their solutions can be stated. Indeed, its usefulness is a direct function of its being an uninterpreted calculus. Because the *logistice speciosa* has no meaning or content of its own, the results that are derivable in it can be interpreted either arithmetically or geometrically. It is in just this way that, as Viète (1646, 32) proudly announces, "the Analytical Art claims for itself the greatest problem of all, which is

To solve every problem."

Descartes' symbolic language, we will see, is not such an uninterpreted formal system. Despite its superficial similarity to Viète's *logistice speciosa*, Descartes' symbolic language functions in an essentially different way. It is a fully meaningful language in its own right.

3.3 *Mathesis Universalis*

We know that Descartes was inspired to begin working on mathematical and physico-mathematical problems by the Dutchman Isaac Beeckman, whom he first met late in 1618 when Descartes was twenty-two. Already within a few months of that meeting Descartes had the idea of a new science.[41] As Viète had envisaged his Analytical Art, so Descartes envisaged his science as one that "would provide a general solution of all possible equations involving any sort of quantity, whether continuous or discrete, each according to its nature" (CSM III 2; AT X 157). But as indicated already in the unfinished *Regulae*, Descartes has something very different from Viète's Analytical Art in mind. Because, as he has come to think, "the exclusive concern of mathematics is with questions of order and measure and . . . it is irrelevant whether the measure involves numbers, shapes, stars, sounds, or any other object whatever," what he envisages is "a general science which explains all the points that can be raised concerning order and measure irrespective of the subject-matter," a *mathesis universalis* (CSM I 19; AT X 377–8). Whereas traditional mathematics, up to and including Viète's, concerns objects, that is, geometrical figures, kinds of numbers (conceived as collections of units), and so on, Descartes' new mathematics is to be a science of order and measure. What Descartes means, we will see, is that mathematics is now to be conceived as a science not of things, objects, but of the relations and patterns that objects can exhibit, of "the various relations or proportions that hold between these objects" (CSM I 120; AT VI 20).

We saw in section 3.1 that Galileo uses diagrams to exhibit the relationships between time, speed, and distance traveled that are involved in various kinds of motion. Descartes similarly exhibits relationships in diagrams, only now what are exhibited are arithmetical relationships among arbitrary quantities. In fact, what

[41] In a letter dated March 26, 1619, Descartes tells Beeckman that he has just, in the past six days, "discovered four remarkable and completely new demonstrations" and talks of producing "a completely new science" (CSM III 2; AT X 154).

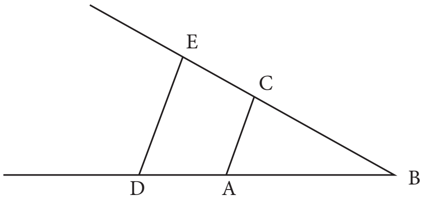


Figure 3.4 A graphic display of the relationship between two numbers and their product.

Descartes does is to employ two very different means of representing the subject matter of this new universal mathematics. In cases in which relations among arbitrary quantities are to be considered separately, they are to be taken, Descartes tells us, "to hold between lines, because I did not find anything simpler, nor anything I could represent more distinctly to my imagination and senses." Where they are to be considered together they are to be designated "by the briefest possible symbols." "In this way," he explains, "I would take over all that is best in geometrical analysis and in algebra, using the one to correct all the defects of the other" (CSM I 121; AT VI 20).[42]

We want, for example, to represent the relationship between two numbers and their product, that is, not two numbers and their product in a relation (of equality) but the relationship itself that holds generally between pairs of numbers and the number that is their product. This can be achieved graphically, as a relation among line lengths, where AB is to be understood as the unit length and DE is drawn parallel to AC (see Figure 3.4).

Because BE is to BD as BC is to BA (because angle B is common to both triangles and the lengths DE and AC are parallel), it follows that BE is the product of BD and BC: if BE:BD = BC:BA, that is, BE/BD = BC/BA, and BA is the unit length, then BE $\times$ 1 = BC $\times$ BD; that is, BE is the product of BC and BD. But, as Descartes goes on, this same relationship can also be expressed symbolically: where a is the length of BD and b that of BC, the product of the two lengths can be given as ab. Descartes claims, in other words, that the geometrical relationship between the line lengths that is presented in the above diagram is not merely *analogous to* but an *alternative expression of* that which is expressed symbolically. It is the relationship itself, which is common to the two means of expression, that is displayed both in the diagram and in symbols. And the same is true of the graphic and symbolic representations in Descartes' geometry of a sum, of the difference between two lengths, of the division of one length by another, and of a square root. Both the graphic display

[42] According to Descartes, the analysis of the ancients "is so closely tied to the examination of figures that it cannot exercise the intellect without greatly tiring the imagination," and the algebra of the moderns (perhaps Viète's in particular?) is "so confined to certain rules and symbols that the end result is a confused and obscure art which encumbers the mind, rather than a science which cultivates it" (CSM I 119–20; AT VI 17–18).

with lines and the symbolic display are a means of mapping or "making visual" the relations and patterns that are of concern in Descartes' universal mathematics.

In Euclid's geometrical practice, we have seen, diagrams formulate content in a way that is mathematically tractable. A drawn circle, for example, iconically presents the relation of parts that is constitutive of a circle. Just the same is true of a drawn triangle, or any drawn figure in Euclid. What is drawn, and what is seen in the drawing, is the content, conceived as parts in relation, of the concept of some sort of geometrical figure. Already in Galileo we find an essentially different conception of a drawing of, say, a triangle. Galileo uses drawn triangles to formulate not the content of the concept of a triangle but instead a relationship among measurable "dimensions" of space, time, and motion. Here we see Descartes doing something similar. Relying on fundamental geometrical features of triangles, and appealing to a given unit, he is able to formulate in a diagram the relationship that holds between two numbers, any two numbers, and their product. Exactly that same relationship is to be expressed symbolically, as ab, in the language of arithmetic and algebra as Descartes intends it to be read. What is exhibited both in the diagram and in the symbolic language is not an object (that is, a number or geometrical figure) but a relation that objects can stand in.[43]

In Viète's *logistice speciosa*, one abstracts from differences between geometrical and arithmetical objects. An expression such as 'A *quadratum* + B *plano*' can be interpreted either geometrically (as involving figures classically conceived) or arithmetically (as involving numbers classically conceived as collections of units). Independent of any interpretation, the expression is a mere form. Viéte's *logistice speciosa* functions as an uninterpreted calculus, one that can be interpreted either geometrically or arithmetically. What we have just seen is that in Descartes' geometry an essentially new mathematical notion is introduced, that of a geometrically expressible quantity that is, as numbers are, dimension-free. Just as operations on numbers yield numbers in turn, so operations on line segments in Descartes' geometry yield line segments in turn. That is why there is in Descartes' symbolism nothing corresponding to Viète's annotations '*plano*', '*solido*', and so on. In Descartes' *Geometry* a letter such as 'a' or a combination of signs such as '$(a + b)^2$' is not an uninterpreted expression that can be interpreted either geometrically or arithmetically; it is a representation of an indeterminate line length that in the case of a complex sign displays that length via a display of the relations that it bears to other lengths. Descartes' symbolic language is, then, always already interpreted. As Descartes employs them, letters and combinations of them, signify something in particular, namely, relations among line lengths (themselves conceived as representing arbitrary quantities), either those that are given or those that are sought.

[43] Chapter 1 of Grosholz (1991) also concerns itself with the method of Descartes' geometry, and its relationship to Euclid's geometry. More than is done here, she highlights limitations of Descartes' method.

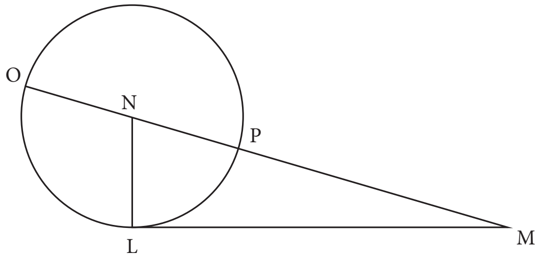


Figure 3.5 Diagram for finding the root of $z^2 = az + b^2$.

We have seen how Descartes represents both diagrammatically and symbolically the relationship of two numbers to their product. Now we need to see how this new form of inquiry focused on such relations can yield new results, for instance, the positive roots of quadratic forms. Suppose, for example, that $z^2 = az + b^2$. (As we do still today, Descartes uses letters from the beginning of the alphabet to represent given indeterminate quantities and letters from the end of the alphabet for the unknowns.) To find the root z we construct a right triangle NLM with LM equal to b and LN equal to $1/2a$, and then prolong MN to O so that NO is equal to NL (see Figure 3.5).

Because OM is to LM as LM is to PM, OM $\times$ PM = LM^2, that is, $z(z - a) = b^2$, or $z^2 - az = b^2$. The diagram thus formulates the relation that is expressed symbolically in '$z^2 = az + b^2$'. The desired root, then, is the length OM, that is, ON + NM, or $1/2a + \sqrt{(a^2 + b^2)}$. As Descartes soon discovered, this result can be generalized using an ingenious compass of Descartes' own devising.

Soon after his first meeting with Beeckman, Descartes began building compasses that would enable a mechanical means of computing the solutions to various problems. One such compass is that shown in Figure 3.6. In the *Geometry* Descartes describes its behavior—that is, the pattern of motions it generates—as follows.

> This instrument consists of several rulers hinged together in such a way that YZ being placed along the line AN the angle XYZ can be increased or decreased in size, and when its sides are together the points B, C, D, E, F, G, H all coincide with A; but as the size of the angle is increased, the ruler BC, fastened at right angles to XY at the point B, pushes towards Z the ruler CD which slides along YZ always at right angles. In like manner, CD pushes DE which slides along YX always parallel to BC; DE pushes EF; EF pushes FG; FG pushes GH, and so on. Thus we may imagine an infinity of rulers, each pushing another, half of them making right angles with YX and the rest with YZ. (Descartes 1637, 44, 47)

The compass is constructed so that the series of right triangles CYB, DYC, EYD, FYE, GYF, HYG, and so on are all similar: YB is to YC as YC is to YD, and YD is to YE, and YE is to YF, and so on. That is, YB/YC = YC/YD = YD/YE = YE/YF = YF/YG = YG/YH (and so on). If, now, we take YB to be the unit, and YC to be the unknown, then

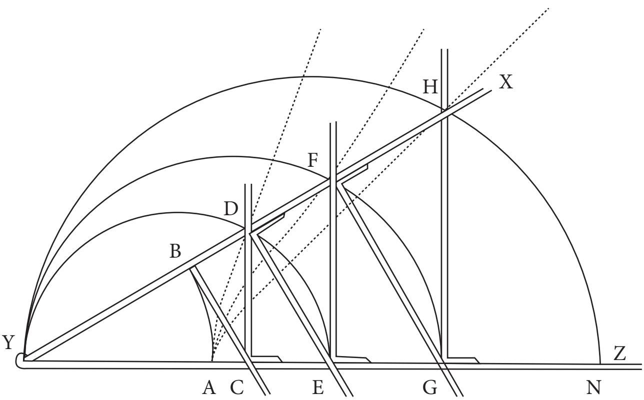


Figure 3.6 Descartes' compass.

this series is precisely the continuing proportional that Descartes discusses in Rule Sixteen of the *Regulae*, namely, $1{:}x = x{:}x^2 = x^2{:}x^3 = x^3{:}x^4 = \ldots$ Given that YE = YC + CE, that is, that $x^3 = x + \mathrm{CE}$, we can use the compass to solve arbitrary equations of the form '$x^3 = x + a$': for a given value of a, if one opens the compass to the point at which the length CE is equal to a (the unit length having been given by YB), then the length YC is the root that is wanted.[44]

Descartes' use of his compass exhibits a number of remarkable features. First, and most obviously, it requires abandoning the traditional understanding of roots, squares, and cubes as, literally, figures. "Squaring" a line in Euclidean geometry literally means making a square, and cubing a line makes a cube. Higher powers make no geometrical sense at all—as is reflected in the fact that there are no names for such higher powers other than, say, "the fourth power" (or as it was sometimes put, unintelligibly, "the square-square"). Descartes' conception of powers in terms of continued proportions replaces the traditional conception in terms of geometrical objects. Even the terminology of squares and cubes is, Descartes thinks, "a source of conceptual confusion and ought to be abandoned completely.... We must note above all that the root, the square, the cube, etc. are nothing but magnitudes in continued proportion which, it is always supposed, are preceded by the arbitrary unit" (CSM I 68; AT X 456–7).[45]

Notice further the generality of the solution that is marked by the use of two different sorts of letters, 'x' for the unknown and 'a' for the known parameter of the problem. Descartes emphasizes the value of such general solutions in Rule Sixteen of the *Regulae*: by using "the letters a, b, c, etc. to express magnitudes already known"

[44] This is one of the cases Descartes mentions in his letter of March 26, 1619 to Beeckman.

[45] By the time he was writing the *Geometry*, Descartes had given up the idea of a reform of language; he there employs the familiar expressions 'square' and 'cube', while making it clear that what he means thereby are the proportional magnitudes.

one abstracts "from numbers . . . or from any matter whatever" (CSM I 67; AT X 455–6). For example, "if the problem is to find the hypotenuse of a right-angled triangle whose sides are 9 and 12, the arithmetician will say that it is $\sqrt{225}$ or 15. We on the other hand will substitute a and b for 9 and 12 and will find the hypotenuse to be $\sqrt{(a^2 + b^2)}$, which keeps distinct the two parts a^2 and b^2 which the numerical expression conflates" (CSM I 67–8; AT X 456). It is in just this way, through the formulation of an expression such as '$\sqrt{(a^2 + b^2)}$', that one can see not only what the answer is but how it depends on the given data. In the general expression of the answer, '$x = \sqrt{(a^2 + b^2)}$', one exhibits the *relationship* of that answer to the given data, the way it relates to what is given.

In his mathematical practice, we have seen, Descartes is not interested in any traditional subject matter but instead in order and measure, that is, in the patterns the objects forming the subject matters of various disciplines exhibit. One such pattern is that displayed by the length of the hypotenuse of a right triangle relative to the lengths of the other two sides, the pattern exhibited in the equation '$x = \sqrt{(a^2 + b^2)}$'. More generally, as is made explicit in Rule Eighteen of the *Regulae*, Descartes is interested not in particular sums and products but instead in the arithmetical operations themselves, addition, subtraction, multiplication, division, and root extraction. That is, he is not merely abstracting from any actual instances of objects of various kinds (as Aristotle suggests mathematicians do). Nor is he dealing with an object that is itself somehow more abstract (as Plato's mathematicals and Forms seem to be). Nor, finally, is he abstracting from content altogether, as Viète does, to consider only a form.[46] Instead the whole discussion is effectively moved up a level, from talk about, or better formulation of, relata, that is, objects such as numbers and figures, to talk about, the formulation of, relations, the patterns such objects can be seen to exhibit. The function of the letters 'a' and 'b' in the equation '$x = \sqrt{(a^2 + b^2)}$' is to enable one to exhibit the precise mathematical relation that holds between the length of the hypotenuse of a right triangle and the lengths of the other two sides. The Cartesian geometer is in this way directed not on independently existing objects, whether sensible or intelligible, but instead on the relations, proportions, and patterns that such objects can display.

Perhaps it will be objected that relations are and must be empty, mere forms, independent of any reference to objects standing in those relations, that relations cannot be conceived antecedently to, and independent of, objects standing in those relations. In one sense this is right. Whatever Descartes' own view of the matter, we cannot *begin* with the conception of symbolic language that Descartes achieves, one

[46] Descartes' mathematics is sometimes read formalistically. See, for example, Gaukroger (1992). But this is an anachronism. As Hatfield (1986, 64) notes, although "the twentieth century mind" is tempted to read Descartes' notation purely formally, "Descartes scorned attempts to make words or symbols and formal rules for manipulating them primary; these are merely arbitrary sensory reminders for the content manifest in thought itself." "Descartes . . . despised formalism" (Hacking 1980, 170).

according to which the signs of the language—letters such as 'x' and 'y', and 'a' and 'b'. and symbols such as '+' and '='—function together to exhibit relations. Such a language is essentially late, possible at all only through a radical transformation in our primordial understanding as embodied in natural language. But although it is impossible to understand a symbolic language such as Descartes' within which to exhibit relations independent of any objects except against the background of natural language and the understanding of things that it enables, Descartes' language is, in its way, autonomous. That is, it is—or at least can, and did, become—a fully-fledged language embodying an understanding of the world, one that enables, we will see, a radically new mode of consciousness and intentional directedness on reality.

We have seen that between the thirteenth and sixteenth centuries a kind of a grid came to be laid over all reality, in particular, that both space and time came to be conceived as measurable dimensions. We also saw that Galileo had the further idea of using a spatial display not only to exhibit relations among, say, speeds over time (as Oresme had already done) but also to establish truths about the quantities standing in those relations. Descartes completes the thought by taking a visual display of lines to exhibit the relation itself. Descartes sees a drawing of, say, a right triangle, not as an *object* (even one that is somehow essentially general, or arbitrary) but instead as a presentation of one way, an especially interesting and revealing way, that measurable quantities can be *related* to one another. And having achieved this insight, he is able to understand the symbolic language of algebra as an alternative way of exhibiting this and other relations.

A Euclidean diagram appropriately used can reveal the relationship between, say, the squares on the sides of a right triangle. That same relationship, suitably reconceived as a relation among line lengths (themselves conceived as representative of arbitrary magnitudes), is exhibited symbolically as $a^2 + b^2 = c^2$, where c is the length of the hypotenuse. In his mathematical practice Descartes employs four (and only four) such basic reconceptualizations of traditional geometrical relations, all of which are utilized in his solution of the following problem in the *Geometry*.[47] Given the square AD and the line BN, the task is to prolong the side AC to E, so that EF, laid off from E on EB, shall be equal to NB (Descartes 1637, 188). Heraclides' solution, as given in Pappus's *Collection*, is the diagram displayed in Figure 3.7. That is, BD is extended to G, where DG = DN. Taking now the circle whose diameter is BG, it can be shown (by a chain of reasoning that need not concern us) that the point E that is wanted is at the intersection of that circle and AC extended as needed. As Descartes remarks regarding this solution, "those not familiar with this construction would not be likely to discover it."[48] His own approach is

[47] I am indebted to Manders (unpublished) for pointing this out.

[48] Already in the *Regulae*, Descartes complains that the ancient geometers' practice of demonstrating using diagrams, "did not seem to make it sufficiently clear to my mind why these things should be so and how they were discovered" (CSM I 18; AT X 375).

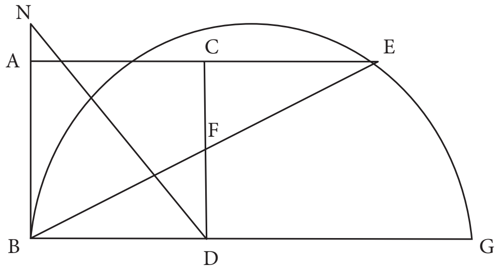


Figure 3.7 Heraclides' solution to the problem that given the square AD and line BN, one is to prolong the side AC to E so that EF, laid off from E on EB, shall be equal to NB.

methodical, a stepwise construction that eventuates in an equation the root of which solves the problem.

We know that BD equals CD because they are sides of one and the same square. Call that length, which is given by the terms of the problem, a. Let c be the length EF, and take the length DF as that which is sought, the unknown x. CF, then, is equal to $a - x$ because, by compositionality, CF + FD = CD. Because the triangles BDF and ECF are similar, we know that BF:DF :: EF:CF, that is, that $a - x{:}c :: x{:}\mathrm{BF}$. So, transforming this proportion into an equation, we know that $\mathrm{BF} = cx/(a - x)$. But because BDF is right at D, we also know that $\mathrm{BF}^2 = \mathrm{BD}^2 + \mathrm{FD}^2$; that is, $\mathrm{BF}^2 = a^2 + x^2$. Combining the two equations thus gives us $(cx/(a - x))^2 = a^2 + x^2$, or $x^4 - 2ax^3 + (2a^2 - c^2)x^2 - 2a^3x + a^4 = 0$. The root of this equation can then be constructed diagrammatically.

In solving this problem, Descartes appeals to four relationships that can be expressed both graphically in a diagram and symbolically: equality (of sides of a square), compositionality (of lines from their parts), proportionality (of triangles), and the relationship that is expressed in the Pythagorean theorem. And as we have just seen, the symbolic expression of these relations enables one to combine, by putting equals for equals, the given information systematically into a single equation and thereby to solve the problem without any need for the additional constructions that are involved in the Euclidean demonstration. In Descartes' geometry, one does not need to discover the diagram that is the medium of a Euclidean demonstration but only to express the given information in symbolic form and then combine it all (by putting equals for equals) into a single, soluble equation. It is just this that enables Descartes to claim that he has a *method* for the discovery of truths in mathematics. Although there is no method by which to discover the diagram that is needed in a Euclidean demonstration, once we conceive the problem symbolically—the diagram not as an iconic display of objects of various sorts in relations but as a presentation of relations and proportions that can equally well be expressed algebraically—Descartes can show us how to combine the given information symbolically in a single equation and thereby to solve the problem.

Before Descartes, although there were mathematical geniuses able to discover the diagrams that are needed to demonstrate various theorems and problems, there was no general method by which to solve such problems. What Descartes discovered, through the use of his symbolic language, was exactly that, a method by which systematically to solve traditional problems of geometry, even some, such as the Pappus problem, that the ancients were unable to solve.[49] As Poincaré would later remark, "before Descartes, only luck or genius allowed one to solve a geometrical problem. After Descartes, one has infallible rules to obtain the result; to be a geometer, it suffices to be patient."[50] Little wonder, then, that Descartes saw in his method a model for all knowing.[51]

3.4 The Order of Things

Descartes' understanding of the sorts of problems Viète considers is essentially different from Viète's own understanding of them insofar as Viète remains oriented on objects, whether geometrical or arithmetical, and Descartes focuses directly on relations that objects can stand in. But there is a further difference between them as well. Whereas Viète systematically ignores "indeterminate" problems, that is, problems in two (or more) unknowns, Descartes does not. Already in his letter to Beeckman of March 26, 1619 he envisages being "able to demonstrate that certain problems involving continuous quantities can be solved only by straight lines or circles, while others can be solved only by means of curves produced by a single motion, such as the curves that can be drawn with the new compasses . . . and others still can be solved only by means of curves generated by distinct independent motions which are surely only imaginary" (CSM III 2–3; AT X 157).[52] But Descartes did not at that point know exactly how this was to go. Although he knew how to use his compass to find the roots of cubic equations, he did not yet know how to describe a curve, conceived in terms of the motion of a point, in an equation in two unknowns. He had the idea of conceiving spaces, geometrical objects, relationally, but not yet the idea of considering space itself as an antecedently given whole of relations. On November 10, 1619 he seems to have discovered precisely that, and thereby the foundations of his marvelous science. From conceiving space in terms of the (measurable) relative locations of objects, Descartes came, through a thoroughgoing figure/

[49] Descartes compares his method to the method of calculating in Arabic numeration in the *Discourse on Method*. Much as "if a child who has been taught arithmetic does a sum following the rules, he can be sure of having found everything the human mind can discover regarding the sum he was considering," so "the method which instructs us to follow the correct order, and to enumerate exactly all the relevant factors, contains everything that gives certainty to the rules of arithmetic" (CSM I 121; AT VI 21).

[50] Henri Poincaré, Preface to his *Oeuvre de Laguerre*, vol. I, quoted in Manders, (unpublished). In fact this is not true in the most interesting cases.

[51] As Klein (1968, 123) notes, although (as we saw) the ancient Greeks faced a problem of the "generality" of its objects in light of its methods, early modern mathematics "determines its objects *by reflecting on the way in which those objects become accessible through a general method*." As we will see in Chapter 4, this thought is fully explicit in Kant's Copernican turn.

[52] In the letter Descartes compares these three cases to arithmetical problems that can be solved by means of, respectively, rational, irrational, and "imaginary" numbers. In the last sort of case, he thinks, there is in fact no solution: we can imagine how to solve such problems but cannot actually solve them.

ground switch, to conceive space as an antecedently given irreducible whole of possible positions, and so as prior to and independent of any objects. It is this conception of space that is the key to understanding and expressing curves as equations.

As already noted in Chapter 1, we (unlike other animals) not only learn various routes through a terrain but also synthesize all the various routes we have learned into one unified conception of the layout of the land as a whole. We learn "to conceive of some very large spaces as integrated wholes rather than piecemeal as they are experienced" (Tversky 2003, 72). Such integrated wholes can be depicted graphically in maps that show various landmarks in their relative locations to one another as if seen from above. Furthermore, because it is achieved by synthesizing into one integrated whole one's procedural knowledge of routes from landmark to landmark, the conception of space that is exemplified in such a map is constitutively object based: "the things in space are fundamental" on such a conception (Tversky 2003, 67). By the seventeenth century, maps showing the relative locations of things were being widely produced for various purposes, and were increasingly being drawn to scale.

And now something remarkable can happen. One can make a gestalt shift, a global figure/ground switch that reveals space as an *antecedently given whole* of possible positions within which objects, landmarks, may but need not be placed, each independent of all the others. Such a view of space, which is not literally a *view* at all, is not object based. On this conception, not objects but space itself "is the foundation" (Tversky 2003, 67). Whereas on the first conception the map is a kind of picture or image of the layout of the land, a kind of "bird's eye" or bottom-up (because object-based) view of it, on this second, top-down, conception, the map records the locations of things in space (each independent of all the others) not as it is experienced in everyday sensory perception but as it is conceived or grasped in thought. Beginning with the piecemeal acquisition of routes, through the integration of those routes into a single whole of the relative locations of things, one in this way achieves finally (through a global figure/ground gestalt shift) a conception of space as an antecedently given whole of possible locations. Although not on the first conception of it, space on this second conception is intelligible prior to and independent of any reference to objects. It is a given whole within which objects can, but need not be, located. This is precisely the conception of space that Descartes achieved.[53]

[53] Høyrup (2004, 129–30) notes that Aristotle considers what Høyrup suggests is the modern view of space (and time) as prior to the things in it (what happens) but "he rejects them in the *Physics* because he cannot make philosophical sense of them" (2004, 130). Even the ancient Greeks, that is to say, could in a way make sense of the idea that space is prior to the things that are found in it (and similarly for time); for, after all, if I take things away, the place for them remains. But *pace* Høyrup, this is not yet the modern conception of space. On the modern conception, space as a whole is prior to and wholly independent of any objects that might be located in space. This conception cannot be achieved simply by imagining things being taken away but requires a thoroughgoing gestalt switch of one's view of a map of things in their relative locations.

Consider again Descartes' compass as shown in Figure 3.6. And imagine now that the angle XYZ is increased by raising the arm XY. In that case, as Descartes explains, "the point B describes the curve AB, which is a circle; while the intersections of the other rulers, namely, the points D, F, H describe other curves, AD, AF, AH, of which the latter are more complex than the first and this more complex than the circle" (1637, 47). The task is to find some means of expressing the curves so described in Descartes' new symbolic language, and it is precisely here that Descartes invokes his essentially new idea of space as an antecedently given whole within which the needed points can be plotted. He envisages his compass, and the movements it makes, in what we know of today as Cartesian space. Given space so conceived together with the (arbitrary) designation of certain imagined lines in that space as "principal lines" (as Descartes calls them) to which all others are to be referred, it is easy to find the equation governing the motion of the curve AB. This motion describes a circle whose radius is r = YA = YB. Dropping a perpendicular from the point B to the line AN (one of the principal lines), at a point we can label W, and letting that perpendicular BW = y and YW = x, the motion of the point B is along the path expressed in the equation $x^2 + y^2 = r^2$ in two unknowns. A circle, which had seemed to the ancients to be a certain plane figure, a particular sort of two-dimensional object with its characteristic nature, is now to be understood by appeal to the lawful relation holding between the two unknowns, x and y, as expressed in the equation that governs the motion of a point in Cartesian space.

Now we need an equation for the curve described by the movement of point D. Notice first that YC and CD together determine the point D because $YC^2 + CD^2 = YD^2$. So we let YC = x, and CD = y. What we want is an equation determining the relationship of the two values, y to x. Let YA = YB = a. Because we know that YD is to YC as YC is to YB, that is, that YD:YC = YC:YB, or $YD/x = x/a$, we know also that $YD = x^2/a$. But we know as well that $YD^2 = x^2 + y^2$ because triangle YCD is right at C and YC = x and CD = y. It then follows that $(x^2/a)^2 = x^2 + y^2$, from which it follows that $x^4 = a^2(x^2 + y^2)$. We have the desired equation for the curve AD, the equation expressing the relationship between the lengths x and y that determines the point D at any moment on its trajectory as the compass is opened.

To find the equation for the curve traced out by the moving point F is a little more complicated, but essentially similar. Here we let YE = x and EF = y. Again YA = YB = a. We know that YF:x = x:YD. So $YD = x^2/YF$. But we also know that x:YD = YD: YC. So by substitution, $x: x^2/YF = x^2/YF: YC$, or $YC = (x^2/YF)^2/x = x^3/YF^2$. But we also know that YD:YC = YC:a. In other words, $x^2/YF: x^3/YF^2 = x^3/YF^2: a$, or $ax^2/YF = (x^3/YF^2)^2$. This, by familiar algebraic operations, yields $YF = \sqrt[3]{(x^4/a)}$.[54] But $YF^2 = x^2 + y^2$; so $\sqrt[3]{(x^8/a^2)} = x^2 + y^2$, or $x^8 = a^2(x^2 + y^2)^3$. An exactly analogous chain of reasoning yields the equation for the path of point H, and any further points one

[54] We begin with $ax^2/YF = (x^3/YF^2)^2 = x^6/YF^4$, from which it follows that $ax^2 = x^6/YF^3$. So $YF^3 = x^6/ax^2 = x^4/a$. So, $YF = \sqrt[3]{(x^4/a)}$.

cares to consider as further rulers are added to the compass. In every case, as Descartes says (1637, 47), the description of the curve can be conceived "as clearly and distinctly as that of the circle, or at least that of the conic sections." Much as the Arabic numeration system, by contrast with (say) Roman numeration, enables one to solve arithmetical problems of arbitrary complexity simply by following rules in the proper order, so here again we see that Descartes' new way of approaching problems in geometry, by contrast with that of Euclidean geometers, enables one to solve geometrical problems of arbitrary complexity simply by following rules in the proper order. And the method clearly generalizes to problems in more than two unknowns. It provides just the understanding that enables Descartes to claim, in the opening sentence of the *Geometry* (1637, 2), that "any problem in geometry can easily be reduced to such terms that a knowledge of the length of certain straight lines is sufficient for its construction."

In ancient Greek mathematics, problems are classified according to the sorts of objects that need to be invoked to solve them. Plane problems are those that require recourse only to lines and circles, conceived as self-subsistent geometrical objects, in their solution. Next are solid problems, so called because their solutions require appeal also to "conics," that is, to hyperbolas, ellipses, and parabolas, which are understood as intersections of a cone (that is, a solid, a figure having length, breadth, and depth, the limit of which is a surface) and a plane. Conics, on this view, are plane figures and so in that respect like circles, but unlike circles they are intelligible only by reference to a solid figure, a cone, and are for this reason an essentially different sort of plane figure. The last sorts of problems are the so-called line problems that require appeal to "lines"—that is, loci of points to which no known figures, or boundaries of figures, correspond—in their solutions. Because such lines do not form the boundaries of any figures, they were regarded with deep suspicion by ancient geometers; they were taken to be mechanical rather than properly geometrical.

In Book III of his *Geometry*, Descartes continues this traditional line of thought; he classifies traditional determinate construction problems by the ancient criteria. In Book II, however, a new classification is given, a classification not of problems but of curves. According to this classificatory scheme, the simplest class of geometrical curves includes the circle, the parabola, the hyperbola, and the ellipse because all these curves are given by equations the highest term of which is either a product of the two unknowns or the square of one: they are one and all expressible in an equation of the form: $ax^2 + by^2 + cxy + dx + ey + f = 0$.[55] Whereas on the ancient view, conics are essentially different from circles, from Descartes' perspective in

[55] Unsurprisingly, Descartes also rejects the traditional distinction between mechanical and properly geometrical curves. According to him, mechanical curves are those that "must be conceived of as described by two separate movements whose relation does not admit of exact determination" (1637, 44). Because the relationship between the two separate movements cannot be exactly determined, Descartes thinks, the curves themselves cannot be exactly and completely known.

Book II, they are essentially alike. And they can be seen to be alike because a geometrical curve, from being conceived as the boundary of a figure, is now to be conceived instead as a curve all points of which "must bear a definite relation to all points of a straight line," where this relation in turn "must be expressed by means of a single equation" (Descartes 1637, 48). As Descartes conceives it, a curve is not an edge of a thing but instead the locus of points in space, where space is conceived in turn as an antecedently given whole of possible positions. Such a curve can be traced by a continuous motion but it is not constituted by the relative positions of points on it; it is constituted by the relationship expressed in an equation, that is, by the location of each point, independent of all the others, directly in space (relative to some arbitrarily given principal lines). From the perspective that Descartes provides, a problem such as that posed by the sort of quadratic equation that was the focus of Viète's interest, can be thought of as a limit case, with one unknown set equal to zero, of an indeterminate equation, for example, $x^2 + ax = b^2$ as the limit case of $x^2 + ax - b^2 = y$, that in which y is set equal to zero. As Descartes writes to Mersenne regarding the account of curves he develops in Book II of the *Geometry*, it is "as far removed from ordinary geometry, as the rhetoric of Cicero is from a child's ABC" (CSM III 78; AT I 479).

Descartes' new universal mathematics was to be a science of order and measure, and we saw in section 3.3 the ways Descartes is able to exhibit various relations of measure, for instance, that of two quantities to their product. But that is not yet to have a science of order. It is only in Descartes' account of curves conceived as the paths of points moving through Cartesian space, paths that can be expressed in equations in two unknowns, that we see how this new science is to be a science of order. Already in Rule Ten of the *Regulae*, Descartes enjoins us "methodically [to] survey even the most insignificant products of human skill, especially those that display or presuppose order" (CSM I 34–5; AT X 403). And as is explained in the ensuing discussion, "the simplest and least exalted arts . . . in which order prevails" are "weaving and carpet-making, or the more feminine arts of embroidery, in which threads are interwoven in an infinitely varied pattern." These arts, together with "number games and any games involving arithmetic," "present us in the most distinct way with innumerable instances of order, each one different from the other, yet all regular." "Human discernment," the discussion concludes, "consists almost entirely in the proper observance of such order." In the *Geometry* Descartes provides us the means to understand such order mathematically.

Something exhibits order, on Descartes' account, if it exhibits a pattern not of meaning, significance, or fittingness, but as the expression of a rule. As the pattern discernible in a woven cloth, by contrast with that discernible in, say, a free-hand drawing, is created by following a rule governing how at each stage the various threads are to be woven together, so order more generally is achieved by following a rule. The path traced by a moving point according to the rule expressed in an equation in two unknowns provides the paradigm of this conception of order.

The equation gives the rule that governs the movement of the point and thereby underlies the pattern that is exhibited in the curve that is described, or marked out, by the moving point.

Much as Descartes considers relations independent of any objects that might stand in those relations, so in his account of curves, he considers the laws governing the motions of points independent of those points and the visible traces they generate. Such a law is not *in* the objects it governs as a principle of their motion; and it is not a generalization about objects derived from some insight into those objects. The law is *independent* of the objects it governs, something in its own right that can be expressed, using two or more unknowns, in the symbolic language of algebra; and it is grasped in pure thought.[56] The law in this way both underlies and explains the appearance of, say, circles, their characteristic symmetries. Circles, that is to say, are not for Descartes, and for modern mathematics more generally, sensory objects, that is, objects with a characteristic look and feel, as Aristotle had claimed and ancient mathematicians assumed. They are instead (so Descartes thinks) *purely intelligible* objects, objects whose essences are given by equations that are grasped by the pure intellect independent of any images, and indeed of our sense organs generally. No matter what kind of body one has, even if one has no body at all, one can (Descartes holds) grasp the essence of a circle as Descartes conceives it by grasping the law that underlies (and explains) its visual representation.

As should be clear, this conception of order as what is subject to a law is radically different from the ancient conception of order. The ancient conception of order is in terms of what is fitting or appropriate, the relative significances of things; it is hierarchical and teleological. Furthermore, on the ancient view, to come to what Taylor calls self-presence just is to grasp this order. "On this [the ancient] view the notion of a subject coming to self-presence and clarity in the absence of any cosmic order, or in ignorance of and unrelated to the cosmic order, is utterly senseless: to rise out of dream, confusion, illusion, *is just* to see the order of things" (Taylor 1975, 6). Similarly, I think Descartes would say, to rise out of dream, confusion, illusion, *is just* to see the order of things, only what Descartes means by 'order' is not what the ancients meant. What Descartes means by 'order' is *law-governed*, according to a law, where a law is to be understood on the model of an algebraic equation in two (or more) unknowns that governs the motion of a point in Cartesian space. Descartes in this way realizes a radically new order of intelligibility, a conception of things unfolding not as an expression of their natures but under law. Descartes' new science of order is first and foremost a science of laws.

[56] Or so Descartes would claim. In fact, for reasons that will eventually become clear, the aspiration of early modern mathematics to be the work of the pure intellect will be fully realized only with the nineteenth-century revolution in mathematics that is the topic of Chapter 5. Only with that second revolution will the concepts of mathematics be stripped of *all* the sensory content that at first attaches to them.

3.5 Descartes' Metaphysical Turn

On November 10, 1619, Descartes had an epiphany, one that according to his biographer Baillet revealed to him "the foundations of a marvelous science" (CSM I 4, n. 1). I have suggested that what he realized that day (through a thorough-going figure/ground gestalt switch of our first, everyday conception of space in terms of the relative positions of objects) was what is now known as Cartesian space, space as a given whole of possible positions. Within a few months Descartes began writing his *Regulae*.[57] That work, however, was abandoned in 1628. The following year Descartes began working on *Le Monde*, and it was around this time that Descartes seems to have formed his distinctive conception of reality and our knowledge of it. Here we focus on four key elements of this new metaphysics: (1) Descartes' conception of mind as itself a substance, *res cogitans*, (2) his identification of body with extension, the subject matter of mathematics, (3) his account of the pure intellect as an autonomous faculty operating independently of the senses, and (4) his understanding of the eternal truths of mathematics as freely created by God. As we will see, although Descartes had all the essentials of his new mathematical practice already in November of 1619, it would take him a decade of work on the *Regulae* before he would come to conceive his new mathematical practice as a *purely* intellectual enterprise, one that in no way involves the senses, the imagination, or therefore, the body. Once he did have this conception of the pure intellect, the rest of his metaphysics quite naturally followed. Descartes' mathematics was in this way at once the foundation for and the catalyst of Descartes' radically new metaphysics.

The science of mathematics as Descartes understands it takes as its subject matter not objects but instead the relations and proportions that can obtain among objects, where these relations and proportions are exhibited in the symbolic language of arithmetic and algebra. Nevertheless, Descartes holds in the *Regulae*, the intellect should always be aided by the imagination; although expressible in the formula language of mathematics, mathematical entities must also be capable of graphic expression. Around 1628 Descartes discovered (so he thought) that imagination is not needed in mathematics, that the pure intellect is an autonomous faculty of knowing. Certainly we can imagine, that is, form images of, some mathematical entities—in Meditation Six, Descartes gives as examples, a triangle and a pentagon—but in other cases, say, that of a chiliagon, we cannot. And yet in every case, we can have mathematical understanding and knowledge as expressed in and mediated by the symbolic language of arithmetic and algebra. It follows that, contrary to the claims of the *Regulae*, imagination is not needed in mathematical practice but only the pure intellect.[58] And as Descartes eventually came to realize, this new insight changes everything. Descartes' new mathematical practice in this way ushers in a new metaphysics.

[57] I here follow Schuster (1980).

[58] Again, this actually is, at this moment in history, only an aspiration.

In the *Regulae* Descartes argues that mathematical practice should utilize images on the grounds that the intellect, reflecting in the absence of an image, can mistakenly separate what is in fact inseparable: "we generally do not recognize philosophical entities of the sort that are not genuinely imaginable . . . henceforth we shall not be undertaking anything without the aid of the imagination" (CSM I 59; AT X 442–3). As is made manifest in the subsequent discussion, the problem is that possibilities can be discovered by the intellect alone that are not real possibilities, that are shown to be unreal by an image: "even if the intellect attends solely and precisely to what the word denotes, the imagination nonetheless ought to form a real idea of the thing, so that the intellect, when required, can be directed towards the other features of the thing which are not conveyed by the term in question, so that it may never injudiciously take these features to be excluded" (CSM I 61; AT X 335). Though through the intellect alone one might discover what is logically possible, not all logical possibilities are real possibilities. Images, Descartes thinks at this point, help us to discover what is necessary despite not being logically necessary.

Descartes claims in the *Regulae* that by forming images of things one can avoid the error of taking to be separable what is in fact inseparable. For example, one might, using the intellect alone, determine that extension is not body and on that basis mistakenly conclude that there can be extension without body. Such a mistake is avoided, on the *Regulae* account, by one's forming a real idea of an extension in the imagination and discovering on that basis that it is impossible to form an image of extension that is not also an image of body. The distinction between extension and body is *only* a distinction of the intellect. There cannot actually be extension without body—which is why a vacuum is impossible according to Descartes.[59] That extension requires a body is thus an instance of the sort of judgment Kant would later describe as synthetic a priori, of a judgment that is necessary but not logically necessary, not analytic (by logic alone).

According to the *Regulae*, reason cannot derive the necessary truth that there is no extension without body from the concept of extension alone but must in effect go outside the concept of extension to an image in order to see the necessity of the connection between extension and body. But when Descartes realizes that his new mathematical practice does not require the use of images, when he realizes that his new system of written signs enables him to discover truths independently of his ability to form images, he needs some other way of constraining the intellect, and he needs this because not everything that is logically possible is really possible. This,

[59] "The impossibility of a vacuum, in the philosophical sense of that in which there is no substance whatsoever, is clear from the fact that there is no difference between the extension of a space, or internal place, and the extension of a body. For a body's being extended in length, breadth and depth in itself warrants the conclusion that it is a substance, since it is a complete contradiction that a particular extension should belong to nothing; and the same conclusion must be drawn with respect to a space that is supposed to be a vacuum, namely that since there is extension in it, there must necessarily be substance in it as well" (CSM I 229–30; AT VIII a 49).

then, provides grounds for thinking that God freely creates the eternal truths, the essences. These truths are not logically necessary. It is not, for example, analytic or logically necessary that extension should be the extension of a body; but Descartes thinks that it *is* nonetheless necessary that extension be the extension of some body. And, on his mature account, it is God who is the source of this non-logical necessity.[60] According to Descartes' mature account, we discover the necessary relationship between being extended and being a body using only the pure intellect, by careful examination of our clear and distinct idea of extension, an idea that is created by God and instilled in us as his creatures, and seeing its necessary, though not logically necessary, relationship to the idea of body.

Much the same is true, Descartes thinks, of other fundamental features of reality. We have innate ideas of various simple natures, for instance, substance, duration, order, thinking, doubt, and so on, on the basis of which we can discover, by thought alone, both the essence and fundamental laws of nature and the essence and fundamental laws of the mind, that is, how inquiry ought to be conducted. (See Marion 1992.) Henceforth, not experience, history, and tradition but pure thought is to be the primary and most fundamental means to knowledge.[61] The only authority is that of reason.[62]

Descartes came to his new conception of the science of mathematics through a fundamental and radical reorientation in his understanding not only of what is displayed in a diagram, a drawn figure, but of space itself. Descartes' conception of space is, furthermore, essentially late, possible at all only through a metamorphosis of an earlier, object-based conception. But Descartes takes this new orientation to reveal a cognitive capacity that is always already available to a thinker, something always already there to be discovered. And he explains the fact that this capacity had not before been recognized by appeal to the simple fact that we begin our lives as children

[60] What is logically necessary, on this account, is so even for God. It follows that some necessary truths are not created by God. That God exists, for instance, is presumably logically true, grounded on the (alleged) fact that God is logically possible and if logically possible then (by logic alone) also actual, hence necessary. Such a truth is not created by God. Similarly, it is not in God's power to make something that is a circle at the same time not be a circle because that is logically impossible. But it is in God's power to have made non-logical necessary truths about circles, such as that all radii are equal, be in fact false in much the way we think that it can be false (given certain assumptions) that the sum of the angles of a triangle equals two right angles. In a letter to Mersenne, May 27, 1630, Descartes writes that "God was free to make it not true that all the radii of the circle are equal—just as free as he was not to create the world" (CSM III 25; AT I 152). Whereas Suárez had claimed that God is the author of the existence of things but not also of their essences, Descartes now claims that God created both essences and existence. See Marion (1998, 273–5).

[61] See Garber et al (1998). As they note, "Descartes' explicit call for the complete rejection of learning and tradition had persuaded many Cartesians that they, at least, now possessed a permanent foundation for philosophy that no mere tradition could hope to supply. Even the tradition of Christian Epicureanism which Gassendi had established failed to offer such a guarantee. In the ensuing competition between Gassendists and Cartesians, not only would the future of atomism be affected, but the relevance of history and tradition to philosophical inquiry would also be decided" (1998, 587–8).

[62] But even reason will sometimes demand that experiments be done and observations made. See Larmore (1980).

lacking the full use of reason and so become accustomed to relying on our senses and traditional beliefs rather than on reason alone: "we were all children before being men and had to be governed for some time by our appetites and teachers" (CSM I 117; AT VI 13). Neither, Descartes thinks, can be trusted. Furthermore, in childhood the mind is "so closely tied to the body," so completely immersed in it, that one comes to confuse the two, attributing to bodies that which can belong only to mind and to the mind that which can only belong to bodies (CSM I 218; AT VIII a 35; also CSM III 188; AT III 420). From the perspective Descartes has achieved according to which mind and body are wholly separate, radically distinct *sorts* of things, the scholastic Aristotelian philosophy that he was taught as a student is subject to exactly this error in supposing that there are substantial forms and real qualities attaching to corporeal substances "like so many little souls in their bodies" (CSM III 216; AT III 648).

Because in childhood one accepts many principles "without ever examining whether they were true" (CSM I 117; AT VI 14), and inevitably so given that we do not have the full use of reason as children, once in one's life, Descartes thinks, one ought to submit all one's beliefs to the strictest rational scrutiny so as to discover how knowledge is possible. The "greatest benefit" of the "extensive doubt" of the first meditation "lies in freeing us from all our preconceived opinions, and providing the easiest route by which the mind may be led away from the senses" (CSM II 9; AT VII 12). Hyperbolic doubt purifies the mind, so that the pure intellectual with its innate ideas might shine forth. It sweeps away all our childish beliefs, rids us of our prejudices about the world and the means by which we know it, and thereby clears the ground so that we may come to grasp through the pure intellect how things really are.

Descartes' doubts are presented in the *Meditations* as self-standing. They are to serve to purify the intellect of the various prejudices accumulated in childhood and through one's education. But it is hard to see how this is supposed to work insofar as Descartes' radical doubts, in particular his doubts about the existence of the external world, are barely intelligible on the ancient understanding of being and the ancient conception of our intentional directedness on reality. They do not give someone who is not already a Cartesian compelling reason to doubt in the ways Descartes enjoins us to doubt. (See Broughton 2002.)[63]

Nor can we understand Descartes' discovery of the pure intellect and all that it entails by appeal to his rejection of Aristotelian philosophy and its mode of explanation in terms of substantial forms.[64] First, as already noted, Descartes had rejected school philosophy long before he became a Cartesian for whom the mind, the pure

[63] Burnyeat (1982, 42) suggests that Descartes' doubts are possible because his inquiry is strictly methodological but it is not at all clear that this really can explain what needs to be explained, namely, how it could be so much as intelligible that one might seriously doubt the existence of the external world while maintaining one's apparent experience of it.

[64] Garber (1986) seems to suggest as much. No mention is made of the many non-Cartesians who also rejected the school philosophy.

intellect, is wholly separate from the body and an autonomous faculty of knowing. Along with many others, he was at first a mechanist without being a Cartesian. And others remained atomists and (quasi-Aristotelian) empiricists, even after Descartes himself had made his metaphysical turn. They could remain atomists because Descartes in fact had no good argument against atomism that did not depend in turn on his (anti-empiricist) conception of the pure intellect. The atomist could agree, for example, that there is no mathematical atom—that in mathematics quantity is infinitely divisible—because the atomist could distinguish, as, for instance, Gassendi does, though in a way Descartes' epistemology disallows, between mathematical atomism and natural or physical atomism. Nor could Descartes argue that atomism is false because God can divide even physical atoms; all the atomist needed to claim was that no *natural* force could divide the atom.[65] Rejecting Aristotelian hylomorphism in favor of a more mechanistic philosophy may be necessary for Descartes' conception of the pure intellect, but it is far from sufficient.

Nor, finally, can Descartes' discovery of the pure intellect be explained by appeal to the newly emerging anti-Aristotelian mode of explanation in terms of laws. The reason is simple: the relevant notion of a law—namely, that of a law of motion as expressed in a mathematical equation—is due to Descartes himself, due in particular to his advances in mathematics. Although both Descartes and many of his contemporaries had already rejected both the traditional hylomorphic conception of nature and the traditional conception of explanation by appeal to Aristotelian forms, only Descartes succeeded in developing the alternative to it that was needed.[66] The only viable explanation of the fact that Descartes became the first Cartesian philosopher is the fact that he was first a Cartesian mathematician. It was Descartes' new mathematical practice together with the new mode of intentionality it engendered that enabled Descartes to become a Cartesian metaphysician.

According to the classical scholastic Aristotelian view that Descartes aims to supplant, one's conceptions of things are inevitably conceptions of existing things because ultimately those conceptions are themselves inextricably tied to one's perceptual experiences of the relevant objects. Essence, on such a view, is not prior to existence. This conception is furthermore fundamental to natural language; our first language is and must be one that speaks of those things in the environment of which we are perceptually aware. And it is this everyday perceptual experience of things through the medium of natural language that is the model for all cognition in scholastic Aristotelian philosophy. Even the "pure intellect," for the scholastic Aristotelian, grasps

[65] See Garber (1992, 123–5).

[66] As Milton (1998, 686) writes, we find already in Bacon's writings that "the old theory of substantial forms had been at least officially abandoned; the intention was to replace it by a theory of natural laws, but the precise character of the explanations involved remained obscure even to Bacon himself.... It was that other reformer of the sciences, René Descartes, who made the decisive innovation, by formulating and bequeathing to his successors the vision of a science of moving bodies in which laws of nature, conceived quite specifically as laws of motion, were the most fundamental principles of explanation."

only what is mind independent; even an "intelligible species" is, as a sensible species is in the case of perception, a means by which the intellect understands what is outside the mind. All thought, on the traditional conception of it, is constitutively world directed and world involving. That the "external world" might not exist may have been conceivable as an abstract, academic possibility for the ancients. It was not in any sense a real possibility, and ancient skeptical arguments do not consider it.

But if, as Descartes' new mathematical practice seemed to show, the mind can make discoveries wholly independently of any relation to anything outside the mind, simply by reflecting on its own ideas without even the assistance of the imagination, then both the scholastic Aristotelians and the anti-Aristotelian atomistic empiricists had to be wrong to hold that knowledge of existence, which is achieved through sense experience, is prior to knowledge, if any, of essences. Instead, "according to the laws of true logic," Descartes came to think, "we must never ask about the existence of anything [never ask if it is, *an est*] until we first understand its essence [what it is, *quid est*]" (CSM II 78; AT VII 107–8).[67] Much as we have, Descartes thinks, an idea of space prior to and independent of any and all sensory experience, so essences generally are to be conceived to be prior to and independent of any objects that might instantiate those essences. Descartes' new mathematical practice seemed in this way to reveal not merely a method of discovery, but the mind itself as something in its own right, independent of and wholly different from any other thing—save for God.

On the ancient view, perceptual experience is the model for all intellection. Even pure thought is constitutively world-directed in the way that perception is; and it is possible at all only given our everyday perception of things. Descartes inverts this picture by beginning with the pure intellect as revealed in his new mathematical practice, and then modeling perceptual experience on that form of cognition. Even perceptual experience is not, on Descartes' new account as outlined in the second meditation, inherently object involving. Furthermore, because in Descartes' new mathematical practice the pure intellect is focused not on objects given to sense or to thought but instead on relations and patterns, its "objects" (that is, that on which it is trained) cannot be things outside the mind, that is, objects such as material spheres and cubes. Its "objects" are the patterns and relations that are now held to *underlie* the spherical or cubic appearance of some objects, that is, what it is to be a sphere or cube, an *essence*. The notion of an essence is thereby completely transformed. From being what is essential to some actually existing object, that which makes it to be what it is and so to be at all, essence has become a purely mental entity, a *meaning*, or as Descartes calls it, an *idea*, something that can be directly grasped by the pure intellect and is, on Descartes' new account of cognition, the means by which we understand anything at all. Much as time had come to be divorced from events in Galileo's

[67] This is a central theme in Secada (2000). See also Carriero (1986).

physics and space from our experience of objects in Descartes' mathematics, so "essence ... is divorced from the object of reference" in Descartes' metaphysics and wedded, not to the word, as Quine (1951, 22) suggests, but to the mind, and becomes thereby a Cartesian idea.

A Cartesian idea, a paradigm of which is the relation expressed in the equation $x^2 + y^2 = r^2$, is nothing like an Aristotelian species, whether sensory or intelligible, through which one experiences or thinks of something outside the mind. Nor is it a universal abstracted from one's sensory experience of instances. Nor, finally, is it a Platonic mathematical or Form seen with the mind's eye. It is a purely mental entity. Although Plato called already for a turning of the soul away from the realm of the senses and becoming to the realm of intelligible things, of being, it is Descartes who realized such a turn, who achieves, in his mathematical practice with the formula language of algebra, the intellectual vision that Plato sought.[68] Unlike a sensory quality such as redness, or even an object such as a sphere on our everyday understanding of it as something with a characteristic look and feel, there is nothing that a properly Cartesian idea looks, feels, or smells like. Although, it can perhaps be pictured (as, for instance, a circle can), it is itself intelligible rather than sensory. And as intelligible it can come to have a kind of transparency, a clarity and distinctness to thought that sharply contrasts with the opacity, or brute givenness, of sensory experience.[69]

Thought on Descartes' mature account has content independent of the deliverances of the senses, independent even of there being anything without the mind at all—save, of course, for God. Thought is contentful not in virtue of being directed on something mind independent but simply by virtue of itself and its innate ideas, by virtue, that is, of the God-given created truths. And among these truths are, first and foremost, the truths of mathematics, the subject matter of which is extension. It follows, Descartes argues, that the essence of matter is extension. We know that the essence of matter is extension because we know that material things "possess all the properties which I clearly and distinctly understand, that is, all those which, viewed in general terms, are comprised within the subject-matter of pure mathematics" (CSM II 55; AT VI 80). Once Descartes has in place his radically new conception of

[68] That it is algebra that is critical for Descartes is made clear in a comment he makes to Burman: "to enable the intelligence to be developed, you need mathematical knowledge" and "mathematical knowledge must be acquired from algebra" (CSM III 351; AT V 176–7).

[69] The intelligibility of Cartesian ideas as contrasted with the sheer brute presence of sensory qualities is central to the third meditation proof for the existence of God. I do not have within me the idea of God as something sensory, as I have an experience of redness, or even of seeing the word 'God' written in (say) black ink on a piece of white paper. I do not experience but instead understand, grasp intellectually, my idea of God, and that is what is wholly inexplicable except, Descartes thinks, by appeal to God. How can I, a mere finite being, not merely contain within me a representation of the infinite but also understand or grasp, even imperfectly as I do, the idea of the infinite, know it as infinite? Only God, Descartes thinks, could bestow such understanding on me. See also Descartes' appeal to "the idea of a machine of a highly intricate design" in motivating the proof (CSM II 75; AT VII 103, also CSM II 97; AT VII 134–5).

pure intellection, his fundamentally transformed mode of intentional directedness, his understanding of matter as *res extensa* quite naturally follows.[70]

Descartes' new conception of curves, finally, provides the model for understanding the notion of a law of nature. We have seen that an equation in two unknowns in Descartes' symbolic language expresses a law governing the motion of a point in Cartesian space. A law of nature similarly is a rule governing matter, extended substance, in motion (CSM I 93; AT XI 37), one that is expressible in an algebraic equation (CSM I 97; AT XI 47). Such laws, like meanings or Cartesian ideas generally, are intelligible independent of the objects they govern. As already noted, a law of motion is not *in* the objects it governs as an internal principle of change, as the principle of motion of a thing is for Aristotle; and it is not a generalization about objects derived from one's experience of objects. It is instead to be conceived independently of the objects it governs, as something in its own right that is an object of knowledge as such. A law of nature, as Descartes comes to conceive it, is the (direct or immediate) expression of a pattern that objects can instantiate. As such a law, it furthermore grounds a new form of explanation in science. On the modern view that Descartes inaugurates, things happen not as the expression of a thing's nature or internal principle of changing and staying the same but as determined by the laws governing all matter, as instances of universal patterns. The central task of science as it is now to be understood is to discover these laws.

The formula language of mathematics that Descartes develops and uses to discover mathematical truths thus enables a radically new cognitive orientation, a new mode of intentionality or world-directedness. We do not now simply find ourselves with the world in view but must instead self-consciously judge, given sufficient reason, that things are thus and so. Sense experience, from providing us with immediate perceptual knowledge of things is now to be understood as something caused in us by the impacts of mere matter. Cognition more generally is to be understood to be directed on things without the mind not intrinsically, as on the classical view, but only through an act of will. The modern subject correlatively comes to understand itself as distinctively free, where to be free is, as Descartes explains in Meditation Four, to act for reasons. Both mind and nature are, then, most fundamentally to be understood in terms of laws, either laws of nature (motion) or laws of freedom (reason). To be, on the modern conception that is enabled by Descartes' new mathematical practice, is to be subject to laws. The order of things, that in terms of which they are intelligible at all, has become the order of law.

3.6 Conclusion

I have traced Descartes' mature philosophical views to his new mathematical practice, a practice that is enabled in turn by a metamorphosis in Descartes' understanding of

[70] See Hatfield (1993).

space. This new practice, we have seen, is focused not on objects but on relations; and as Descartes shows, such relations can be expressed in the formula language of elementary algebra. Pure intellection has thus become (at least in intention) an actuality, not as Descartes himself thought, by stripping away the accretions of youth and education to reveal the true and immutable natures created and implanted in us by God, but by a radical transformation in one's cognitive orientation as mediated by this new sort of language. The world, from being manifest in perceptual experience, is now to be represented in thoughts that are expressible not in the sensory, narrative language of everyday life but instead in a symbolic language, a language that is non-sensory, non-narrative, essentially written, and primarily a vehicle of thought.

This new sort of language engenders at the same time a new understanding of the being of beings. What had been an essential unity of (substantial) form and matter is now to be conceived as split into, on the one hand, autonomous mind with its innate ideas, and on the other, matter the motions of which are governed by discoverable laws of nature. The narrative order of nature, progressively unfolding in an organic process that actualizes it according to its own nature, gives way to the order of law, of reason and freedom on the side of the mind and of nature and causes on the side of matter. Science is henceforth to be reductive and mechanistic; to understand what a thing *is* one need only consider what it *does*. We have achieved precisely the conception of being that underlies and shapes a project such as Brandom's in *Making It Explicit*, the project of making explicit what something would have to be able to do in order to count as rational at all. We have achieved the sideways-on view. We have become modern.

Understanding

Our conception of reality, hitherto of a single meaningful whole within which we have our rightful place, is split by Descartes into two essentially different realms, an inner realm of meanings and reasons and an outer realm of brute physical causality. But as Kant sees, if Descartes is right, if all meaning and significance lies on the side of the mind, in the inner realm, if the reality on which thought aims to bear is merely brute, causally efficacious stuff, then our natural scientific judgments cannot *possibly* answer to what is as it is. So, Kant concludes, even primary qualities of things, although empirically real, must be transcendentally ideal. What our natural scientific judgments answer to is not how things are in themselves but only how they appear to creatures like us, creatures with our form of sensibility. And this must be so because the very idea of knowledge *requires* that it be the same thing that one thinks and that is, which must, therefore, be something conceptual articulated, something that *combines* form, or meaning, and matter, stuff. The world that is the object of our knowledge *must* be in conceptual shape (matter *and* form) if there is to be knowledge of it, and it *cannot* be in conceptual shape given what, as we discover with early modern developments in mathematics and physics, concepts (forms) are, namely, mental entities. The only way to resolve the difficulty that Kant can see is by distinguishing between an empirical perspective from within which reality is always already knowable and conceptually articulated (form and matter inextricably combined) and a transcendental perspective from which to register that in itself reality is not knowable because not conceptually articulated (mere matter). According to Kant's critical philosophy, pure reason is not and cannot be a power of knowing as Descartes had thought. Not reason but only the understanding is a power of judgment, of knowing.

Then, in the nineteenth century, the practice of mathematics is again profoundly transformed. Eschewing all appeal to constructions in pure intuition, whether Euclidean diagrams or formulae in the symbolic language of arithmetic and algebra, German mathematicians such as Riemann, and Dedekind following him, began to prove theorems deductively, directly from concepts. And with this development Kant's account of mathematical practice as involving constructions that are grounded in and made possible by space and time as the forms of sensibility was decisively refuted. What was much less clear was how exactly Kant had gone wrong, what account was to be given instead.

With developments in mathematics in nineteenth-century Germany it became clear that Kantian pure intuition, and with it the forms of sensibility, have no role to play in mathematical practice. And with pure intuition and the forms of sensibility out of the picture, the distinction between Kant's transcendental and empirical perspectives collapses. Kant's Transcendental Idealism, because crucially dependent on that distinction of perspectives, is thereby dismantled as well. But there are two essentially different ways this can go depending on where Kant's profoundest insights are taken to lie, whether in the transcendental perspective or in the empirical perspective. Almost all twentieth-century analytic philosophy assumed the former, assumed, that is, that form and content are essentially and constitutively opposed. It is in just this way, and for just this reason, that instead of seeing developments in mathematics in the nineteenth century as *showing* us how to dismantle the inside/outside conception of our being in the world that is bequeathed to us by early modernity, we have, at least for the most part, and mostly unwittingly, taken the sideways-on view for granted and have sought to understand the new mathematical practice of reasoning deductively from concepts from *within* its overall framework. Mathematical logic is only one example of the fruits of inquiry that takes as its starting point the ultimately Cartesian idea that inside are meanings and norms while outside is only brute, causally efficacious nature.

But with over a century of work in mathematical logic and philosophy of mathematics it has become increasingly clear that it is impossible to understand the practice of mathematics, and the knowledge to which it gives rise, given this dualism of form and content, given the sideways-on view. Mathematical logic, and the model theoretic and quantificational conception of language to which it gives rise, are utterly, and strangely, irrelevant to mathematics as it is actually practiced, and useless in any serious attempt to understand that practice, how it works as a mode of intellectual inquiry. The problem is not the rules of inference. The problem is the way those rules, and language as a whole, are understood in mathematical logic. Chapter 6 aims to loosen the grip of this conception—insofar as it can be loosened in the absence of any viable alternative.

4

Kant's Critical Turn

How can thought conceived representationally, as it is on Descartes' view, answer to what is? Descartes' answer is in terms of the two very different relationships that mind and world have to one another, by way of the sense experiences we are caused to have by impacts of moving bodies in the world, and by way of the pure intellect, which thinks and through an act of will judges of things as they are. One's experience of an object that one perceives, a red cube, say, is to be understood as a confused effect of a thing that the intellect grasps in a clear and distinct idea. There is, then, both a path from things in the world to thought, via sense experience (which is, as we know, mediated by natural language), and a path from thought to things in the world, by way of the pure intellect (as mediated by Descartes' new symbolic language of mathematics) and the will. And though, on this conception, there is no inherent or inevitable harmony between what we are caused to experience and so have a natural tendency to think, on the one hand, and what we have reason and so ought to think, on the other, there is, Descartes holds, harmony nonetheless, guaranteed by God's infinite power and benevolence. Although the pre-established harmony between thought and reality that is mediated by natural language had been lost, nonetheless, Descartes thinks, we can legitimately call on God to underwrite not only the veracity of our clear and distinct ideas in mathematics, as well as their applicability in physics, but also the evidentiary force of sense experience, its (limited) capacity to provide reasons. Unsurprisingly, this solution to the difficulty failed to convince any but a few. But what then was the solution? How *does* thought conceived representationally answer to what is?

In his Inaugural Dissertation of 1770 Kant thought he had the essentials of an answer. He argues that we have two fundamentally different cognitive faculties: sensibility, which is passive and a faculty of intuition, of singular, immediate representations; and understanding or intelligence, which thinks by means of concepts, which are discursive and general, and "abstracted from the laws inherent in the mind (by attending to its actions in the occasion of an experience)" (Kant 1770, 387–8; AK 2:395). Sensibility, Kant claims, gives us knowledge of phenomena, things as they appear; intelligence is the faculty by which we grasp noumena, things as they are. Kant furthermore suggests that space and time, conceived as given infinite wholes, are the principles of the form of the sensible world, that they are presupposed by experience rather than derived from it, that they are singular representations, pure sensitive cognitions, and that they are subjective rather than objective, ideal rather than real.[1] We are told that geometry deals with space and pure mechanics with time, and that the concept of number that is dealt with in arithmetic, though a concept of the understanding, requires both space and time in order to be actualized in the concrete. Because the objects of pure mathematics "are not only the formal principles of every intuition, but are *originary intuitions*, it [mathematics] provides us with a cognition which is in the highest degree true, and, at the same time, it provides us with a paradigm of the highest kind of evidence in other cases" (Kant 1770, 390; AK 2:398).

On the side of reason, similarly, there are principles of the form of the intelligible world, in the first instance, reciprocity between substances, and as well a priori concepts that are necessary conditions of thought of objects, concepts such as that of existence, necessity, substance, and cause. What Kant does not do in the Dissertation, as he explains in a famous letter to Herz written a few years later, on February 21, 1772, is to explain how these concepts, which "are neither caused by the object nor do they bring the object itself into being," have any relation at all to their object (Kant 1772, 133; AK 10:130). There is no comparable problem on the side of sensibility: "the passive or sensuous representations have an understandable relationship to objects, and the principles that are derived from the nature of our soul have an understandable validity for all things insofar as those things are supposed to be objects of the senses" (Kant 1772, 133; AK 10:130). It is only the pure concepts of the understanding that raise the question, silently passed over in the Dissertation, "of how a representation [that] refers to an object without being in any way affected by it can be possible . . . whence comes the agreement that they are supposed to have with objects—objects that are nevertheless not produced thereby?" (Kant 1772, 133; AK 10:130–1). In that same letter Kant rejects as viciously circular any appeal to God to resolve the difficulty.

It is easy to think that in the Dissertation Kant has half, admittedly the much easier half, of his critical view in his conception of mathematics as grounded in space and time as the forms of sensibility. According to this line of thought, the problem is, first, to come to see that as mathematics is grounded in space and time as forms of sensibility, so pure natural science is grounded in the pure concepts as forms of the understanding, and then to show the objective validity of those concepts in a transcendental deduction. In fact, in the years leading up to the first *Critique* Kant radically revised even his conception of the science of mathematics in the course of coming to his critical views.

[1] Already by 1768, in the essay "Concerning the Ultimate Ground of the Differentiation of Directions in Space," Kant had decisively rejected the Leibnizian conception of space as a totality of relative positions derived from one's perception of non-spatial properties of monads. As he argues, the existence of incongruous counterparts (such as a left and right hand) shows that space must be a *given* whole, that "it is only in virtue of absolute and original space that the relation of physical things to each other is possible" (Kant 1768, 371; AK 2:383). Both Descartes' new mathematical practice and Newtonian physics require such an absolute conception of space as it contrasts with our everyday conception of space in terms of the relative locations of objects.

Before Kant it had seemed natural to assume that whereas (early) modern mathematics is (in some suitably broad and non-technical sense) deductive, a matter of determining by logic alone what necessarily follows from what is given, early modern physics is instead inductive, a matter of generalizing from given particular instances. Although each in his own way, all of Descartes, Leibniz, Hume, and Locke thought that mathematics is deductive. On the side of physics we find Newton explaining in the third edition of the *Opticks* (1717) that the method of modern science

> consists in making Experiments and Observations, and in drawing general conclusions from them by Induction.... *Hypotheses are not to be regarded in experimental Philosophy*. And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general.[2]

But, as already noted in the Introduction, although induction *is* legitimate in premodern scientific reasoning, because things are understood as instances of substantial forms and so express their natures in their characteristic behaviors (which can then be read off those behaviors), induction is *not* in the same way legitimate in *modern* scientific reasoning. Once objects have come to be understood not as instances of substantial forms but as things governed by essentially general causal laws, that is, by laws of nature as they first came to be understood by Descartes, it is no longer possible to see the behaviors of things as expressive of general truths about them. Because causal laws (conceived as Descartes taught us to conceive them) govern things as from the outside and are fully intelligible independent of any objects, they cannot be expressed *in* the behavior of the objects that they govern. Indeed, because they are constitutively general they are *incommensurable* with the behaviors of the objects they govern. There is, just as Hume (1748, sec. IV) argues, a logical gap between the observed instances and a (constitutively general, essentially modern) law. The attempt to justify one's knowledge of a law of nature by appeal to experience must inevitably involve circular reasoning.

It is natural to think that our knowledge of things forms a kind of pyramidal structure at the base of which are things we just know, say, by seeing that they are so or by way of ideas that seem to be innate in us, and that we build on that basis, by induction in the empirical sciences, and by deduction in mathematics. Nevertheless, Hume shows, one cannot, either by induction or by deduction, legitimately infer something that has the constitutive generality of a (modern) law of nature on the basis of one's experience of the objects that are governed by such laws. In the context of early modernity, induction can ground only what Kant describes as the "assumed and comparative universality" (B3) of an empirical generalization, never the necessity and strict universality of a law, which allows of no exception. The laws of nature that it is the aim of Newtonian physics to discover are necessary but not logically

[2] Quoted in Dear (1995, 240); emphasis added.

necessary. Indeed, the principle of causality itself, that every event has a cause, is likewise necessary without being logically necessary; there is no contradiction in the idea of an uncaused event. And the same is true, Kant holds, of the propositions of mathematics; they are necessary but not logically necessary. There is no contradiction in denying a truth of arithmetic such as that $7 + 5 = 12$ because the concept of the sum of seven and five does not contain already within it the concept of twelve any more than the concept of a triangle contains within it that the sum of its angles equals two right angles.[3]

As Kant would come to understand it, *the* problem of philosophy (at least as it is pursued in the first *Critique*) is to understand the nature and possibility of our knowledge of truths that are necessary but not logically necessary, the nature and possibility of knowledge that is, as he came to think of it, at once synthetic and a priori. Because it is synthetic rather than analytic, such knowledge cannot be achieved by logic alone, by appeal to the principles of identity and contradiction; because it is a priori, that is, necessary and strictly universal, it also cannot be known by appeal to experience. And Kant's solution to the difficulty clearly involves what he thinks of as the forms of sensibility and understanding, space and time on the side of sensibility and the pure concepts, the categories, on the side of the understanding. What is less clear is how exactly these forms figure in Kant's solution to the difficulty. There are two possibilities, not clearly distinguished in Kant's writings. The first and most obvious possibility is that articulated already in the Dissertation: these forms serve to provide the grounds, the foundation, for synthetic a priori knowledge. As analytic judgments are grounded in the laws of logic, and a posteriori judgments are grounded in the deliverances of sensory experience, so synthetic a priori judgments are grounded in the forms of our cognitive faculties, the judgments of mathematics in the forms of sensibility and the judgments of pure natural science in the forms of the understanding. Given that space and time and the pure concepts are forms of these faculties we cannot help but experience the world as informed by these forms.

Unfortunately, as Hume had convinced Kant, this account of the role of the forms of sensibility and understanding cannot resolve the difficulty. The problem posed by synthetic a priori knowledge is, as Kant puts it, one of right, *quid juris*, not one of fact, *quid facti*. The task is to discover not what *causes* us to take certain judgments to be true, indeed, necessarily true, but instead with what *justification* we hold such judgments to be true. Even supposing it were the case that our cognitive architecture, our psychology, were such that we simply must experience things in a certain way, that in no way addresses the question of our right so to experience. Indeed, it seems to erect an insurmountable barrier to our making any progress at all on the question of right. What we wanted to understand is how a certain sort of *knowledge* is possible.

[3] Indeed, we saw this already in Descartes, in the idea that not all logical possibilities are real possibilities, which (we saw) Descartes at first takes to mean that mathematical reasoning inevitably must involve also the imagination and later explains by appeal to the God-created truths of mathematics.

It is no answer to be told that we simply cannot help but take the judgments in question to be true. As Kant notes already in the *Inquiry*, "the feeling of conviction which we have with respect to these cognitions [those that cannot be thought otherwise than as true] is merely an avowal, not an argument establishing that they are true" (Kant 1764, 269; AK 2:295). To respond to the question of right by invoking the forms of sensibility and understanding as necessary in this way is skeptical in just the way that Hume's appeal to custom in his account of the origin of our judgments of causal laws is skeptical. It does not answer the question of the nature and possibility of our synthetic a priori *knowledge*.

Our problem is to understand Kant's appeal to the forms of sensibility and understanding in accounting for our knowledge of the synthetic a priori judgments of mathematics and pure natural science. Our first suggestion, that these forms ground various synthetic a priori judgments that can then serve as the foundation for other synthetic a priori judgments, must be rejected insofar as it fails to answer the question Kant poses for himself, insofar as it is merely skeptical. But what is the alternative? Dogmatism uncritically asserts that we *just know* that certain foundational and necessary but not logically necessary judgments are true. The skeptic in turn rejects such assertions because they are merely dogmatic. What the skeptic does not deny is that knowledge properly speaking requires that there be such foundational judgments; with the dogmatist, the skeptic accepts the foundational conception of knowledge arguing only that there is and can be no foundation of the required sort. The best that can be done is to explain (rather than justify) why and how we form the judgments we form by appeal, for instance, to custom, or the forms of our faculties. As we will see in detail below, Kant's critical philosophy aims to overcome the dilemma by rejecting the foundational picture on which it rests. Once we see that no appeal to a Given could possibly help to answer the question of our right to judge *because* it is Given, in effect, something we are somehow *made* to think, we should be able to see that it is the foundationalist picture of knowing that is misguided. It is not the products of science but the *practice* of science, in particular, the activity of construction in mathematics and the activity of self-correction in physics and philosophy, that holds the key to understanding our synthetic a priori knowledge. It is what we *do*, our *capacity* for certain sorts of activities, that enables us to know, and rightfully to claim that we know. The forms of sensibility and understanding serve, on this reading, not (at least first and foremost) as the conditions of the possibility of foundations but instead as the conditions of the possibility of the *activity* that is the pursuit of truth.

4.1 The Nature of Mathematical Practice

According to Descartes' early account, the necessary non-logical relations that are needed in mathematics, without which it would be paralyzed, are to be grounded in the faculty of imagination. When Descartes came to realize that his new mathematical practice enabled him to discover truths even in cases in which no corresponding image could be formed, by reason alone, he suggested instead that God creates these necessary but non-logical truths and implants in us the ideas by which to discover them using only the pure intellect. Mathematics thus comes to be seen as at once ampliative, a science within which to discover new and significant truths, and also strictly logical, that is, by means of reason alone.

Descartes' new mathematical practice is not strictly logical in the Scholastic sense of logic, however. It is strictly logical instead in what Descartes thinks of as the true sense of logic, in the kind of logic that provides the rules that govern the direction of the mind, "which teaches us to direct our reason with a view to discovering the truths of which we are ignorant" (CSM I 186; AT IX B 14). (Both Locke and Leibniz similarly held that by logic alone, as they understood logic, one might extend one's knowledge.) Whereas in Scholastic logic, one abstracts from all content and meaning to consider only various forms of propositions (conditionals, disjunctions, various sorts of categoricals, and so on), in the true logic one reasons *on the basis of* content and meaning, and discovers thereby truths of which one had been ignorant.[4] What Kant realizes is that this new mathematical practice, like the old Euclidean practice with diagrams, is *nonetheless constitutively written*, that it involves a system of written marks within which to reason. It is not, then, a purely cognitive activity but instead a paper-and-pencil one that makes essential use of both perceptual and motor skills, or at least the imagined use of such skills.

Both ancient Greek diagrammatic practice and Descartes' new mathematical practice with the symbolic language of arithmetic and algebra constitutively involve written marks. Nevertheless they are, Kant at first thinks as explained in the *Inquiry*, quite different as practices. A Euclidean demonstration, he there suggests, involves consideration of an instance, (a drawing of) a particular circle, say, "in order . . . to discover the properties of all circles" (Kant 1764, 250–1; AK 2:278). In a Euclidean demonstration, one is supposed to see that certain things are and must be true of the instance under consideration, and hence that they hold generally. The case of arithmetic and algebra is very different; in this case,

> there are posited first of all not things themselves but their signs, together with the special designations of their increase or decrease, their relations, *etc*. Thereafter, one operates with these signs according to easy and certain rules, by means of substitution, combination, subtraction and many kinds of transformation, so that the things signified are themselves completely forgotten in the process, until eventually, when the conclusion is drawn, the meaning of the symbolic conclusion is deciphered. (Kant 1764, 250; AK 2:278)

Whereas Euclidean geometry involves the consideration of an instance, arithmetic and algebra involve instead the writing and rewriting of signs according to rules.

[4] It should not be assumed that Scholastic logic is formal in the sense that Kant takes (general though not transcendental) logic to be formal. No one before Kant took logic to be formal in precisely Kant's sense. Nor could they given that Kant's conception of logic as formal is made possible by Kant's transcendental philosophy, by his idea that all finite cognition involves a given object that is thought through a (Kantian) concept. See MacFarlane (2000).

Only in the *Critique* will Kant argue that *all* of mathematics functions in essentially the same way, through the construction of concepts in pure intuition.

According to Kant's mature account, all of mathematics, whether Euclidean demonstration or algebraic calculation, functions in the same way, through the construction of concepts in pure intuition. Because the intuition is pure, grounded in the forms of sensibility—space and time—it follows immediately that mathematics is applicable to the natural sciences; although not empirical, mathematical knowledge is nonetheless knowledge about empirical objects. But if that is right, then both Kant's earlier accounts must be wrong. Euclidean geometry cannot function by picturing, however, imperfectly, particular geometrical objects, and the symbolic language of arithmetic and algebra similarly, cannot be merely formal, its meaning exhausted by the rules governing the use of its signs.

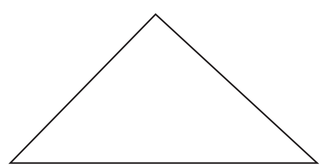
Already in Chapter 2 we developed an alternative to the idea that Euclidean geometry involves reasoning about an instance. A Euclidean diagram does not picture objects but instead formulates conceptual content. And because it formulates content in a system of signs that involves not only complexes of primitives but inscribed relations among those complexes, it is possible to reconfigure the parts of a Euclidean diagram in a variety of ways, and thereby to identify new complexes in the diagram, new relations among concepts. What one is thus able to take out of the diagram is not something that was merely implicit in one's starting points; the conclusion can be obtained from one's starting points but only through the process of construction. The demonstration is ampliative, a real extension of our knowledge, for just this reason: the potential of the starting point to yield the conclusion is actualized only in the course of the construction. This account of diagrammatic reasoning in Euclid is Kant's own mature understanding of this case (leaving aside his appeal to pure intuition). Although Kant at first took geometry to involve instances given in intuitions that enable one to see that something is and must be true of that instance, by the time of the *Critique* he had come to see that geometry essentially involves a *process* of construction/reasoning through which connections are revealed between the concepts of interest. Although it had at first seemed that Euclidean geometry involves only (drawn) instances and algebraic reasoning only the mechanical manipulation of signs, Kant came to see that both forms of mathematical practice involve both something like the construction of an instance of a concept and a process of further construction/reasoning through which the desired conclusion is realized.

By the time of the writing of the first *Critique* Kant has discerned a fundamental similarity in diagrammatic and algebraic reasoning. Both, he comes to think, involve a constructive activity, the capacity to realize in an unfolding array of written marks a new mathematical result, one that extends our knowledge. These two only apparently quite different forms of mathematical reasoning furthermore reveal the essentially spatial and temporal character of this constructive mathematical practice. That mathematical reasoning is in some way essentially spatial is easiest to see in the case of Euclidean geometry, though Kant comes to think that even arithmetic and algebra constitutively involve written displays, that is, constructions of concepts in intuition. That mathematical reasoning is in some way essentially temporal, that it is a stepwise process of construction that "[brings] forth the truth together with the proof" (A734/B762), is more easily seen in the case of arithmetic and algebra. Unlike a course of reasoning through a diagram in Euclid, which involves only a perceptual reconfiguring, reasoning in arithmetic and algebra is at the same time a matter of re-writing. Steps of reasoning are steps of rewriting. In fact, as Kant came to see, both the spatial expanse of the page and an essentially temporal process of construction/reasoning are involved in both of the mathematical practices of concern to him.[5]

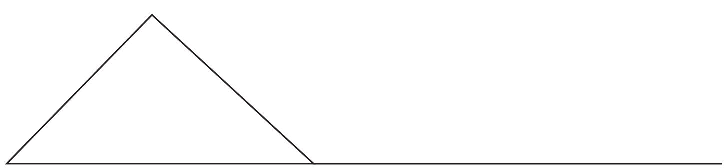
Kant's geometrical example in the *Critique* seems carefully chosen to display as far as possible for the case of Euclidean diagrammatic practice the essentially dynamic process of construction. The demonstration is of the relationship between the sum of angles of a triangle and a right angle. Kant writes:

> He [the geometer] begins at once to construct a triangle. Since he knows that two right angles together are exactly equal to all of the adjacent angles that can be drawn at one point on a straight line, he extends one side of his triangle, and obtains two adjacent angles that together equal two right ones. Now he divides the external one of these angles by drawing a line parallel to the opposing side of the triangle, and sees that here there arises an external adjacent angle which is which is equal to an internal one, etc. (A716/B744)

In this demonstration the reasoning follows the construction especially closely. One is to imagine first a triangle.

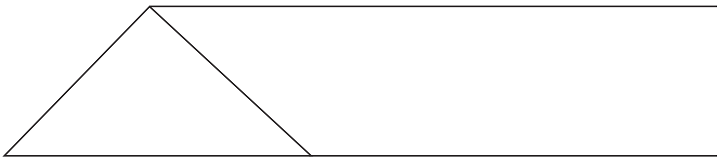


Then one side is extended to create adjacent angles equal to two right angles.



[5] No more than in ancient geometrical practice is existence at issue in the construction: "In mathematical problems the question is not about this [the conditions of possible experience of an object] nor about existence as such at all, but about the properties of the objects in themselves, solely insofar as these are combined with the concept of them" (A719/B747).

And finally a line is drawn parallel to the opposing side that divides the external angle in a way revealing the result that is wanted.



Although it is more obvious in arithmetic and algebra, even in this case we are to see that the demonstration "[brings] forth the truth together with the proof" (A734/B762).

We saw in Chapter 2 that reasoning in Euclid is ampliative, a real extension of our knowledge, in virtue of the three levels of articulation in a Euclidean diagram and the sort of reasoning in the diagram that is possible as a result. As was also indicated, this idea, that reasoning in mathematics involves a visual display of signs with three levels of articulation, appears already in Kant's *Inquiry*. As Kant remarks, signs in mathematics "show in their composition the constituent concepts of which the whole idea consists" (Kant 1764, 251; AK 2:278). The Arabic numeral '278', for instance, shows that the number designated consists of two hundreds, seven tens, and eight units. A drawn triangle similarly is manifestly a three-sided closed plane figure; like the numeral '278' it is a whole made up of simple parts. As Kant further notes, these complexes are then combined to show "in their combinations the relations of the . . . thoughts to each other" (Kant 1764, 251; AK 2:279).[6] In mathematics one combines the wholes that are created from primitives into larger wholes that exhibit relations among them. There are, then, three distinct levels of articulation in such mathematical displays of signs.

We have seen already that reasoning in Euclid is general throughout. Kant, although he does sometimes continue to think of a drawing in Euclid as giving an instance, also provides evidence in the *Critique* that such an instance is peculiar insofar as it is not fully determinate. In the B Preface we are told that the geometer must "ascribe to the thing nothing except what [follows] necessarily from what he [has] put into it in accordance with its concept" (Bxii). In the schematism section we read that "in fact it is not images of objects but schemata that ground our pure sensible concepts," that

> no image of a triangle would ever be adequate to the concept of it. For it would not attain the generality of the concept, which makes it valid for all triangles, right, acute, etc., but would always be limited to one part of this sphere. The schema of a triangle can never exist anywhere except in thought, and signifies a rule of the synthesis of the imagination with regard to pure shapes in space. (A140–1/B180)

[6] Again, Kant is describing what the words of natural language that are used in philosophy cannot do. It is clear that he means indirectly to say what the marks used in mathematics can do.

Kant returns to the point in the Method:

> The individual drawn figure is empirical, and nevertheless serves to express the concept without damage to its universality, for in the case of this empirical intuition we have taken account only of the action of constructing the concept, to which many determinations, e.g., those of the magnitude of the sides and the angles, are entirely indifferent, and thus we have abstracted from these differences, which do not alter the concept of the triangle. (A714/B742)

In mathematics, one "considers the concept *in concreto*, although not empirically, but rather solely as one which it has exhibited a priori, i.e., constructed, and in which that which follows from the general conditions of the construction must also hold generally of the object of the constructed concept" (A715–16/B743–4). That is, as Kant explains in his response to Eberhard,

> in a general sense one may call construction all *exhibition* of a concept through the (spontaneous) production of a corresponding intuition. If it occurs through mere imagination in accordance with an *a priori* concept, it is called pure construction (such as must underlie all the demonstrations of the mathematician; hence he can demonstrate by means of a circle which he draws with his stick in the sand, no matter how irregular it may turn out to be, the properties of a circle in general, as perfectly as if it had been etched in copperplate by the greatest artist). (Kant 1790a, 287 note; AK 8:192, note to 8:191)

Although as a drawing, the figure is particular, it functions in a Euclidean demonstration as something general, as an "exhibition of a concept," just as was argued in Chapter 2.

It is these two ideas—that systems of signs that are used in mathematics enable written displays that involve three tiers of articulation (primitive parts, wholes of those parts, and wholes of the intermediate wholes), and that such displays formulate conceptual content in some way—that enable Kant to explain how mathematical practice is at once a priori and ampliative. Consider Kant's example in the B introduction, that of 7 + 5 = 12. We come to know such a truth, Kant suggests, as follows. "I take first the number 7, and, as I take the fingers of my hand as an intuition for assistance with the concept of 5, to that image of mine I now add the units that I have previously taken together in order to constitute the number 5 one after another to the number 7, and thus see the number 12 arise" (B15–16). The number twelve is to be constructed by the stepwise addition of units: a unit that is initially given as a part of the number 5 is to come to be seen instead as a part of the given number 7 to yield thereby the number 8, and so on. The three levels of articulation that Kant identifies in the *Inquiry* are manifest. First there are the primitive parts, the units; next are the wholes of these parts, the given numbers seven and five conceived as collections of units; and finally we have the larger whole (potentially, the sum that is wanted) of which those given numbers are parts, the whole in virtue of which the parts (that is, the individual units) of those given numbers can be reconceived as parts of different wholes. What we have, in other words, can be depicted thus:

/////// /////.

Again, the primitive parts are the individual strokes; the wholes of these parts are the two collections, considered separately; and the largest whole is the whole array, the whole of these two collections as parts. The display enables the desired chain of constructive reasoning as follows. A unit that is at first taken to be a part of the number five is now reconceived as a part of the collection that is the number seven, thereby making it a collection that is the number eight. That is, we reconceive the whole display such that the two collections are now these:

//////// ////.

And we need to *do* this because the fact that 7 + 1 equals eight by definition, which Leibniz had claimed enables the solution to be deduced from definitions, cannot help in the demonstration until and unless we reconceive a unit given as a part of the number five instead as a part of the collection that is the seven units of seven.[7] Now we do the same again, that is, imagine that another unit from the collection of four belongs instead with the collection of eight:

///////// ///.

Continuing the process until all units have been reconceived in this way, one sees the number twelve (a collection of twelve strokes) come into being, and the problem is solved.

The practice of calculating in the Arabic numeration system illustrates essentially the same point. Suppose that one wished to determine, say, the product of twenty-seven and forty-four. The Arabic numeration system provides a means as follows. First one writes signs for the two numbers to be multiplied in a particular array:

$$\begin{array}{r} 27 \\ \times\underline{44}. \end{array}$$

The first number is that given by the first line (reading across left to right[8]) and the second is written directly below it. As in the case of the sum of seven and five as Kant conceives it, three levels of articulation are discernible, first, the primitive parts, that is, the primitive signs '2', '4', and '7', then the wholes of these parts, namely the signs '27' and '44' that are signs for the numbers given as the terms of the problem, and finally, the whole display. The calculation is enabled by this three-tiered structure as follows. First, one reconfigures at the second tier, taking a part of the whole '44', namely the rightmost '4', and reconceiving it as belonging with the '7' in '27'.

[7] Frege (1884, sec. 6) makes the same point by appeal to the use of brackets in the formula language of arithmetic.

[8] In fact, such a reading depends on a prior reading right to left in order to determine what position the '2' occupies. Like written Hebrew and written Arabic, Arabic numerals were designed to be written, and read, right to left.

$$\begin{array}{r} \scriptstyle 2 \\ 27 \\ \times\underline{44} \\ 108 \\ \underline{1080} \\ 1188 \end{array}$$

Figure 4.1 A calculation in Arabic numeration.

Multiplying the two numbers symbolized in this new whole yields the number twenty-eight so one puts a new primitive sign ‘8’ under the rightmost column and to carry the two one puts the sign ‘2’ above the leftmost column. Next one takes the same sign ‘4’ and considers it together with the ‘2’ in ‘27’, and so on in a familiar series of steps that result in what is shown in Figure 4.1. The last line is of course arrived at by the successive addition (starting at the right) of the numbers given in the columns at the third and fourth rows. It is by reading down, and right to left, that one understands why just those signs appear in the bottom row; but it is by reading across, left to right, that one knows the product that is wanted. In this way, through simple calculations on successive reconfigurations of various parts of the original two-dimensional display, one achieves the result that is wanted.

And exactly the same point applies, Kant claims, to calculations in algebra.

> Even the way algebraists proceed with their equations, from which by means of reduction they bring forth the truth together with the proof, is not a geometrical construction, but it is still a characteristic construction, in which one displays by signs in intuition the concepts, especially of relations of quantities, and, without even regarding the heuristic, secures all inferences against mistakes by placing each of them before one’s eyes. (A734/B762)

Even in algebra one exhibits the contents of concepts in a spatial array of written signs, for instance, the concept of a sum of a number and its reciprocal as ‘$x + 1/x$’ or the concept of a product of two sums of integer squares as ‘$(a^2 + b^2)(c^2 + d^2)$’, which can then be rewritten in various ways according to rules ultimately to reveal new mathematical relations among concepts. (Later we will see in more detail how and why this works.)

Reasoning in mathematics, on Kant’s account, involves the two-dimensional display of the contents of mathematical concepts, concepts such as that of a triangle or circle in a Euclidean diagram, and that of a sum of squares or a product of sums in Descartes’ symbolic language. And because such displays can be combined into larger wholes that can in turn be reconfigured, either perceptually as in Euclid or by rewriting as in Descartes’ algebra, new relations among concepts can be discovered in the course of mathematical reasoning in these systems of written signs. Such results are synthetic, that is, ampliative, real extensions of our knowledge, and also a priori, necessary and strictly universal, just as Kant says.

This practice furthermore provides a graphic illustration of the interplay of concepts and intuitions in cognition as Kant understands it. In the case of Euclidean diagrammatic reasoning, the various parts of the given manifold of points, lines, angles, and areas in the diagram must be synthesized according to a concept in order to be thought now, say, as a radius of a circle and now as a side of a triangle. And as was shown in section 2.5, to work through a demonstration just is to perceive the parts now as one thing (as synthesized under one concept) and now as another (under another concept) in an ordered series of steps that take one from one's starting point to the desired conclusion. In algebra one does not in the same way reconceive perceptible parts. Instead one rewrites according to rules to show thereby that something given one way (through these concepts) can also be given another way (through different concepts). Kant makes the point in a letter to Schultz written shortly after the appearance of the second edition of the *Critique*.

> I can form a concept of one and the same magnitude by means of several different kinds of composition and separation, (notice, however, that both addition and subtraction are syntheses). Objectively, the concept I form is indeed identical (as in every equation). But subjectively, depending on the type of composition that I think, in order to arrive at that concept, the concepts are very different. So that at any rate my judgment goes beyond the concept I have of the synthesis, in that the judgment substitutes another kind of synthesis (simpler and more appropriate to the construction) in place of the first synthesis, though it always determines the object in the same way. Thus I can arrive at a single determination of a magnitude $= 8$ by means of $3 + 5$, or $12 - 4$, or 2×4, or 2^3, namely 8. But my thought "$3 + 5$" did not include the thought "2×4." Just as little did it include the concept "8," which is equal in value to both of these. (Kant 1788, 283; AK 10:555)

A concept, Kant suggests to Schultz, can be considered either objectively or subjectively, and two concepts can be objectively the same, that is, concepts of one and the same object, but subjectively different. In '2×4', for instance, the number eight is thought as a product; in '$3 + 5$' that same object is thought instead as a sum. Objectively, the two expressions '2×4' and '$3 + 5$' are the same because in both cases the number designated is eight; but subjectively they are not the same because although what I think *of*, namely the number eight, is the same in the two cases, what I *think*, in the one case the sum of three and five and in the other the product of two and four, is quite different in the two cases. As Frege would put Kant's point here, although the two expressions designate one and the same object (have the same *Bedeutung*), they do so under different modes of determination; they express different senses (*Sinn*).

On this account, space and time are the conditions of the possibility of the science of mathematics not because they provide foundational claims on the basis of which to reason (although they do that too), but because they are the conditions of the possibility of the activity of construction that is constitutive of the pursuit of truth in this science. Notice, further, that nothing in this activity as an activity suggests that mathematics is a science only of sensory things, that it does not apply also to things as they are in themselves. And yet this is just what Kant claims: not only is it the case that "space represents no property at all of any things in themselves nor any relation of them to each other," it is also the case that "space is nothing other than merely the form of all appearances of outer sense" (A26/B42). (The same is of course true of time.) The first of these conclusions is nothing more than a corollary of the claim that space is the form of outer intuition. Whether or not things in themselves have spatial properties, the spatial properties we experience things to have, have their origin in the experiencing subject rather than the experienced object. *Our* representations of such properties are not, then, representations *of* properties of things in themselves—though this is compatible with their being isomorphic to such properties. Kant's second conclusion, that space is *nothing other* than the form of outer sense, denies the isomorphism. Space, although empirically real, is transcendentally ideal.

Kant claims that space and time, although empirically real, are transcendentally ideal. And he does so because space and time are, he thinks, systems of relations. Things as they are in themselves, that is, *substances*, are constitutively *self*-subsistent; they cannot bear constitutive relations to other things (because that is incompatible with their being self-subsistent). But spatial things do bear constitutive relations to other things. Substances, then, cannot be inherently spatial. The spatiality of the things we experience cannot be really real. (And the same of course holds for the temporality of the things we experience.) As Kant explains in the *Prolegomena* (1783, 82; AK 4:286), in the case of spatial things, "the part is possible only through the whole" but this "never occurs with things in themselves as objects of the understanding alone." As we read in the second edition of the *Critique*,

> through mere relations no thing in itself is cognized; it is therefore right to judge that since nothing is given to us through outer sense except mere representations of relation, outer sense can also contain in this representation only the relation of an object to the subject, and not that which is internal to the object itself. It is exactly the same in the case of inner sense. (B67)

Not only space and time but reality itself insofar as it is constitutively spatiotemporal is transcendentally ideal. Because space and time are inherently relational structures, Newton's mathematical physics, the triumph of seventeenth- and eighteenth-century science, is not and cannot be a science of things as they are but only of things as they appear to creatures like us, creatures with our form of sensibility.

4.2 An Advance in Logic[9]

Kant's conception of the practice of mathematics in terms of the constructive activity of the mathematician provides an account of how that practice is at once ampliative and a priori, how it enables the discovery of relations among concepts that were not thought already in those concepts. But this account is not self-standing because

[9] Tiles (2004) is the only other work I know that recognizes Kant's logical advances for what they are. My own views were formed before I discovered this work.

knowledge, Kant thinks, is properly speaking of objects and in mathematics there are no objects.

It may look, to be sure, as if the possibility of a triangle could be cognized from its concept in itself (it is certainly independent of experience); for in fact we can give it an object entirely *a priori*, i.e., construct it. But since this is only the form of an object, it would still always remain only a product of the imagination, the possibility of whose object would still remain doubtful, as requiring something more, namely that such a figure be thought solely under those conditions in which all objects of experience rest. (A223–4/B271)

"All mathematical concepts are not by themselves cognitions, except insofar as one presupposes that there are things that can be presented to us only in accordance with the form of that pure sensible intuition" (B147). The constructive activity of the mathematician, the conditions of possibility of which are space and time as forms of sensibility, thus provides only part of the story, the remainder of which is given in the Transcendental Logic. It is the Transcendental Logic that provides a general theory of judgment, whether mathematical or natural scientific, Transcendental Logic that is the science of truth.[10] Understanding this science, and its significance for Kant's deepest insights into the striving for truth in the exact sciences, is the task of the remainder of this chapter, beginning with Kant's distinction between intuitions and concepts.

Kant's division of cognitions into intuitions and concepts can be seen as a successor to Descartes' division of, on the one hand, sensory experience as a confused mental effect of the impact of a body on our sense organs, and on the other, clear and distinct ideas that are innate in us and fully meaningful independent of the existence of any object (excepting God). For Kant, intuitions, singular representations immediately of objects, are, we will see, essentially sensory (insofar as they give objects rather than merely the forms of objects), and something with respect to which we are passive. They are caused in us. And concepts, general representations through marks, are not in themselves object involving, and they belong to spontaneity. What is original with Kant is, first, the idea that intuitions and concepts are essentially different in belonging to two very different faculties, respectively, sensibility and understanding, and second, the thought that *both* are involved in any cognition. In Kant's famous slogan: "Thoughts without content are empty, intuitions without concepts are blind" (A51/B75). Thus, as Kant immediately goes on,

it is . . . just as necessary to make the mind's concepts sensible (i.e., to add an object to them in intuition) as it is to make its intuitions understandable (i.e., to bring them under concepts). Further, these two faculties or capacities cannot exchange their functions. The understanding

[10] As already noted, what Kant thinks of as general logic, which was hitherto simply logic, is not, Kant thinks, properly a science but merely formal, that is, devoid of all content and hence all truth. And it is formal *because* it abstracts from all relation to any object; lacking any relation to any object it is wholly devoid of content and hence of truth. Again, this conception of the formality of logic is possible at all only on the basis of Kant's radically new conception of cognition as constitutively involving concepts and intuitions as Kant understands them.

is not capable of intuiting anything, and the senses are not capable of thinking anything. Only from their unification can cognition arise.

Much as Descartes divides the ancient notion of a primary substance with its characteristic nature into a form in mind and mere matter in the world, so Kant divides the ancient conception of cognition—which, as the cognitive grasp of an object in its nature, as what it most fundamentally is, is both inherently object-involving and constitutively general because of a thing as the *kind* of thing it is—into an intuition, which is singular and that through which an object is given, on the one hand, and a concept, which is predicative and that through which a (given) object is thought, on the other. Because Kant nevertheless thinks that both are involved in any cognition, Kant realizes thereby an essentially new conception of cognition. Not grasp of an object as what it is but *judgment* is henceforth to be understood as the smallest unit of (human) cognition.[11]

Concepts, as Kant understands them, are general representations of objects by way of marks. They have a singular use (and as well a particular and a universal one), but concepts themselves cannot be singular.[12] They cannot, according to the *Jäsche Logic*, because no concept is fully determinate, that is, a lowest concept (*conceptus infimus*) or lowest species: "even if we have a concept that we apply *immediately* to individuals, there can still be specific differences in regard to it, which we either do not note, or which we disregard" (Kant 1800, 595; AK 9:97). Any concept, even in its singular use (say, the concept *house that Jack built* in its singular use in the judgment that the house that Jack built still stands), can be made more determinate through the addition of further concepts to it (as in the concept *stone house that Jack built*). There can be "thoroughly determinate cognitions" Kant thinks—that is, cognitions of particular individuals, singular representations properly speaking—but "only as intuitions" (Kant 1800, 597; AK 9:99). Intuitions thus contrast with concepts not merely in degree, as singular and general concepts of objects (as on Leibniz's view), or

[11] Of course, judgment is central already for Descartes, but in Descartes judgment is grounded in the, ideally clear and distinct, perception of ideas. Only in Kant is the *judgment* understood to be the smallest unit of cognition.

[12] Kant makes the point in the *Jäsche Logic* (Kant 1800, 589; AK 9:91): "It is a mere tautology to speak of universal or common concepts—a mistake that is grounded in an incorrect division of concepts into *universal*, *particular*, and *singular*. Concepts themselves cannot be so divided, but only *their use*." The point is also made in the Vienna Logic based on lectures Kant gave in the early 1780s (see AK 24:909). In the Blomberg Logic, based on lectures from the early 1770s, Kant instead follows Meyer in putting the distinction as a distinction between *conceptus singularis*, in which only one particular, individual thing is thought, and *conceptus communes* through which one thinks something that many things share in common (see AK 24:257). Now according to Parsons (1992, 93, n. 14), in Kant's writings "the terms *Anschauung* and *intuitus* . . . emerge as central technical terms in the 1768–70 period, when Kant makes the sharp distinction between sensibility and understanding and makes the decisive break with the Leibnizian views of space and sense-perception." Kant nevertheless does not in 1770 have the conception of the distinction between these faculties and the representations they involve that he will defend in the first *Critique*. In the Inaugural Dissertation the distinction between concept and intuition is only a distinction of origins, whether in sensibility or in the understanding.

in origin, in sensibility or understanding (as Kant had held in the Inaugural Dissertation), but in kind, that is, logically, as immediate to mediate: "in whatever way and through whatever means a cognition may relate to objects, that through which it relates immediately to them, and at which all thought as a means is directed as an end, is intuition" (A19/B33).

The everyday concepts of natural language, we have seen, are immediately of objects and inherently referential. Objects correspondingly are instances of kinds; they have natures and are intelligible as such. Things just do show up in our everyday perceptual experience as what they are. (Given the way natural language comes about, this is furthermore a necessary feature of our everyday perceptual experience.) Kant's conception both of a concept and, correlatively, of an object is very different. Unlike the concepts of everyday language, which have both a referring and a predicative use, Kantian concepts are predicates of possible judgments and are not themselves object involving. They cannot give objects, and they cannot because objects as they are now to be conceived—as the locus of the truth of our judgments rather than as things we can just see to be as they are—are "all-sided determinations." An object as Kant conceives it is not an instance of a form, some kind of thing in particular, but instead that to which a predicate, any predicate, can be applied truly or falsely; a Kantian object settles for any predicate you like either that the predicate applies or that it does not apply. Because any description of an object through predicates can be made more determinate, an *individual* concept, that is, a concept that is of an object in its individuality, its endless specificity, would have to contain an endless number of predicates; and because it would, no finite being could grasp such a concept. A Kantian concept is "a representation that is contained in an infinite set of different possible representations (as their common mark), which thus contains these **under itself**; but no concept, as such, can be thought as if it contained an infinite set of representations **within itself**" (B40). But because and insofar as we are now to conceive an object as an all-sided determination, that is just what an individual concept would have to be, something "[containing] an infinite set of representations **within itself**." For a finite being there can be no such concept; we (finite beings) cannot think an object as such. Objects, all-sided determinations, must be *given* for the cognition of a finite being.

A concept, according to Kant, can be contained in an endless number of different representations, the concept *number*, for instance, in the representation 'the number that is twice one', in the representation 'the number that is twice two', and so on for all the counting numbers. All the numbers thus represented are contained *under* the concept *number*. What is contained within a concept, by contrast, is its marks, for instance, being a number and being twice one. Because we finite beings cannot comprehend an infinite number of things (at least as such), and because we grasp a concept by way of our grasp of its component parts, it follows that we could not possibly grasp a concept that contained an infinite number of component concepts. Cartesian space, which we know to contain an infinite number of component parts, cannot, for example, have the form of a concept. Because it is a whole of parts that can be grasped as a whole, that is, prior to one's having grasped any of its parts, it has (must have) the form of an intuition.[13] Concepts are inherently general, through marks; intuitions are singular, immediately of objects.

The second difference between intuitions and concepts on Kant's view is metaphysical rather than logical.[14] As Kant emphasizes again and again in the *Critique*, the faculty of intuition through which cognition is related immediately to objects is a passive or receptive faculty: "intuition takes place only insofar as the object is given to us . . . only if it affects the mind in a certain way" (A19/B33). Concepts, by contrast, are "grounded on the spontaneity of thinking" (A68/B93). The understanding, the faculty of spontaneity, is only a faculty of concepts; sensibility or receptivity is alone a faculty of intuition: "the understanding is not capable of intuiting anything, and the senses are not capable of thinking anything" (A51/B75). According to Kant's account in the *Critique*, as contrasted with that of the Dissertation, cognition—that is, *thought* that is *of* an object—thus essentially involves both receptivity and spontaneity. The Transcendental Logic opens by making just this familiar point (A50/B74).

An object, on Kant's view, is the locus of the truth of a judgment, and as such is an all-sided determination, something absolutely specific, just what it is and not another thing, and also a unity within which parts can be discerned but which is first and foremost a whole, one thing.[15] Because an object is an all-sided determination, no concept can represent it as the unique individual it is; for, again, any concept can be made more specific through the addition of another determination. One cannot then think an object (some specific individual) through concepts alone; our spontaneity is not, and cannot be, a faculty of intuition. But the idea of such a spontaneity is not incoherent according to Kant. As he emphasizes both in the Transcendental Aesthetic and in the Transcendental Logic of the B edition of the *Critique*, and again in the third *Critique*, we can conceive—that is, think, though not cognize as actual or

[13] The parts of space "are only thought **in it**. It is essentially single; the manifold in it, thus also the general concept of spaces in general, rests merely on limitations" (A25/B39). That is, our representation of space is not built up out of spaces; rather our representations of spaces ("the manifold in it") are thought in space through limitation (that is, by marking off boundaries). But only in an intuition does the whole precede the parts rather than conversely: "that representation . . . which can only be given through a single object, is an intuition" (A32/B47). As "essentially single" in this way, our fundamental representation of space has the logical form of an intuition. (As we will later see, in section 5.3, there is also a third sort of case, namely, that of a Kantian idea or concept of reason, which is like an intuition insofar as the whole is prior to the parts but is nonetheless a concept. Both Husserl and Hilbert will suggest that the concepts of nineteenth-century mathematics are such concepts of reason.)

[14] See Kant (1800, 546–7; AK 9:36). In this passage Kant characterizes sensibility, the faculty of intuitions, and understanding, the faculty of concepts, also as lower and higher, respectively, "on the ground that sensibility gives the material for thought, but the understanding rules over this material and brings it under rules or concepts." This way of drawing the distinction, it will be suggested, is ontological, rather than logical or metaphysical.

[15] Of course an object on the ancient view is also, in its way, just what it is and not another thing. But what it is first and foremost is a primary substance, an instance of a form, not, for example, Socrates in all his endless individuality but Socrates *qua* human.

even as actually possible—an intuitive understanding or intellectual intuition that through its spontaneity alone thinks an object as such. Such an intellectual intuition, Kant suggests, would be "**original**, i.e., one through which the existence of the object of intuition is itself given" (B72); and because original in this way, such an intuition "would not require a special act of the synthesis of the manifold for the unity of consciousness, which the human understanding, which merely thinks, but does not intuit, does require" (B139). Because, for us, an object must be given for cognition, our cognition requires correlatively an act of spontaneity in order to think that object, in order, that is, to be aware of it at all. Our cognition is thus not unitary in the way the cognition of an intuitive understanding would be. Instead it has two aspects or moments, a moment of passivity or receptivity through which the object is given and also a moment of activity or spontaneity through which that object is thought. And as Kant explains in the third *Critique*, the distinction "between the possibility and the actuality of things" is grounded in just this two-fold relation we have to the objects of cognition: "if two entirely heterogeneous elements were not required for the exercise of these [cognitive] faculties . . . then there would be no such distinction (between the possible and the actual). That is, if our understanding were intuitive, it would have no objects except what is actual" (Kant 1790b, 272; AK 5:401–2). In a finite being, "thinking and intuiting, hence the possibility and actuality of things, are two different conditions for the exercise of its cognitive faculties" (Kant 1790b, 273; AK 5:402–3). In a being capable of spontaneous or intellectual intuition, thinking and intuiting would be one and the same. The thought of such a being would be pure act.

The idea of a spontaneity or a faculty of understanding that is at the same time a faculty of intuitions, of singular cognitions immediately of objects, is not incoherent according to Kant. Although we cannot establish even the possibility of such a faculty (as an object of possible knowledge for us), we can conceive of it. And such a conception is useful insofar as it highlights just the metaphysical dimension of the concept/intuition distinction with which we are here concerned. Our intuition—though not intuition as such, that is, as logically characterized, as singular cognition immediately of an object—is essentially passive; for us objects must be given by means of a receptive faculty. And our spontaneity—though not spontaneity as such, that is, the active capacity of the understanding—is a capacity for combining (given) representations in cognition of an object. Given all of this, it follows that Kant's Dissertation account of sensibility and understanding as two separate faculties of knowing, the one of knowing phenomena and the other of knowing noumena, cannot be right. Neither the receptive faculty of sensibility nor the spontaneous faculty of concepts, we are now to see, is a faculty of knowing independent of the other.

The distinction between concepts and intuitions is at once a logical distinction between two species of cognition, the one singular and immediate, the other general, by way of marks common to many, and also a metaphysical distinction between two aspects or moments in any cognition of an object. Through intuition, an essentially passive capacity, an object is given, and through concepts belonging to spontaneity that object is thought. What from the logical perspective are two sorts of cognition are from the perspective of transcendental philosophy only two aspects of the only sort of (finite) cognition there is, aspects that are involved in any (human) cognition, whether logically singular or logically general. But even this metaphysical characterization of Kant's distinction needs one last supplementation to bring Kant's conception of (finite, human) cognition as discursive fully into view. According to Kant, not only are intuitions singular, immediate, and passive, and concepts general, through marks, and functions of spontaneity, they are also, respectively, sensory matter and rules: "sensibility gives the mere material for thought, but the understanding rules over this material and brings it under rules or concepts" (Kant 1800, 546–7; AK 9:36). Our receptivity through which an object is given is sensory and our spontaneity through which a given object is thought is a faculty of rules. While it is in principle possible that we might have had intuitions, singular representations immediately of objects, by some other means—bestowed on us, say, by an act of a divine being—we in fact have intuitions through being affected by objects, that is, through sensibility.[16] It follows, because sensation is itself merely determinable rather than determinate, that such intuitions can provide only "the matter of all appearance" (A20/B35). Sensing, that is to say, is not a form of knowing, however primitive or confused. Sensations are modifications in us that can be *for* us, that is, objects of awareness or the means by which we are aware of other things, only in virtue of the unifying synthetic activity of the understanding. Absent that activity, these modifications "would be nothing but a blind play of representations, i.e., less than a dream" (A112). "Intuitions without concepts are blind" (A51/B75).[17] Concepts, correlatively, are not representations (of objects) at all; they have the form of rules (A106) governing the synthetic activity of the understanding. Even the simplest awareness of anything involves an act of spontaneity according to rules; and as rules, concepts are empty, not cognitions at all, independent of sensibility:

> if an intuition corresponding to the concept could not be given at all, then it would be thought as far as its form is concerned, but without any object, and by its means no cognition of anything at all would be possible, since, as far as I would know, nothing would be given nor could be given to which my thought could be applied.... thinking of an object in general through a pure concept of the understanding can become cognition only insofar as this concept is related to objects of the senses. (B146)

All our knowledge begins with sensory experience, and ultimately, all our knowledge must relate back to it as well.

[16] As Kant explicitly recognizes, passive intuition need not be by means of sensibility, though that is how it is with us: "this [intuition]... takes place only insofar as the object is given to us; but this in turn, at least for us humans, is only possible if it affects the mind in a certain way" (B33).

[17] In some metaphysics lectures from the mid-1770s (Kant 1770s, 23; AK 28:199), we read that "blind means when one oneself cannot see; but also that through which one cannot see." *Blindes Glas* is opaque glass, glass through which one cannot see. Blind intuitions, that is, intuitions without concepts, are similarly representations through which one cannot see, which are not revelatory of things in the world.

We have seen that by contrast with the cognition of an intuitive understanding, our cognition essentially involves a moment of passivity through which an object is given and a moment of activity through which that (given) object is thought. Our intuition is not spontaneous or active but essentially receptive; our spontaneity does not itself produce the object but can only think it, and the way it thinks it is through concepts conceived as rules of unity. Because our intuition is sensory, and hence not as such a form of awareness at all, the activity of our spontaneity is needed in order to unify the given matter of sensation. Our understanding, by contrast with that of an infinite being, is, then, constitutively discursive: it is through concepts conceived as rules of unity that a given object is thought in relation to an intuition that is in itself merely a "determination of the mind" (A50/B74).

For Kant, intuitions contrast with concepts not only logically, as singular representations immediately of objects and general representations by way of common marks respectively, but also metaphysically, as passive and active moments in our cognition of any object (intuition as that through which the object is given and concept as that through which it is thought), and, finally, ontologically, as the determinable matter and determining form respectively of any cognition. Given that concepts are, for Kant, predicates of possible judgment, it would seem to follow, as Thompson (1972, 333) has argued, "that for Kant an intuitive representation has no place in language, where all representation is discursive. In language we presuppose intuitive and create discursive representations." Because (for a finite being) there is no thought of an object except through a concept, what we might think of as the linguistic correlate of a Kantian intuition, namely, a proper name conceived as a mere label for a (given) object, cannot occur in language. All proper names must be conceived instead as involving concepts, that is, as disguised descriptions that assume existence and uniqueness.

But how then does the activity of judging succeed in getting at objects for Kant? We are told in the B deduction, section 19, that "a judgment is nothing other than the way to bring given cognitions to the **objective** unity of apperception. That is the aim of the copula **is** in them: to distinguish the objective unity of given representations from the subjective" (B141–2). What Kant means here is indicated in some remarks he makes in the context of his discussion of the ontological proof for the existence of God. As he first notes, "**being** is obviously not a real predicate, i.e., a concept of something that could add to the concept of a thing" (A599/B627). As he then goes on,

> thus when I think a thing, through whichever and however many predicates I like (even in its thoroughgoing determination), not the least bit gets added to the thing when I posit in addition that this thing *is*. For otherwise what would exist would not be the same as what I had thought in my concept, but more than that, and I could not say that the very object of my concept exists. (A600/B628)

And from this it follows that "whatever and however much our concept of an object may contain, we have to go out beyond it in order to provide it with existence" (A601/B629). All judgments of existence are thus synthetic.[18]

Kantian concepts are not constitutively object involving. But judgment, Kant thinks, is constitutively object involving. It follows that judgment cannot in Kant's view be conceived (following Aristotle) as a matter of saying of the Ss that all, or some, or none, are P, a matter, that is, of ascribing a property to, or withholding it from, a thing or things of a certain sort. Instead judgment is conceived as an act of positing a relation of concepts *as* objectively valid, that is, as combined in an object or objects. This difference between the two conceptions of judgment is graphically illustrated in the difference between Euler and Venn diagrams.

In an Euler diagram one shows that all S is P by drawing the P-circle around the S-circle, that no S is P by drawing the P-circle wholly outside the S-circle, and that some S is P by drawing the P-circle in a way that overlaps with the S-circle. The form of depiction presupposes that there are Ss that can be encircled in these ways. What it shows is how things stand with the Ss. In drawing the P-circle as one does—around the S-circle, or disjoint from the S-circle, or overlapping the S-circle—one thereby judges that things are thus and so; the act of drawing the P-circle as one does just is to relate in some way the predicate P to the Ss, all or some of them. The act of drawing the P-circle as one does is itself the act of judging. An Euler diagram in this way illustrates the conception of judgment as a predication, as saying of the Ss that all, some, or none are P.

The notion of judgment that is suggested by a Venn diagram is very different. In a Venn diagram the placement of the circles does not show anything; in particular, it does not show, is not to be read as showing, that some S is P as it would were it to be read as an Euler diagram. The overlapping circles in a Venn diagram merely give two concepts (which as predicates of possible judgment may be empty) as the matter for judging. It is the shading, or placement of an 'x', that serves to exhibit something about the matter provided by the concepts, and different sorts of shading realize different judgments about that matter. From the perspective of a Venn diagram, then, it is natural to distinguish between the content of a sentence and its form, that is, between the drawn circles themselves, which represent concepts conceived as predicates of possible judgment, and what is said of those concepts by way of shading or the placement of an 'x' to mark that there is something with the relevant property or properties. The *form* of the judgment is given by what is asserted of the concepts involved.[19] Because this form is also what relates the concepts to objects (some, none, or all), the conception of judgment that is exhibited in a Venn diagram naturally gives rise to the idea that one might determine a table of the pure concepts of the

[18] 'I exist' is the exception. Such a judgment is analytic according to Kant.

[19] "In every judgment one can call the given concepts logical matter (for judgment), their relation (by means of the copula) the form of the judgment" (A266/B322).

understanding on the basis of a table of the forms of judgment.[20] The pure concepts so conceived are concepts that are applied to other concepts, namely, a subject concept and a predicate concept, in the course of judgment; they are concepts that serve to connect, to refer, those other concepts to objects.[21] And those under the title of quantity in particular serve to introduce what we today think of as quantifiers; they are the mechanism of reference to objects.

We have seen that Kant's distinction between two sorts of representations—intuitions and concepts—is at once a logical distinction between singular and general representations, a metaphysical distinction between representations with respect to which we are passive and those with respect to which we are active, and an ontological distinction between something that is the matter for cognition and something that has the form of a rule. I have furthermore suggested that this division of representations can be seen as a successor of sorts to Descartes' division of sensory experience, which is a confused effect of the impacts of material bodies on our sense organs, and innate ideas that are fully meaningful independent of the existence of any objects. But if that is right then it is in fact two very different sorts of language that underlie this division in Descartes, natural language as the medium of our everyday sensory experience of things and Descartes' symbolic language as the medium of his mathematical inquiries. Thus it may seem that Kant's distinction of two sorts of representations, intuitions and concepts, is not a *logical advance* at all but is instead the result of a thoroughgoing *confusion* of two essentially different ways we can be intentionally directed on reality, one that falsifies both. And in a way this is right. But in another way it is not right, or at least it is not the whole story. Although Kant's distinction has its historical roots in the very different modes of intentional comportment that are enabled, on the one hand, by natural language, and on the other, by Descartes' symbolic language (and hence constitutes a kind of confusion), the distinction is nonetheless an advance in logic insofar as it enables Kant to realize (as we will soon see) something essential about the striving for truth, that is, about our capacity for knowledge as the rational beings we are. Kant's insight will need to be further developed (by Frege) so as to free it from the confines of Kant's Transcendental Idealism. But that further, *clearly* logical development is possible at all only in light of Kant's division of representations into intuitions and concepts both of which are involved in any cognition.[22] Hence, though only in

[20] The pre-modern idea of judgment that is iconically displayed in an Euler diagram does not support this idea because on that conception predicating is not logically distinguished from judging.

[21] It is worth noting that in the *Jäsche Logic*, although Kant most often uses Euler-style diagrams (for example, in the diagrams of section 21)—in part because his concern is general rather than transcendental logic and so abstracts from all relation to any object—in section 29, Kant comes very close to a Venn-style depiction in his discussion of a categorical judgment in which the object x, contained under concept b, is also contained under concept a. The idea that one needs two different sorts of letters here, that is, both x and, functioning in a very different way, also b and a, is of course something we find already in Descartes' algebra.

[22] Frege's advance beyond Kant will, furthermore, enable us to re-introduce, in Chapter 8, the distinction between the language of everyday life and a properly mathematical language that Kant conflates in his account of concepts and intuitions.

retrospect, Kant's division too can be seen as a logical development, and indeed, as "the first serious advance in real logic since the time of the Greeks."[23]

But what then are we to make of Kant's remarks in the B Preface regarding the science of logic? Kant writes:

> That from the earliest times **logic** has traveled this secure course [of a science] can be seen from the fact that since the time of Aristotle it has not had to go a single step backwards, unless we count the abolition of a few dispensable subtleties or the more distinct determination of its presentation, which improvements belong more to the elegance than the security of that science. What is further remarkable about logic is that until now it has also been unable to take a single step forward, and therefore seems to all appearance to be finished and complete. (Bviii)

As Heidegger (1967, 150) reads it, "this saying testifies to his [Kant's] sure presentiment of this revolution which he had initiated . . . roughly speaking, this means that from now on this appearance [that logic is finished and complete] proves itself to be void. Logic is newly founded and transformed." This, I think, is exactly right.

4.3 Kant's Transcendental Logic

I have suggested that we need to recognize two very different modes of intentional directedness, that of natural language and everyday life, and (beginning with Descartes) that of modern mathematical science as mediated by the symbolic language of arithmetic and algebra. In the first mode, we experience things sensorily, effortlessly taking in perceptible things as what and how they are. In the second, our intentional directedness on things without the mind requires (as Descartes thinks of it) an act of will. We do not in this mode simply take in how things are but instead represent them, mirror them in acts of thought by way of ideas. And although in fact this second shape of consciousness is made possible only by way of a transformation, a metamorphosis of the first, it is taken to be, understands itself to be, not only self-standing but originary. From the perspective of this second mode of intentional directedness, the first amounts to nothing more than a naïve prejudice in favor of sensory experience, the false assumption that sense experience correctly represents things as they are. Sense experience, now to be conceived as something that is caused in us by the impacts of mere matter on our sense organs, provides only data in the light of which the autonomous intellect theorizes (in the language of arithmetic and algebra—that is, in Newton's physics) about how, given the appearances of things in sense experience, things must actually be.

As Kant sees, there are two difficulties. First, how can sense experiences that are caused in us have any justificatory status at all; how can they provide reasons, even if

[23] The words are Russell's (1914, 50), though he was not talking about Kant's division of intuitions and concepts. Russell was talking about Peano's distinction between singular and general sentences and thereby between singular terms and predicate expressions.

only in the form of data constraining our scientific theories? The second is to show how it is we are justified in bringing our innate or a priori ideas to bear in our judgments about how things are. Kant has independent grounds for thinking that mathematics is grounded in space and time conceived as forms of sensibility.[24] Because sensibility is a receptive faculty through which objects are given there is no problem of dogmatism for this case: "since an object can appear to us only by means of such pure forms of sensibility, i.e., an object of empirical intuition, space and time are thus pure intuitions that contain a priori the conditions of the possibility of objects as appearances and the synthesis in them has objective validity" (A89/B121–2). The problem is to show that the pure concepts, as concepts of the understanding, likewise have objective validity. The way to solve both problems—both that of the justificatory role of experience and that of the objective validity of the pure concepts—is, as Kant eventually sees, by coming to realize that these are not two different problems but instead aspects of one: the pure concepts have objective validity because it is only in virtue of their involvement that sense experience can amount to experience *of an object*, where an object just is that to which judgment is answerable. Sensibility and understanding are *inextricably* combined, both in experience with respect to which one is passive, and in judgment conceived, not now, we will see, as an act of will, but instead as an act of spontaneity. There are not two modes of involvement of mind and world, as it had seemed to Descartes, but only two aspects of any cognition. Cognition just *is* thought of a given object through concepts.

Descartes' new mathematical practice transformed our understanding both of ourselves and of the world. The modern subject is autonomous, distinctively free, and the world is radically other, a merely causal structure the laws of which it is the task of theoretical (Newtonian) science to discover. But as Kant came explicitly to see, the autonomy of the subject, the knower, is empty if it is not rationally constrained by what is. Our autonomy, the autonomy of a finite knower, *constitutively* involves answerability, and that requires in turn that experience constitute a tribunal for judgment. But experience can be such a tribunal only if it is already conceptually articulated. As we will see in more detail below, it is for just this reason that intuitions and concepts not only *are*, as we saw in the previous section, but *must be* distinguished logically, as singular and general, metaphysically, as to their origins, whether in sensibility or in the understanding, *and* ontologically, as to their form, as, respectively, representations and rules.

Kantian concepts are at once predicates of possible judgments and that through which objects are thought, that is, anything for consciousness at all. Kantian intuitions correlatively give objects and are the source of all objectivity. All thought is through concepts, and all content is empirical content. And yet science, as opposed

[24] As we have seen, in early modern mathematics space must be conceived as a given whole of possible relations. It is not, as on our most basic view of space, derived from our experience of the relative locations of objects.

to, say, natural history, seeks to discover laws. In mathematics, we have seen, the discovery of laws, that is, necessary relations among concepts, is made possible by the mathematician's activity, by the construction of concepts in pure intuition. Mathematical cognitions are intuitive cognitions, cognitions of objects, though only as to their form. Philosophical cognitions, Kant thinks, are instead discursive, that is, "rational cognitions from concepts" (A713/B741). His problem is to understand how there can be synthetic a priori knowledge in this case given that "the understanding cannot yield synthetic cognitions from concepts at all" (A301/B357–8). "From mere concepts no synthetic cognition but only merely analytic cognition can be attained" (A47/B64–5). How, then, are the rational cognitions of philosophy, cognitions from concepts, possible?

In any synthetic judgment, because the predicate is not contained in the concept of the subject, some third thing is required to mediate the relationship of the predicate to the concept of the subject. If the judgment is a posteriori, that third thing is experience, that is, empirical intuition; in this case, it is in experience (if at all) that one finds the two concepts related. But as Hume already saw, experience can ground only comparative universality, never strict universality (or necessity). Synthetic a priori judgments thus require a different ground. In the case of mathematics that ground is, again, pure intuition, that is, intuition but only as to its form. Kant furthermore seems to suggest that this is the *only* remaining option:

> on what does our understanding rely in attaining to such absolutely necessary and universally valid truths? There is no other way than through concepts or through intuitions, both of which, however, are given, as such, either a priori or a posteriori. The latter, namely empirical concepts, together with that on which they are grounded, empirical intuition, cannot yield any synthetic proposition except one that is also merely empirical, i.e., a proposition of experience; thus it can never contain necessity and absolute universality of the sort that is nevertheless characteristic of all propositions of geometry. Concerning the first and only means for attaining to such cognitions, however, namely through mere concepts or a priori intuitions, it is clear that from mere concepts no synthetic cognition but only analytic cognition can be attained. (A47/B64)

There are concepts and there are intuitions, both of which can be pure or empirical. Empirical concepts and intuitions can ground only empirical or a posteriori propositions; and pure concepts can ground only analytic propositions. If there are to be grounded synthetic a priori judgments then only one option remains: pure intuition. But that, Kant recognizes, simply cannot be right. If the transcendental deduction is to succeed, there must be another way.

Were concepts, for Kant, merely general representations of objects belonging to spontaneity and intuitions singular representations of receptivity—if, that is, they were two sorts of representations that could, for instance, be combined in a judgment as a singular term and a predicate are combined, say, in the judgment that Socrates is mortal—the possibility of two uses of reason, the one intuitive (in mathematics) and the other discursive (in philosophy), would be unintelligible. As grounded in concepts, all philosophical knowledge would be analytic hence not scientific knowledge properly speaking at all. But if, as Kant came to think, concepts and intuitions are conceived instead as two *aspects* of *any* cognition, the one constituting an active moment in one's cognitive relationship to an object and the other a passive moment in that same relationship, the situation is very different. Kant's two uses of reason, the one mathematical and the other philosophical, though they cannot be grounded in two sorts of representations, can, we will see, be grounded in the two moments, active and passive, that together, Kant now thinks, constitute any human cognition. The problem of metaphysics, of how it is possible for metaphysics to be a science, can be solved, but only if concepts are not merely general representations of the understanding but rules that are empty independent of any relation to any object—intuitions, correlatively, providing the matter of all cognition—only, that is, if cognition essentially involves both concepts and intuitions as logically, metaphysically, *and* ontologically distinguished.

Philosophical cognition, we are told in the Discipline chapter of the *Critique*, "considers the particular only in the universal, but mathematical cognition considers the universal in the particular, indeed even in the individual" (A714/B743). That is, as Kant immediately points out, the two sciences differ not in their matter, their objects (the one concerned with, say, qualities, the other with quantities), but instead in their form, in the way they approach their objects. And they are different in this way

> precisely because there are two components to the appearance through which all objects are given to us: the form of intuition (space and time), which can be cognized and determined completely a priori, and the matter (the physical), or the content, which signifies a something that is encountered in space and time, and which thus contains an existence and corresponds to sensation. (A723/B751)

As we learn in the Transcendental Aesthetic, perception involves both an a priori or formal aspect and also an a posteriori or material aspect. The latter, sensation, is "the effect of an object on the capacity for representation, insofar as we are affected by it" (A20/B34). As we learn in the Transcendental Deduction, perception also necessarily involves the synthetic unity of apperception, and thereby the pure concepts, without which it would not be *conscious* perception at all. Perception thus involves *two* a priori or formal aspects, one on the side of receptivity through which objects are given and another on the side of spontaneity through which they are thought. It is just these two aspects that are addressed in the two a priori sciences. Mathematics is the science of the formal intuitional aspect as grounded in space and time, and philosophy, in particular, transcendental logic, is the science of the formal conceptual aspect as grounded in the pure concepts. Given the fundamental differences between these two aspects of cognition—the one passive through which an object is given and the other active through which it is thought—there are, correspondingly, fundamental differences in the methods of the philosopher and the mathematician.

The mathematician considers the universal in the particular, triangles, for instance in the drawing of one triangle (though, as we have seen, the reasoning involved is nonetheless general throughout). The philosopher instead considers the particular in the universal, that is, an object but as thought. The object for the philosopher is an object of possible experience, and because philosophy is grounded in the understanding, as mathematics is grounded in sensibility, the philosopher considers the concept *object of possible experience*, the particular, an object, but in the universal, in its concept. Because, finally,

> the transcendental concept of a reality, substance, force, etc., . . . designates neither an empirical nor a pure intuition, but only the synthesis of empirical intuitions (which thus cannot be given a priori), and since the synthesis cannot proceed a priori to the intuition that corresponds to it, *no determining synthetic proposition but only a principle of the synthesis of possible empirical intuitions can arise from it.* (A722/B750; emphasis added)

The synthetic a priori propositions of philosophy, unlike those of mathematics, are not determining propositions but only principles of synthesis. They have the form not of claims but of rules. Unlike the synthetic a priori judgments of mathematics, they are thus properly transcendental in Kant's sense, and indeed the only transcendental propositions.[25] Transcendental propositions "contain merely the rule in accordance with which a certain synthetic unity of that which cannot be intuitively represented a priori (of perceptions) should be sought empirically" (A720–1/B748–9). Thus is born a new science, transcendental logic, the concern of which is "the laws of understanding and reason, but solely insofar as they are related to objects a priori" (A57/B81–2).

According to Kant, we have seen, all objectivity lies in relation to an object. Because, he thinks, general logic abstracts from all relation to any objects, and it is objects (given in intuitions) that provide thought with content on Kant's mature conception of judgment, this logic concerns only form: "in [general] logic . . . the understanding has to do with nothing further than itself and its own form" (B ix). "It abstracts from all contents of the cognition of the understanding and of the differences of its objects, and has to do with nothing but the mere form of thinking" (A54/B78). Logic comes in this way, and for the first time in history, to be seen as distinctively formal, that is, as concerned with form as it contrasts with content.[26] And given this new conception of logic—that is, what Kant now calls general logic—as formal because independent of any relation to any object and hence without content, yet another science of logic, one that is not formal, can now come into view, at least as a possible science. If it can be shown, in the Transcendental Deduction, that there are a priori concepts of the understanding corresponding to

[25] "A transcendental proposition is therefore a synthetic rational cognition in accordance with mere concepts, and thus discursive, since through it all synthetic unity of empirical cognition first becomes possible, but no intuition is given by it a priori" (A722/B750).

[26] That Kant's logic was distinctively formal was recognized already by Trendelenburg in his *Logische Untersuchungen* (1870): "the idea of formal logic originated with Kant" (quoted in Sluga 1980, 49).

the pure intuitions of sensibility, then it will follow that there is a science grounded in such concepts. Transcendental logic is this science, at once a logic, that is, a science concerned with rules of the activity of thinking, and also transcendental, "occupied not so much with objects but rather with our a priori concepts of objects in general" (A11), that is, "with our manner of cognition of objects insofar as this is to be possible a priori" (B25). Whereas general logic "has to do with nothing but the mere form of thinking" (A54/B78) and hence provides only "the negative condition of all truth" (A59–60/B84), transcendental logic has to do "with the laws of the understanding and reason but solely insofar as they are related to objects a priori" (A57/B80). Transcendental logic is for just this reason a logic, or science, of truth: "for no cognition can contradict it without at the same time losing all content, i.e., all relation to any object, hence all truth" (A62–3/B87).

I have suggested that Kant's key insight on the side of mathematics is that although the new mathematical practice inaugurated by Descartes does not use images or Euclidean diagrams, but instead symbols and equations, it is nonetheless essentially constructive. Like Euclidean diagrammatic reasoning, it constitutively involves a written system of signs, and hence both perceptual and motor skills. It is not, as Descartes thought, the work of the pure intellect. Instead, Kant argues, we need to focus on the *process* of mathematical proof, that is, the activity of constructing, of paper-and-pencil reasoning, if we are to understanding the nature and possibility of our knowledge of necessary but non-logical relations in mathematics; for it is this process that reveals the necessary bonds among concepts that constitutes knowledge in mathematics, that "[brings] forth the truth together with the proof" (A734/B762; cf. B15–16).

The practice of natural science is very different according to Kant. It achieves knowledge of its laws, which like the truths of mathematics involve necessary but non-logical relations, not through constructions but through postulation and experimentation: "reason, in order to be taught by nature, must approach nature with its principles in one hand, according to which alone the agreement among appearances can count as laws, and, in the other, the experiments thought in accordance with these principles" (Bxiii).[27] On Kant's account, our inquiries into the nature of the empirical world become *science* properly speaking only when we learn to approach nature not as a student awaiting instruction but "like an appointed judge who compels witnesses to answer the questions he puts to them" (Bxiii). Empirical inquiry is in this way constitutively self-correcting. And much as space and time are the preconditions of the constructive activities of mathematics so, we will see, the pure concepts of the understanding, in particular those under the title of relation, are the

[27] As noted already, modern science—by contrast with ancient science, within which we can discover the natures of things by observing behaviors that express those natures—cannot be inductive. Because it understands nature in terms of the notion of a thing as governed by independently intelligible, exceptionless causal laws, it must, as Kant sees, be instead postulational, explicitly theoretical.

precondition of the activity of self-correction in natural science.[28] This activity, it will be argued, is in turn the key to understanding the objectivity of judgments in the exact sciences on Kant's account.

4.4 The Forms of Judgment

The understanding is a faculty of judging. Hence, Kant supposes, we can determine its form by abstracting from all content of judgments.[29] The result is given in Kant's table of judgments with four titles and three moments under each. The titles are: quality, quantity, relation, and modality. They concern, respectively, the subject concept of the judgment, the predicate concept, the relationship of the subject and predicate concepts, and the relationship of the judgment to "thinking in general."[30] As to quantity, a judgment is either universal, particular, or singular; that is, it addresses either all things, or only some things, or just one thing of a certain kind (as thought through the subject concept)—though, as Kant adds both in the *Jäsche Logic* and in the *Critique*, for the purposes of general logic, the concern of which is inference, singular judgments can be treated as universal. Only in transcendental logic, the concern of which is cognition or judgment (that is, truth), do we need to recognize and distinguish all three sorts of judgment: universal, about all objects of a certain sort, particular, about only some objects of a certain sort, and singular, about one object in particular as thought under some concept.

Under the title of quality, judgments are either affirmative or negative or infinite, though here again the last division is of no concern to general logic. In general logic infinite judgments can be subsumed under affirmative judgments. In transcendental logic they cannot. An affirmative judgment affirms a predicate of a subject. In such a judgment "the subject is thought *under* the sphere of the predicate" (Kant 1800, 600; AK 9:103). In a negative judgment the subject is in a way thought outside the sphere of the predicate; in a negative judgment one denies a content, denies that the predicate applies. In an infinite judgment, which in general logic is treated as a kind of affirmative judgment, one affirms that the predicate does not apply; one affirms, in other words, that the subject lies outside the sphere of the predicate, in some other sphere: "in the *infinite* it [the subject] is posited in the sphere of a concept that lies outside the sphere of another" (Kant 1800, 600; AK 9:104). To judge (infinitely) of something that it is not A for some concept A "it is not thereby determined, concerning the finite sphere A,

[28] The forms of judgment falling under the titles of quantity and quality are preconditions of all science, mathematics as much as physics; only those under the title of relation (and modality, but these do not concern the content of judgment) are peculiar to the practice of the natural, empirical sciences.

[29] We saw in section 4.2 that a Venn diagram illustrates the relevant distinction of form and content for the case of categorical propositions. The overlapping circles represent two concepts that provide the matter for judging; the shading or placement of an 'x' serves to give the form at least as to quantity and quality.

[30] Because Kant recognizes that not all judgments are categorical in form, the notion of a subject concept and a predicate concept must be very broadly construed here.

under which *concept* the object belongs, but merely that it belongs to the sphere outside A, which is really no sphere at all but only *a sphere's sharing of a limit with the infinite, or the limiting itself*" (Kant 1800, 600; AK 9:104). Whereas in a negative judgment one denies some content but does not affirm thereby something in its stead, in an infinite judgment one denies that the predicate applies in a way that affirms that some other predicate applies instead. (See also A71–2/B96–7.)[31]

This distinction between negative and infinite judgments, between denial and negative affirmation, is furthermore essential in transcendental logic because it enables Kant at once to make the negative judgment that, for instance, it is not the case that things in themselves are spatial, that is, to deny the claim that things in themselves are spatial, while avoiding making the infinite (affirmative) judgment *of* things in themselves that they are non-spatial. The former, negative judgment is not an assertion about things in themselves as the latter, infinite judgment is; only the infinite judgment would commit one to there being a concept under which the object of the subject concept belongs (though not which it is). The negative judgment denies that a particular concept applies to the subject concept, but unlike an infinite judgment it involves no commitment to the possibility of applying any other concept. It is just this distinction between negative and infinite judgments that enables Kant, in the B Phenomena and Noumena section, to distinguish between a negative sense of noumenon, as "a thing insofar as it is not an object of our sensible intuition, because we abstract from the manner of our intuition of it," and a positive sense, which would be "an object of a non-sensible intuition" and hence would assume a special kind of intuition different from ours (B307). We cannot have any conception of a noumenon in the latter, positive sense; but we do have a conception of a noumenon in the former, negative sense. Indeed, Kant thinks, "the doctrine of sensibility is at the same time the doctrine of the noumenon in the negative sense" (B307).

The third title in Kant's table is that of relation. And here we have something essentially new. It is needed because although Kant agrees with the tradition in taking all judgments to have the form of a subordination of representations one to another, he denies that there is only one manner of subordination. There are, he thinks, three logically different forms of subordination: "as *predicate* to *subject*, or as *consequence* to *ground*, or as *member of the division* to the *divided concept*" (Kant 1800, 601; AK 9:104). The latter two are distinguished from the first in involving problematic judgments (that is, judgments the truth or falsity of which is not in question in the judgment as a whole).[32] As Kant puts the point for the case of the second form of subordination in the Logic:

[31] Aristotle makes what is essentially this distinction in the form of a distinction between a denial that is the assertion of its negation (Kant's infinite judgment) and a denial that is not, and does not entail, the assertion of its negation (Kant's negative judgment).

[32] To recognize these different forms of subordination is thus to require that one also take into account what Kant thinks of as the modality of a judgment, its relationship to thinking, whether problematic, assertoric, or apodictic.

In categorical judgments nothing is problematic, rather everything is assertoric, but in hypotheticals only the *consequentia* [that is, the relation of judgments as ground and consequent] is assertoric. In the latter I can thus connect two false judgments with one another, for here it is only a matter of the correctness of the connection—the form of the *consequentia*, on which the logical truth of these judgments rests. There is an essential difference between the two propositions, All bodies are divisible, and, If all bodies are composite, then they are divisible. In the former proposition I maintain the thing directly, in the latter only under a condition expressed problematically. (Kant 1800, 602; AK 9:105–6)

In a disjunctive judgment similarly, the disjuncts themselves are not thought as true but only the whole disjunction. To claim, for example, that "a learned man is learned either historically or in matters of reason" (Kant 1800, 603; AK 9:107) is not to claim either that a learned man is learned historically or that a learned man is learned in matters of reason, but only that he is one or the other. (One or the other but not both: it is clear that Kant thinks of the disjuncts of a disjunction as both mutually exclusive and jointly exhaustive.) In each case, then, we have a relation among concepts and indeed a kind of subordination, but in three different modes.

The final title, modality, is unlike the other three "in that it contributes nothing to the content of the judgment (for besides quantity, quality, and relation there is nothing more that constitutes the content of a judgment) but rather concerns only the value of the copula in relation to thinking in general" (A74/B99–100). As already noted, the antecedent and consequent of a hypothetical judgment as well as the disjuncts in a disjunction are problematic, while the whole is, or at least can be, assertoric. That is, it is only the whole that is truth-evaluable in the judgment even though taken as freestanding such judgments (that is, those in the antecedent and consequent of a hypothetical or the disjuncts of a disjunction) are instead assertoric.

As concerns content, then, Kant's table has three titles and three moments under each title, where according to the tradition before him there were only two titles, quantity and quality, and only two moments under each. As already indicated, Kant himself thinks that the third moment, in both the case of quantity and the case of quality, is properly not a concern of general logic, though it needs to be in place in the interests of transcendental logic. Regarding the title of relation, he holds that the tradition erred. Not all judgments are categorical in form; there are in addition both hypothetical and disjunctive forms. And they are needed, we will soon see, because only so can we understand the striving for truth as a self-correcting enterprise.

Obviously the question of truth cannot arise unless there is a judging *of* something *as* something. But merely classifying something as something is not thereby to judge. After all, even animals classify things by their differential responses to them. The question is, what else is involved in the case of judgment? Kant suggests already in the False Subtlety essay that it is the capacity to reflect, to "make one's own representations the objects of one's thought" (Kant 1762, 104; AK 2:60). Certainly we, unlike mere animals, do self-consciously classify things. We also reason. But animals do that too, at least in a way, insofar as they have habits not only of response to things but also of processing information. (The dog chasing a rabbit along a path comes to a fork in the path; it sniffs down one path then, picking up no scent, straightaway runs down the other.) What distinguishes us is that we can make our reasons explicit as reasons in the form of conditional judgments, and for that we need hypothetical as well as categorical forms of judgment, not only judgments about the As that they are (some, one, or all) B—that is, as Kant thinks of it, that the concepts A and B are combined in objects—but also judgments to the effect that if something is or were an A then it is or would be B.[33] We have already seen that Kant distinguishes (as perhaps we would not) between the judgment that all As that are B are C (categorical) and the judgment that all As are, if B then C (hypothetical). In the first one asserts of the As that are B that they are C; in the second, one does not assert anything categorical of the As: "only the consequence is assertoric" (Kant 1800, 602; AK 9:106). One in this way displays what is a sufficient ground for a judgment without asserting either the ground or the judgment. The capacity to do this is at the same time the capacity critically to reflect on one's conception of what is a reason for what.

Concepts, on Kant's view, whether they are pure or empirical, contain concepts as marks. In the case of the pure concepts, we discover these marks by analysis. In the empirical case they are discoverable only a posteriori. Suppose, for instance, that we come to think, based on our sensory experiences, that whales are a species of fish, where the concept of a fish contains as marks being cold-blooded, having gills, and so on. Were we merely in the habit of thinking that whales are fish, second thoughts would be impossible. But we can have second thoughts in such cases. That is, we can discover on further investigation that, say, whales have lungs rather than gills, and are warm- not cold-blooded. We had been wrong about what whales are. They are not a species of fish. To have such second thoughts about things requires not only categoricals, judgments about, say, whales, that they are fish, that they are warm-blooded, but also hypotheticals, that being a fish entails being cold-blooded. Only in this way can one correct, rather than merely change, one's judgment that whales are fish.

We do not merely have habits of response as non-rational animals do; we have the capacity to *judge* and that means, Kant argues, that we can reflect, and on reflection correct our judgments so as better to accord with how things are. Indeed, as we saw already in Chapter 1, we can have second thoughts at all only because concepts are constitutively inferentially related. Second thoughts are not merely changes of mind (yesterday I held that p but today I affirm not-p) that are caused in us; they are reasoned changes of mind (yesterday I held that p but then I discovered that not-q and since p entails q I came to see that not-p). We can have second thoughts only if our thoughts are inferentially related one to another; only so can they conflict with one another and thereby provide grounds for changes of mind.

[33] See, for instance, Sellars (1981) for discussion of this way of thinking about the difference between us rational animals and other animals.

That concepts must be inferentially related to one another if judgment, as contrasted with mere classification, is to be possible is a familiar theme from, for instance, Sellars (following Kant).[34] And Sellars, like Kant, holds that such relations—if they are to be properly inferential, to provide *grounds* for judgment rather than merely *causes* of judgments—must themselves be available to be the contents of judgments. There can be categorical judgments, which describe rather than merely label, only if there are also hypothetical ones. But even this is not sufficient for judgment on Kant's view. One also needs the capacity to think disjunctively. This is harder to understand. Why are hypothetical judgments, and the inferential relations they articulate, not sufficient, together with categorical judgments, for inquiry, the striving for truth? And if something more *is* needed, why is it disjunctive judgment?

Transcendental logic is to be a logic of truth. Its task is to clarify the striving for truth in the natural sciences (paradigmatically, Newtonian physics), that is, our capacity for inquiry into how things are, for *knowledge*, by showing how self-correction is possible, how it is we can have not merely changes of mind but second thoughts, that is, reasoned changes of mind. Our problem is to understand why inferential relations among concepts, as expressed in hypotheticals, together with categorical judgments regarding how things are, are not sufficient for self-correction. We have already seen that if we have both categorical and hypothetical judgments then we can correct our categorical judgments by appeal to hypothetical judgments. If being A entails being B and we find an object that we take to be A but clearly is not-B, then we can judge that that object is not, but only appears to be, A. Whales look quite like fish but we know that they are not fish, because they are warm-blooded, and it is incompatible with being a fish to be warm-blooded. But we can also correct our hypothetical judgments in light of our categorical judgments. Kant, for example, takes it that gold is a yellow metal, that what it is to be gold is to be a yellow metal. We have since discovered that that is not right, that the yellow color of gold is due to impurities. And in most cases of conflict between our categorical judgments and our hypothetical judgments, it will be reasonably clear how the conflict is to be resolved. But there is no guarantee of this. In some cases, it will seem that *none* of the conflicting claims can reasonably be given up. In such cases, what needs to be revised is not merely a categorical or a hypothetical but our conception of what is possible at all.

The ancient problem of change (rehearsed already in the Introduction) provides a simple illustration of the point. Change is pervasive in our lives, but before Aristotle it seemed that change is impossible insofar as it cannot come from what is or from what is not. We have an inconsistent triad: (1) there is change; (2) change cannot come from what is (because it already is); (3) change cannot come from what is not

[34] Sellars (1958, 306–7) writes that "it is only because the expressions in terms of which we describe objects, even such basic expressions as words for the perceptible characteristics of molar objects, locate those objects in a space of implications, that they *describe* at all, rather than merely label." See also Sellars (1948b).

(because nothing comes from what is not). Each of the three claims taken alone seems compelling. That there is change seems manifest in experience; and that nothing can come either from what is (because it already is) or from what is not (because nothing comes from nothing) also seems to be obviously true. The conflict cannot be resolved, so it seems, by rejecting *any* of our three claims.

Hume's skeptical argument is essentially similar. We have, we think, knowledge of causal relations. Hume shows that such knowledge cannot be either a relation of ideas, known by reason alone (because it is not logically necessary), or a matter of fact, that is, based on experience (because though not logically necessary it is necessary). But surely, Hume thinks, such knowledge could only be based on the one or the other. In this case, as in the case of change, what needs to be revised is not merely a categorical judgment or even a hypothetical one but the whole space of possibilities within which thought moves. And the same is true of Kant's antinomies: that the world both must and cannot be finite in space and time; that composites both must and cannot consist of simple parts; that the causality of freedom both must and cannot be compatible with the causality of nature; and that there both must and cannot be a necessary being as the ground of contingent existence. Indeed, antinomy is at the heart of the whole of Kant's critical enterprise insofar as it appears that the reality on which thought aims to bear both *must* rationally constrain judgment, if judgment is to be rational, to amount to knowledge, even if only in some cases, and *cannot* rationally constrain judgment insofar as reality is a merely causal structure that has no inherently normative significance.

In cases involving only categorical and hypothetical judgments either experience contradicts some purported inferential connection among concepts or some conceptual connection leads to a contradiction among putative experiences. In the cases of concern here we end up contradicting *ourselves* insofar as none of the available options seems to make any sense. We need, then, a third conception of judgment, and it must be just the sort of disjunctive judgment Kant provides.

Suppose that categorical and hypothetical judgments were the only two forms of judgment involved in inquiry, that they were together sufficient to ground our second thoughts. There would in that case be no guarantee that there will not be a stalemate between them, that is, a case in which we find ourselves wanting to affirm that A, that not-B, and that A entails B. And yet there cannot be an irresolvable stalemate for reason. Although it would be merely dogmatic to assume that we have already in hand the conceptual resources that are needed to understand reality as it is, it would be a form of skepticism to deny that we can achieve such resources as are needed. To avoid dogmatism we need to recognize that we may well find ourselves with an inconsistent triad that we simply do not know how to resolve by the usual means; to avoid skepticism we need a form of judgment enabling us to express the difficulty in the form of a judgment and so reason about it. The form of judgment that is needed is precisely what Kant gives us, a disjunctive judgment of the form 'A is (B or C)', where the disjuncts are mutually exclusive and the sphere of possibilities exhaustive. By making our current understanding of all the possibilities explicit in this way, we are at the same time put in a position to come to understand new possibilities that will resolve our difficulties. Kant's table of the pure concepts of the understanding establishes in this way the preconditions for our second thoughts and thereby the science of nature.

Kant aims to uncover the conditions of possibility of the activity of scientific inquiry and argues that in addition to the concepts involved in any and all inquiry, mathematics as much as the empirical sciences, there are three pure concepts of the understanding under the title of relation, that of substance to accident, that of cause to effect, and that of community or reciprocity. I have suggested that this is correct because inquiry is rational not insofar as it has a foundation in something given but insofar as it is a self-correcting enterprise, and in particular insofar as it is self-correcting not merely as regards judgments and relations of entailment, but even as regards our conceptions of what is possible at all. There can be classificatory judgments, substance and accident, only because there are also judgments of cause and effect, more generally, antecedent and consequent, by which to correct classificatory judgments. But Kant sees that even hypothetical judgments are possible only in light of judgments of community or reciprocity. Knowledge, in other words, requires more than the distinction between an object and my experience of it, and more than things with properties in causal (more generally, inferential) relations. It requires also an understanding of being, a horizon of conceivability, of what is so much as possible that is not captured already by the notion of a causal law.[35] Indeed, we have seen, the very idea of a causal law reflects an understanding of being that has its historical moment and is not merely given. The same is true of the notion of representation, and indeed of knowledge; for, as Kant argues, we have fundamentally misunderstood the nature of both. Properly conceived, he thinks, our knowledge can be of things only as they appear to us. Critique of our most fundamental understanding of what is requires, then, more than propertied objects in causal relations; it requires a conception of the whole, and that is what Kant's third relation gives us. With the rise of modern science we came to a radically new understanding of the most fundamental nature of reality, and thereby a new understanding of ourselves and of the nature of knowledge. Kant's table of judgments reflects this. It articulates the forms of judgment that are required if physics, and philosophy, are to be put on the sure path of a science, capable of articulating not only the facts, not only the laws of nature, but the very form that scientific knowledge must take to be knowledge at all.

4.5 Kant's Metaphysics of Judgment

How is knowledge possible? In asking this question Kant assumes that he knows already what knowledge is. Significantly, Plato did not make this same assumption.

[35] Compare Haugeland (1998).

Plato's dialogue *Theaetetus* examines just this latter question: what is knowledge? Three answers are explored—knowledge is perception, knowledge is true judgment, and knowledge is true judgment with an account—and as is characteristic of Plato's Socratic dialogues all are ultimately rejected. Plato nonetheless provides us with many indications, some subtle and some not so subtle, that it is the first answer that comes closest to the truth.[36] Although sensory perception of what is changing or in flux cannot amount to knowledge (for reasons that are rehearsed in the first half of the dialogue), perception with the eyes of the mind of what is unchanging and precisely what it is, nothing more or less, perception that is achieved through just the sort of Socratic examination that is displayed in *Theaetetus*, seems to Plato (at least as I read him) to be knowledge properly so called. Knowledge, on this account, is the cognitive grasp of an object in its nature, as what it is. Judging, although it can be true or false, the holding together or apart, in thought or in speech, what is (or is not) in fact, is not knowing. Judging, even judging truly and with justification, does not put one in the cognitive relationship with something that is constitutive of knowledge.

According to Aristotle, our capacity for judgment is, like any rational capacity, a two-way capacity, at once the capacity to hold together, to affirm, and the capacity to hold apart, to deny.[37] Judgment as Descartes understands it is essentially different insofar as it is an act not of the power of thinking but of the will and so of freedom in the modern sense of action for a (good) reason. As Descartes explains in the fourth meditation, the better or stronger my reason for the affirmation or denial, the freer is my act of judgment. And as is made explicit in Kant's formulation of the Categorical Imperative as the supreme principle of morality, the very *best* reason is the reason that is the same for all rational beings, a good reason for reason itself. Truth, then, is (as Frege says) what is the same for all rational beings. And we can know truths so characterized according to Descartes because the mark of truth is clarity and distinctness: what I clearly and distinctly perceive to be so is (in light of God's goodness) actually so, that is, true. This is, we can suppose, a fallible power in finite beings: what I perceive clearly and distinctly is true but I can be mistaken in thinking that I clearly and distinctly perceive, for example, through habitual belief. (See the third meditation.) As Rödl (2007, 159, n. 24) notes, "it is difficult to develop and maintain the

[36] The examination of this answer is the longest of the three and takes up fully half of the dialogue; and the examination of the second and third answers are not only shorter but also prefaced by the strange digression in the middle of the dialogue, which seems to present a parody of the philosopher. The most direct evidence that Plato thinks that knowledge is a kind of perception, and in particular, a matter of seeing with the mind's eye, is a remark he has Socrates make at the end of the discussion of Theaetetus's claim that knowledge is (sensory) perception. Socrates says: "we are not going to grant that knowledge is perception, *not at any rate on the line of inquiry which supposes that all things are in motion*" (183c; emphasis added), suggesting thereby that knowledge is a kind of perception of what is unchanging, a mental grasp of, for instance, the mathematicals and the Forms.

[37] See Makin (2006) for discussion of Aristotle's notion of rational capacities as two-way capacities.

power to perceive things clearly and distinctly, but she who possesses the power acquires knowledge in its acts."[38]

Descartes holds that through the power of reason alone, properly exercised, one can, with an appropriate act of will, achieve knowledge of things as they are, the same for all rational beings. Kant denies that reason is such a power. The power of judgment as Kant understands it resides not in pure reason but in the faculty of understanding the exercise of which requires also sensibility. Judgment is not, then, a manifestation of freedom (of the will) according to Kant but instead of the spontaneity of the understanding, of its capacity for self-activity (B130, B132). Freedom is a causality of the will, the capacity to act on something in accordance with one's conception of a law or rule, and ultimately, the capacity to act for the sake of the moral law. The exercise of spontaneity is not a case of acting on something, a causality of the will, but instead, as the self-activity of the understanding, is the activity of a constitutively self-conscious being.[39] As the point might be put, judging for Kant is not something we do in the sense of a productive act, a making of something, either out of something or *ex nihilo*, but instead something we are. Judging so conceived does not happen in time the way a productive act does but is instead the actualization of our power of knowing much as seeing is the actualization of our power of sight. It belongs to our nature as rational and as such is expressive of us as knowers. It is also explicitly and essentially self-conscious. *Judging*, at least as Kant understands it, cannot be merely habitual and unreflective as our everyday affirmations and denials often are, and as asserting, as Aristotle understands it, can be. Judging, as Kant understands it, is essentially different both from Descartes' conception of it in terms of an act of will and from affirmation and denial in Aristotle.

What we are inclined to think on the basis of perception or common knowledge (what "we" say) is what Kant in the *Prolegomenon* calls a judgment of perception. Among judgments of perception are, for example, that the room is warm, the sugar sweet, the wormwood repugnant. Such judgments, Kant says, "express only a relation of two sensations in the same subject, namely, myself, and this only in my present state of perception, and are therefore not expected to be valid for the object" (Kant 1783, 93; AK 4:299). These particular judgments are, furthermore, incapable of being transformed into what Kant calls judgments of experience, that is, objectively valid empirical judgments, "because they refer merely to feeling—which everyone acknowledges to be merely subjective and which must therefore never be attributed to the object—and therefore can never be objective" (Kant 1783, 93 note; AK 4:299 note). Sensory qualities such as warmth, sweetness, and disgustingness are not qualities of

[38] In fact, in Descartes' view, one has knowledge only in assenting to a clear and distinct perception, where assent is an act of will. The idea that already in a clear and distinct perception one has knowledge, because to have a clear and distinct perception is to exercise a power of knowing, is a Kantian insight, not a Cartesian one.

[39] I have been influenced here by Longuenesse (1998).

objects but only sensations caused in us by objects. Hence, these judgments of perception (that the room is warm, the sugar sweet, the wormwood repugnant), which connect perceptions within my mental states, cannot be the ground of a judgment of experience, at least not directly. "It is not, as is commonly imagined, sufficient for experience to compare perceptions and to connect them in one consciousness by means of judging" (Kant 1783, 94; AK 4:300). That is just what judgment is on Aristotle's account, a connecting or holding together of things, but after Descartes a different account is needed insofar as "universal validity and necessity" are now to be seen as that "on account of which alone it [judgment] can be objectively valid" (Kant 1783, 94: AK, 4:300). Judgment, properly speaking, which is to say, judgment in the exact sciences, is *all things considered*, a hypothesis that is well founded in light of the evidence of the senses, essentially reflective and self-conscious. And because it is, it is the same to say that it is objective, that is, a relation in the object (and not merely in my experience of the object) as to say that it is necessary and universally valid, that *anyone* ought so to judge.

That judgment, properly speaking, is all things considered is indicated already by Kant's table of the logical forms of judgment, which, we have seen, are the forms of judgment that are necessary for and constitutive of the power of self-correction. I must consider not only what properties things appear to have, but also the causal relations that appear to obtain among things and indeed what seems possible at all, examine in this way all the data or evidence that is available to me in order to judge, on that basis, regarding what actually is so. *I* must make up *my* mind, though not in the sense of *deciding* to do something, to affirm or deny. Instead I make up my mind through the processes of inquiry that I undertake that, if all goes well, culminate in my *realizing* that things are thus and so, coming to see and so to know how things are. To make up my mind in this sense is not to take a stand, form a commitment, or make a move in a language game, but to exercise one's power of judgment in the course of critically reflective inquiry that (if all goes well) eventually shows me how things are. It is such a showing that underlies and makes possible the more standard conception of judging as an action one performs, that is, as taking a stand or forming a commitment or making a move in a language game. I, as a knower, cannot just *decide* to think this or that, but nor can I have my mind made up for me, either by habit or by an uncritical acceptance of the testimony of the senses. Instead I make up my mind through the activity of critically reflective inquiry. And I must be ever mindful of the possibility that on further examination I may need to reassess my judgment. The capacity to judge is, as any capacity is, fallible; its successful exercise requires that circumstances be propitious. It is also, though essentially the capacity of some individual self-consciousness, nevertheless a capacity to speak for all precisely because and insofar as the judgment is, in intention, necessary and universally valid, not merely as I take things to be but things as *they* are, and hence as anyone ought to judge them to be.

To judge, for Kant, is to combine concepts in an object where this act is an exercise of the power of judgment, the (fallible) capacity of the understanding to know how things objectively are as contrasted with the merely subjective feelings that things can give rise to in me. Judgment so understood is an act, that is, an exercise of a power, that puts me in a cognitive relation to objects in the world. And as grounded in the necessary unity of apperception, it constitutively involves the agreement and mutual support of all my other judgments. But again, this act, for Kant, is not an act of freedom as it is for Descartes. It is an act of spontaneity, of the self-activity of a (finite) thinker. Judgment, we are told in the B Deduction, is "the way to bring given cognitions to the **objective** unity of apperception" (B141). It answers not to how things seem to me to be but to how on reflection they must be, how they are in the objects themselves. Judgment in this way posits a relation among concepts in the objects themselves. It is, as Kant describes it in the *Metaphysical Foundations*, "an action through which given representations first become cognitions of an object" (Kant 1786, 190; AK 4:475). But as already indicated, this action is not a productive one of making something out of something; it is instead the exercise of a power, the (fallible) power to cognize, to judge, and so to know. If, following Aristotle, we take *form* to be that in virtue of which a thing is actualized as what it is, and *matter* as that which is so actualized, either as a potential is to what is actual as in the case of the production of, say, a house from bricks, or as a capacity or power is to its exercise, as a case of healing is an exercise of the craft of medicine, we can say that concepts as forms actualize the potential of representations, the matter for judgment, to be cognitions of objects. The synthesizing activity of the understanding in judgment informs the matter for judgment, the concepts that are joined in judgment, not, however, as a making of something out of something, but as the full actualization of what those concepts already are, namely, of an object. The contribution of receptivity, on this account, is not separable from that of understanding, as bricks are separable from a house. It is merely different and distinguishable from its expression in the activity of cognition as a power is different and distinguishable from its exercise.

We have already seen that spontaneity is different from the freedom of the will for Kant. Freedom of the will is the freedom to make something out of something, to realize a possibility through production. It is, then, productive freedom. Acts of productive freedom are furthermore acts we are responsible for; we are responsible for that which we bring about. Such acts are also limited by that on which one acts. Insofar as it needs materials on which to work, insofar as it does not create *ex nihilo*, productive freedom is limited and constrained. Full productive freedom would be the freedom to create from nothing, the freedom of an infinite being. Spontaneity, the active power of judgment, is different from this sort of freedom; it is instead a kind of expressive freedom, the power not to make something into something but fully to actualize something that already is, to make manifest what is otherwise latent.[40] To

[40] This conception of judgment was invoked already in Chapter 1, as the successor of intra-game moves once loci of authority have coalesced, in our account of the actions, perceptions, and judgments of a rational being.

have expressive freedom, spontaneity, is not to be responsible *for* something so much as it is to be responsible *to* something. In judgment conceived as an act of spontaneity, of expressive freedom, one is responsible to how things are, what is objectively the case. Whereas productive freedom changes things, makes actual something that was otherwise lacking, expressive freedom leaves things as they are.[41] Judgment as an act of expressive freedom is not a making but a making manifest. It is fully to actualize the power of a thought to be revelatory of how things are and, by the same token, of things to be revealed to a knower.[42]

On Descartes' account, judgment is an act of will, of productive freedom; it is something one can do with an idea much as one can do things with bricks such as make a house. The freedom of the will is furthermore infinite according to Descartes, and insofar as it is, God-like. But it is also unlike the will of an infinite being insofar as the will or productive freedom of an infinite being is the power to make something out of nothing, *ex nihilo*; our will, although perhaps in its way infinite, needs ideas on which to act, ideas that are, Descartes thinks, implanted in us by God. The infinite will of a finite being can only make something of what is given; it cannot create from nothing. Contingency and finitude are in this way incompatible with full productive freedom; only an infinite being can produce from nothing, take *full* responsibility for how things are. And this, Kant sees, means that judgment, which as an act is (fully) responsible to how things are, is not an act of will, of productive freedom at all. The power of judgment is and can only be the power of a finite being for whom there is an objective reality, a reality *not* of her own making, about which to judge. Contingency and finitude, although they are incompatible with (full) productive freedom, are not incompatible with expressive freedom. They are instead *constitutive* of it. Were there no world without the mind, no reality on which thought aims to bear that is as it is *however we take it to be*, then there could be no expressive freedom. Expressive freedom is not limited or constrained by what is, the reality on which thought aims to bear, but is instead *completed* by it. It is the power to judge of what is that it is.

Acts of judgment can be conceived either as acts of productive freedom or as acts of expressive freedom. To think, for instance, of judgment as an act of commitment, as a matter of making up one's mind about something, taking a stand, or as a move in a language game, or as an application of a concept (where that act is conceived productively, as bringing something into being), is to think of it as an act of productive freedom. It is to change something (for instance, one's score in a language game), to make there to be something where before there was no such thing. But we can also conceive acts of judgment as acts of expressive freedom, as actualizations of the capacity of (true) thoughts to be revelatory of how things are. And the notion of

[41] We have seen already that Kant makes the point that it must be possible that it is the same thing that I think and that is at A600/B628.

[42] A judgment as an act of knowing is, in Aristotle's terminology, an *energeia* (as contrasted with a *kinesis*). See Kosman (1984) and (1994) for discussion of Aristotle's notion of an *energeia*.

applying a concept can be similarly conceived. We can think of applying a concept not as something we *do* (as we build houses with bricks) but as an exercise of our capacity to know, as the realization or actualization of the potential of a thought to be revelatory, that is, as Kant would think of it, as the realization of the potential of an intuition to be *of* an object. In such a case, the object is thereby brought to mind, revealed to a subject through the intuition. The pure concepts serve precisely this function. Because and insofar as they enable second thoughts, their application (in a successful, that is, true, judgment) fully manifests the capacity of a thinker to take in how things are. To apply a concept to a given representation, an intuition, in a judgment is not to make of an intuition something it is not (as one makes a house out of a heap of bricks); it is fully to actualize the intuition as what it already is, though not yet manifestly, namely, a singular representation immediately of an object. It is in just this way that we can understand the fact that Kant claims both that intuitions are one species of cognition, the other of which is conceptual, and that both intuitions and concepts are involved in any cognition. Intuition, conceived as a potentiality, that is, as a (passive) power of cognition, is conceived in abstraction from the involvement of concepts and is in this regard the matter for cognizing. As actualized, in an intuitive cognition, intuition essentially involves the activity of the understanding.

Consider again the idea that judgment is an act of will that we find first in Descartes. Judgment so conceived is a making up of one's mind and as such is an event in one's life, a fact of one's history. What one judges (in this sense) may be true, and indeed one may even have very good reason so to judge. What judgment so conceived does not do is put one in a cognitive relationship to reality, what is. As Plato already notes in *Theaetetus*, and more recent Gettier examples similarly show, judgment so conceived is only accidentally or externally related to what is. *Knowledge*, however, requires (as seeing does) an internal relationship to what is. Insofar as it does, judgment conceived as an act of will, or indeed as a holding together or apart, cannot constitute knowledge. Rather, it is judgment in another sense, judgment as what *underlies* such acts of will (insofar as they are well-founded), that is the locus of truth and knowledge. Descartes, for example, instructs us to affirm only what we perceive clearly and distinctly. What Kant sees is that judgment, if it is to be the locus of truth and knowledge, lies not in the affirmation but in that which is necessarily prior to it as its ground, in the exercise of a capacity as such exercise contrasts with an act of will, not now the capacity for clear and distinct perception but instead the capacity for inquiry through the power of self-correction. Objectivity does lie in relation to an object on Kant's account, but what realizes this relation as a relation of knowing is the self-activity, the spontaneity, of the understanding. It is this act that brings the object to mind as an object of knowledge.

4.6 The Limits of Reflection

Kant's transcendental logic is to be the science of truth revealing the nature and possibility of knowledge. By uncovering the rules that govern inquiry, the striving for truth, it is to reveal the ground of the objectivity of judgment. Now, in nature as conceived on the modern view as the realm of natural law, form and matter are separate in the sense that the laws of nature govern the behavior of objects as from the outside. Unlike natures as Aristotle understands them, laws of nature are not principles of motion internal to things but instead laws that would hold even if there were nothing to behave in accordance with them. Our self-consciousness, the power of judgment, is not, Kant sees, in the same way subject to laws; instead (as is the case with living beings according to Aristotle) it has its own inner principle of activity. The expression of the power of judgment is self-expression. And because it is, we can on reflection discover the laws of thought, not as empirical or psychological laws that in fact govern our thought as from the outside, as laws of nature govern material objects, but as the laws of reason itself, as *our* laws insofar as we are rational. The laws of reason, on this account, are not an imposition on reason (as the law of gravity is an imposition on, say, the sun, moon, and planets) but instead constitutive of its being.[43] They are my laws, internal to my being as the rational being I am. And as laws of my self-activity they are discoverable a priori, whether in general logic, which abstracts from all relation to any object, or in transcendental logic, which concerns the laws of thought in relation to objects.[44]

That we might, on reflection, discover the laws of thought is coherent because and insofar as these laws are our laws as the rational beings we are. It furthermore seems clearly an insight that it is the spontaneity of the understanding, and in particular our capacity for self-correction as grounded ultimately in the synthetic unity of apperception, that is the ground of our capacity for knowledge. As the point has been explicated here, although productive freedom is limited by what is, that on which one acts, the expressive freedom of spontaneity is instead completed by what is. Judgment as an act of expressive freedom is and must be fully responsible to how things are, to things as *they* are, however I take them to be. The "brute externality" of reality, of what is as it is however I take it to be, is not then opposed to the spontaneity of thought but instead completes it. That reason operates freely in its own sphere is, on this account, nothing other than the subjective correlate of what is objectively the case. Kant *almost* sees this. He knows that the self-activity of the understanding must be unbounded, capable of calling *anything* into question as reason sees fit, that, again, objectivity needs to be conceived in terms of our constitutive capacities of self-correction for which nothing is Given and unquestionable in principle. And yet Kant cannot understand how this *can* be: the understanding is and can be a power of knowing *only* in relation to the receptivity of sensibility.

We have seen that according to Kant space and time cannot be transcendentally real because they are constitutively relational. He thinks, that is, that he has good

[43] They are laws of autonomy in the sense invoked in section 1.4.
[44] I owe this point to Newton (2010).

metaphysical grounds for his Transcendental Idealism.[45] But in fact this Idealism is incoherent, not because of the relationality of space and time, but because and insofar as space and time, as well as objects, must be given to spontaneity from outside of it. Indeed, a *transcendental* logic is needed for just this reason. Kant argues, we have seen, that a finite thinker cannot think an object directly through concepts because an object as an all-sided determination is infinitely complex; the concept through which to think an object as the all-sided determination it is would have to contain within it infinitely many representations as marks. No finite being could grasp such a concept. For a finite being, then, there can be cognition of an object only as an intuition, as something given to the receptivity of sense. Independent of the receptivity of sensibility through which an object is given, the understanding is not a power of judgment, *Urteilskraft*, but only a capacity to judge, *Vermögen zu urteilen*.[46]

On Kant's account, then, knowledge insofar as it is objective, that is, of *objects*, requires that an object be given (because, again, a finite being cannot think an object directly through concepts). And this, Kant sees, imposes a limitation on our spontaneity, our power of judgment. The pure concepts, as the conditions of the possibility of second thoughts, of self-correction—although valid for all finite rational beings, that is, for all beings for whom an object must be given[47]—have content only in relation to our form of sensibility. The necessity and universal validity of our judgments extend only as far as beings like us, beings whose sensibility is spatio-temporal. Because sensibility is a receptive faculty rather than a spontaneous one, because it is not self-activity but something given for self-consciousness, our capacity for self-correction is limited by this given, space and time as the forms of sensibility as well as objects themselves insofar as they are given in intuition. Our knowledge is not, then, of things as they are in themselves, the same for all rational beings, but only of things as they appear, the same for all beings like us, beings with our form of sensibility.

Since in us a certain form of sensible intuition *a priori* is fundamental, which rests on the receptivity of the capacity for representation (sensibility), the understanding, as spontaneity, can determine the manifold of given representations in accordance with the synthetic unity of apperception, and thus think *a priori* synthetic unity of the apperception of the manifold of **sensible intuition**, as the condition under which all objects of our (human) intuition must necessarily stand, through which then the categories, as mere forms of thought, acquire

[45] Kant's antinomies (that the world both must and cannot be finite in space and in time, and so on) are also fundamental to Kant's Transcendental Idealism, and indeed to the very idea of Kant's critical philosophy. See Jauernig (2011, 296–7).

[46] Kant explains the difference between capacities and powers in the Metaphysics Volckmann, 1784–5, AK 28:434: "Capacity [*Vermögen*] and power [*Kraft*] must be distinguished. In capacity we represent to ourselves the possibility of an action, it does not contain the sufficient reason of the action, which is power [*die Kraft*], but only its possibility." Quoted in Longuenesse (1998, 7, n.12).

[47] "The pure concepts of the understanding... extend to objects of intuition in general, whether the latter be similar to our own or not, as long as it is sensible and not intellectual" (B148).

objective reality, i.e., application to objects that can be given to us in intuition, but only as appearances; for of these alone are we capable of intuition *a priori*. (B150–1)

Because, for Kant, all objectivity lies in relation to an object conceived as an all-sided determination, our understanding as a power of judgment is inextricably combined with sensibility. All content is empirical content, and all truth is empirical truth, truth about objects that are given in sensibility. Something *must* be given if knowledge is to be possible; but as Kant also sees, nothing *can* be given for knowledge. The expressive freedom that is spontaneity, because it is constitutively self-activity, is inherently *self*-governed and *self*-correcting. There can be for spontaneity no given precisely because what is given is, as given, impervious to second thoughts. Indeed, that is why on Kant's account it is only as thought through concepts, as taken up and combined in the understanding, that the receptivity of sense is anything *for* us at all. But if that is right then we must further suppose that an act of productive freedom underlies and makes possible the expressive freedom of judgment. Transcendentally, the application of concepts is a making (a productive act of transcendental freedom) that brings to consciousness something that (as caused in sensibility) is otherwise as nothing to us. Only empirically is the application of concepts an actualization of the potential of intuition to be revelatory.[48]

Already in the Inaugural Dissertation Kant argues that mathematics is a science of sensory things, that is, mere appearances, because space and time are and must be given relational wholes for mathematical practice. (We have seen that that is right, at least for the notion of space in early modern mathematical practice, in Descartes' geometry. It is also true of the notions of space and time in Newton's physics, of what he thinks of as absolute space and absolute time.) As given relational wholes, space and time belong to sensibility as contrasted with spontaneity. The pure concepts are not in the same way given because they are the conditions of the possibility of any inquiry at all. As such they belong to the spontaneity, the self-activity, of thought and are the ground of the objectivity of our judgments, just as Kant argues in the Deduction. That is, the mere fact that our thought, and indeed the thought of any finite being, is conceptual, through concepts, *does not show* that some form of transcendental idealism is true. Although an infinite intelligence grasps objects directly in a spontaneous intuition and we finite beings grasp objects only through concepts, hence only partially (as this or that kind of thing), we could nonetheless get things right regarding those objects. Of course, we know that it is wrong to think that things as they are in themselves are spatiotemporal. That our experience is spatiotemporal is sufficient reason to hold that transcendental idealism is true, that things

[48] There is, then, for Kant no Given (in the sense that Sellars (1956) argues is a myth), nothing not in conceptual shape that might nonetheless have normative significance. But, Kant thinks, there nevertheless must be something given from outside spontaneity that as matter can be conceptually articulated through an act of transcendental synthesis.

in themselves must be distinguished from things as they appear to us. That our experience is essentially conceptual is, again, not such a reason, and we see that it is not when we recognize that it is constitutive of the conceptual to involve self-correction. We do not merely experience reality through concepts; we also, and inherently as the rational beings we are, have the capacity to acquire new concepts as reason sees fit. The spontaneity of judgment, which is the expressive freedom of a finite and contingent rational being, is *completed* by objective reality; judgment as the exercise of such freedom is an act that, when successful, realizes a cognitive relation of knowing between the (finite) thinker and something objective.

Judgment as an exercise of spontaneity is an act of expressive freedom that is completed by what is actually the case; it answers to what is, however one takes it to be. But (again) for Kant, judgment is possible only if an object is given to it from outside spontaneity, through receptivity. Understanding is a *power* of judging only if an object is given. The cognition of a finite being needs a given, something that is not, as such, in conceptual shape. This does not make any sense, because there can be no given for thought. And yet it must be so. Kant sees both the necessity and the impossibility. As he explains in the *Metaphysical Foundations*, just as "Newton's system of universal gravitation stands firm, even though it involves the difficulty that one cannot explain how attraction at a distance is possible," so Kant's claim that the categories have no application except in relation to objects of possible experience is to stand firm even though it is impossible to explain how the categories make experience possible: "*difficulties are not doubts*" (Kant 1786, 189; AK 4:474).

But Kant's difficulties are not merely difficulties. In fact, Kant's transcendental idealism fails to give any account at all of how knowledge is possible. McDowell (2009, 78) puts the problem this way:

> If there are conditions for it to be knowable by us how things are, it should be a truism that things are knowable by us only in so far as they conform to those conditions. And Kant wants it to seem that if we hanker after an objectivity that goes beyond pertaining to things as they are given to our senses, we are hankering after something that would violate that truism. But it is equally truistic that a condition for things to be knowable by us must be a condition for a possibility of our *knowing* how things are. And if some putative general form for states of affairs is represented as a mere reflection of a fact about us, as the spatial and temporal order of the world we experience is by transcendental idealism, that makes it impossible to see the relevant fact about us as grounding a condition for our *knowing* any instance of that form. Transcendental idealism ensures that Kant cannot succeed in depicting the way our sensibility is formed as the source of a condition for things to be *knowable* by us.

But if the above account is right, the problems go deeper yet. They lie in the very idea that something, an object, must be given for thought. That the spontaneity of the understanding is constitutively self-correcting, able to call anything into question as reason sees fit, must be *the whole story*. The problem is to see how it can be without falling back into an empty coherentism.[49]

4.7 Conclusion

In everyday sensory experience we seem to take in how things are; things have sensory, that is, perceptible, properties and they have characteristic behaviors expressive of the sorts of beings they are that we discover by observing what they do. Awareness of the sort we enjoy furthermore appears to be a biological endowment, one that we share with (some) other animals. What distinguishes us is only our capacity for reflection and second thoughts. This natural attitude comes, with the rise of modern science that is made possible by Descartes' new form of mathematical practice, to seem merely naïve. One cannot simply read off of experience how things are, merely put things into words. Instead one must think, reflect on how, given all the appearances, things must really be. It is the essentially active power of judgment, taking nothing for granted as given, that first makes manifest the intelligible, law-governed structure of reality. The modern subject is in this way autonomous, not only self-conscious but self-consciously self-governed. But although it is Descartes who first realizes the shape of spirit that is (early) modernity, it is Kant who gives it voice, who sees that this new mode of intentional directedness on reality requires the philosopher to make a Copernican turn, to focus not unthinkingly on the object of knowing but self-consciously on the power of knowing, on what reason requires of objects as objects of knowledge. And the individual self-conscious thinker can know this through its own spontaneity, or self-activity, that is, a priori; for these laws belong to spontaneity as the laws of its own self-activity as a knower.

Kant's conception of intuitions and concepts, we have seen, is a successor of sorts to Descartes' distinction between the confused and obscure representations of sense experience and clear and distinct ideas, for instance, of mathematics. What is new with Kant is his characterization of how they differ and the idea that both are involved in any cognition. Cognition is, for Kant, at once conceptual and of an object given through the receptivity of sense. Indeed, we saw, it is this thought that requires that concepts and intuitions be distinguished not only logically, as general, through marks, and singular, immediately of objects, but also metaphysically, as belonging to the spontaneity of the understanding and to the receptivity of sensibility, and

[49] In *Mind and World*, McDowell describes the givens of everyday experience as innocuous. And so they are. But one can adequately understand how they can be only if one understands as well that thought does not inevitably need such a given. We can take everyday experience in our stride, as McDowell urges us to do, but only after we have understood how by reason alone we can achieve knowledge, only after we have understood how reason, pure reason, can be a power of knowing. And we can understand how reason can be a power of knowing only by seeing how that power is actualized through the course of our intellectual history, most immediately in the radical transformations in mathematical practice over the past twenty-five hundred years that we are concerned to trace here.

ontologically, as rules of unity and sensory representations that are the matter for synthesis. The involvement in cognition of concepts and intuitions so understood reveals in turn that cognition has both an active and a passive aspect. And just as the passive aspect enables an understanding of mathematical inquiry, so, Kant thinks, the active aspect can ground a new science, transcendental logic, the science of truth within which are revealed the conditions of possibility of knowing. Kant's table of judgments, I argued, sets out just those forms of judgment that are required for the critical reflection that is involved in our capacity for self-correction. This capacity for self-correction, rather than any imagined foundation, is constitutive of the possibility of knowing.

Judgment in Kant is neither merely unreflective affirmation or denial (holding together or holding apart) as Aristotle thought, nor an act of will as Descartes suggests. It is not a productive act at all but instead an expressive one, an act of the self-activity of the spontaneity of the understanding, an act that, when successful—like any power, the power of knowing that the understanding possesses on Kant's account is inherently fallible—is the realization, through an essentially self-conscious process of inquiry, of what is the case. The expressive freedom of the spontaneity of the understanding is not the freedom to make something out of something but instead the freedom to make manifest what is otherwise hidden or latent; it is the power of thought to be revelatory of things as they are. And because it is, our contingency and finitude, the fact that we are confronted with a world that is not of our making, is not a limitation on expressive freedom but instead constitutive of it. This is Kant's profoundest insight.

Kant *saw* that judgment is an act of expressive freedom that is completed by the objectivity of the reality we seek to know. But this, unfortunately, is properly intelligible on Kant's account only if we distinguish the transcendental perspective from the empirical, recognize that although judgment is revelatory of reality, that reality is only empirically real. Because, for Kant, objects must be given to the receptivity of sense—because pure reason is not, for him, a power of knowing—Kant's account of judgment as revelatory of things as they are is intelligible only if there is posited also a transcendental synthesis, an act of productive freedom that is a making of something (our experience of reality) out of something (given sensory matter). Transcendentally, the world we seek to know is not real but ideal. Although Descartes had claimed to have achieved in his new form of mathematical practice the capacity to think, independent of any images and so of the senses generally, the relations that are the concern of *mathesis universalis*, we have no such capacity. All awareness is, in the final analysis, of objects given in receptivity as thought through concepts.

But if all our awareness is of given objects thought through concepts then Kant's logic of truth can only be a *transcendental* logic within which we discover what our concept of an object must be, and hence what the object itself must be, that we can know it. This is and must be so because the *power* of judgment as Kant understands it is intelligible not merely insofar as there is *something* to which it is answerable, something independent of our thinking and judging; it is intelligible insofar as it is answerable to objects in particular, and hence to something given in sensibility as contrasted with what is thought through concepts of the understanding. In the end, then, Kant cannot provide what is wanted, a cogent account of truth and knowledge in the exact sciences.

But the problem does not lie only with Kant. Kant has the problems he has, at least in part, because the practice of the mathematics and physics of his day are as they are. Not only does Descartes' mathematical practice need space as an antecedently given whole, it seems in no way constitutively to involve capacities of self-correction (as Kant explicitly saw), and insofar as it does not involve capacities of self-correction it cannot, by Kant's own lights, answer to anything outside itself. Instead, the given foundation that is provided for mathematics by the forms of sensibility provides it with content; absent that foundation mathematics would be nothing more than a mere game with signs. The practice of Newtonian science, by contrast, does constitutively involve self-correction. Nevertheless, even it must take something as given, namely, the data that its theories are to explain. By what right, then, do we claim that the fruits of these sciences reveal things as they are as contrasted with how they appear to us? Even if we bracket Kant's own difficulties, we cannot (yet) understand the striving for truth in mathematics and the exact sciences. If progress is to be made, a second revolution is needed, first and foremost in mathematics. It is to that revolution that we now turn.

5

Mathematics Transformed, Again

On Kant's account the practice of mathematics involves an intuitive use of reason as contrasted with the discursive use of reason that Kant associates with the practice of philosophy. In mathematics one does not reason directly from concepts; instead one discovers new truths through the construction of concepts in pure intuition, that is, by paper-and-pencil reasoning. It follows, Kant thinks, that the practice of mathematics is not, as philosophy and physics are, inherently fallible and constitutively self-correcting; because the constructive activity of mathematics puts the truth before one's eyes, he thinks that one can achieve absolute certainty in mathematics. It was in many ways an extremely plausible and cogent account of truth and knowledge in mathematics—at least in the mathematics of Kant's day. But already by the beginning of the nineteenth century the practice of mathematics had begun to change, and over the course of that century it was utterly transformed. Kant, it came to seem, was simply wrong about the fundamental nature of mathematical practice—but how exactly?

According to a very familiar account due originally to Russell and defended more recently by Friedman (1992), Kant needed to suppose that mathematics involves constructions because he lacked the logical resources that are needed in the course of mathematical reasoning. Russell writes in *The Principles of Mathematics* (1903, sec. 4):

> There was, until very lately, a special difficulty in the principles of mathematics. It seemed plain that mathematics consists of deductions, and yet the orthodox accounts of deduction were largely or wholly inapplicable to existing mathematics. Not only the Aristotelian syllogistic theory, but also modern doctrines of Symbolic Logic, were either theoretically inadequate to mathematical reasoning, or at any rate required such artificial forms of statement that they could not be practically applied. In this fact lay the strength of the Kantian view, which asserted that mathematical reasoning is not strictly formal, but always uses intuitions, i.e., the *a priori* knowledge of space and time. Thanks to the progress of Symbolic Logic, especially as treated by Professor Peano, this part of the Kantian philosophy is now capable of a final and irrevocable refutation.

The modern logic of relations, which was developed by Russell following Peano and Peirce in the fall of 1900, enables one to dispense with constructions and instead to infer, for example, the existence of a point between two other points in a dense ordering R: $(\forall x)(\forall y)(\mathrm{R}xy \supset (\exists z)(\mathrm{R}xz \;\&\; \mathrm{R}zy))$.[1] But in fact Kant's philosophy of

[1] See Friedman (1992, ch. 1) for further discussion.

mathematics had been decisively overturned long before that logic was developed. It was overturned not by developments in logic around the turn of the twentieth century but by developments in mathematics over the course of the nineteenth century beginning with Bolzano's purely conceptual proof of the intermediate value theorem in 1817, thirty years before Boole's *Mathematical Analysis of Logic* would set in train the investigations that would culminate in our mathematical logic. Although Kant's idea that mathematics is founded on pure intuition would continue to have its adherents throughout the nineteenth century, and many nineteenth-century mathematicians would continue to work productively in the constructive, computational, and algebraic style of eighteenth-century mathematicians, as they do still today, other mathematicians began to do mathematics in an essentially new way. Our concern is to understand some of the characteristic features of this new form of mathematical practice, and to gain a sense of the profound philosophical problems it poses.

We distinguished in Chapter 2 between reasoning *in* a system of signs, as, for instance, one does in performing a calculation in the positional system of Arabic numeration, and reasoning *on* collections of signs, as one does in (say) solving an arithmetical problem by manipulating the signs of Roman numeration. Kant's view is that mathematics is distinctive as a form of inquiry precisely because and insofar as one reasons *in* systems of signs, whether in diagrams, in Arabic numeration, or in the symbolic language of algebra and analysis. And nineteenth-century constructivists such as Kronecker remained committed to just such a view. Others such as Weierstrass began to take a more conceptual approach that involved a kind of reasoning *on*, for instance, representations of power series. Abel's proof of the insolubility of the quintic, we will see, is essentially similar insofar as it involves an ineliminable appeal to certain forms of expression though not the construction of the solution *in* the system of signs. Other work was more radical yet in eschewing any and all reference to forms of expression, to representations. This wholly purified mathematics, which is most closely associated with the work of Riemann, and with that of Dedekind following him, involves reasoning of course; what it does not involve is anything intuitive, *any* reference to any marks or systems of marks. It is, as we might say, completely, or purely, conceptual.[2] As we will see, Galois' proof of the insolubility of the quintic contrasts with Abel's proof in exactly this regard. Whereas Abel's proof

[2] Ferreirós (1999, 24–38) similarly describes both Weierstrass's work and that of Riemann and Dedekind as conceptual, but also distinguishes them by describing Weierstrass's work as "formal conceptual," because it still uses "forms," that is algebraic equations, and describing that of Riemann and Dedekind as instead "abstract conceptual" because it avoids any and all reliance on forms of expression. Because "abstract" is often used to mean the same thing as "conceptual," it seems preferable to describe it instead as *purely* conceptual. Compare Gray's (2009, 665) remark that in the nineteenth century mathematics "went from being abstract one way to being abstract in a new way, albeit a deeper, more significant way." A more nuanced characterization of the difference between the purely conceptual approach we find in Riemann's work and earlier work is attempted in section 5.3 below.

ineliminably involves reflection on various forms of expression, Galois' proof appeals instead to fundamental concepts of group theory.[3]

Kant explains the synthetic a priori character of mathematical knowledge, the fact that it is necessary but also knowledge properly speaking, by appeal to the idea of constructions in pure intuition. In mathematics as it came to be practiced in the nineteenth century, not only pure intuition but all appeals to constructions, to written formulae as much as to diagrams, were to be eschewed. This new mathematical practice was to be purely conceptual, a matter of thinking in concepts, *Denken in Begriffen*. But, we will see, it was *not* taken to be also merely formal, empty of content and truth. For Riemann, Dedekind, and even Hilbert, this form of mathematics is a *science*, an inquiry into truth in its own proper domain. The truths that are the fruits of this new mathematics are, in other words, synthetic a priori in something like Kant's sense: they are necessary truths that extend our knowledge despite being proved from concepts by logic alone. Or so, we will see, they seem to have been understood by nineteenth-century practitioners of this new style of mathematics.

5.1 Intuition Banished

We saw that space and time, as pure forms of sensibility, play two essentially different roles in Kant's philosophy of mathematics. They provide a given foundation for the mathematician's reasoning in the form of a priori intuitions of various fundamental truths (for instance, that space has three, and only three, dimensions, and that a line whose endpoints are on opposite sides of a given line must somewhere cross the given line). And they are the conditions of possibility of the constructive activity that is reasoning in mathematics. Paper-and-pencil reasoning is a sensory-motor activity, the work of an embodied, temporal thinker. Because it is, space and time are presupposed by the very nature of mathematical investigation, at least as Kant understood it. Both roles would be called into question by the purified mathematical practice that emerged in the nineteenth century. We begin with the first of them, and in particular, with the idea that mathematics relies on a priori intuitions about Euclidean space.

The most oft-cited reason for rejecting the idea that we have an a priori or pure intuition of space is the nineteenth-century development, or discovery, of non-Euclidean geometries in which the parallels postulate does not hold.[4] In hyperbolic

[3] Because this new form of mathematical practice does not rely on any system of written marks but is instead purely conceptual, and deductive, it seemed, at least at first, to be not distinctively *mathematical* at all. (This was in large part what was behind Frege's, and also Dedekind's, logicism.) As a Cambridge don is said once to have quipped, "and now we can solve the problem without any mathematics at all, just group theory."

[4] The parallels postulate had, since Euclid, been regarded with some suspicion. The worry was not that it was false but that it seemed too complicated to be a basic postulate. Various attempts were made to prove it from the other four postulates, or from the other four plus something simpler, to no avail.

geometry, the parallels postulate is replaced with a postulate according to which for any line and point not on that line there are infinitely many lines (rather than only one as in Euclid) that pass through the point and do not intersect the given line no matter how far the lines are extended. In this geometry the sum of the angles of a triangle are not equal to but less than two right angles. In elliptical geometry, every line that passes through the given point also intersects the given line if extended far enough. Hyperbolic geometry was developed and introduced in the 1830s by Bolyai and (independently) by Lobachevski, and perhaps even earlier by Gauss, though he did not publish his findings. Riemann in his Habilitation lecture of 1854 introduced elliptical geometry. As these works show, there is nothing incoherent in replacing the parallels postulate with another postulate inconsistent with it. But if so then the question of the nature of empirical space would seem to need to be separated from the mathematical exploration of these different geometries. Significantly, Frege drew a different conclusion. According to him, Euclidean geometry was, as it was for Kant, the true geometry, and the fact that other logically consistent "geometries" could be formulated did nothing to change that; non-Euclidean geometries were not real mathematics. Frege was, as it turns out, wrong about this. Non-Euclidean geometries have proved to be mathematically significant, and they also play an important role in the physical sciences. (Empirical space as described in Einstein's general relativity is not Euclidean but elliptical, and indeed, of variable curvature, a possibility first explored by Riemann.) Nevertheless, Frege's response to the development of non-Euclidean geometries is instructive insofar as it shows that those developments did not alone suffice to show that Kant was mistaken about the role of pure intuition in geometry. Gauss's avowed reason for not publishing his work in hyperbolic geometry makes the same point: he wished to avoid "the clamor of the Boeotians," that is, Kantian objections of essentially the sort that Frege had.[5]

Both hyperbolic and elliptical geometries could, at least at first, be understood to be mere curiosities, abstract possibilities without any mathematical reality. The same could not be said of developments in projective geometry beginning with the work of Poncelet in the 1820s. The extended space of projective geometry with its imaginary points and lines alongside the more traditional ones seemed *clearly* to be good mathematics, indeed to be a natural, even organic development of Euclidean geometry.[6] But it could be in no way reconciled with our supposed a priori intuitions about space.

Projective geometry studies non-quantitative or non-metrical geometric relations, and its development was motivated, at least in part, by limitations of traditional

[5] Gray (2004, 32) sees something deeper at work in Gauss's decision not to pursue his work on hyperbolic geometry: "something about the new geometry prevented him from pulling his ideas together and investigating them properly as undoubtedly he could have done. The new geometry was disquieting to him, as it was to those whose strong instincts were to reject it once it had been discovered."

[6] See Wilson (1995), to which I am indebted for much of the discussion to follow.

geometry relative to the analytical methods championed by Descartes. Suppose, for example, that we are considering a circle and a coplanar line that cuts it. In analytic geometry one first formulates an equation for the circle and an equation for the line, which requires in turn selecting two further principal lines to serve as the basis of the coordinates, and then one puts equals for equals to find the intersection points. What is interesting about this analytical method is that one can find "points of intersection" even in cases in which ordinary visual inspection of the circle and line in Cartesian coordinates suggests that they do not intersect at all. Admitting such imaginary points of intersection can furthermore unify what appear in traditional geometry to be fundamentally different sorts of cases. What Poncelet saw is that this power that algebra possesses is due to the power of its symbolism, which, at least on paper, allows one to subtract a larger number from a smaller as easily as one subtracts a smaller from a larger, and to form an expression for a square root of a negative number *and* to calculate with it. Synthetic geometry, Poncelet argued, can enjoy the same power provided that the diagram is regarded in a new, less concrete and literal way. He proposed, in particular, "to regard every actual diagram in geometry not as the *subject matter* of geometrical study, but as a complex *sign* whose components were to be operated upon in certain ways without demanding an interpretation for them in terms of anything visualizable" (Nagel 1939, 204).

Consider again a line that intersects a coplanar circle, and imagine now the line being moved in the plane until it no longer appears to intersect the circle; nonetheless, Poncelet argues, it does intersect the circle at certain "hidden" points.[7] And as Wilson (1995, 114) explains,

> rather than thinking of the extra points as "conveniences," the projective geometers saw the additions as revealing the "true world" in which geometrical figures live. Familiar figures such as circles and spheres have parts that extend into the unseen portions of six dimensional complex space, so that when we see a Euclidean circle, we perceive only a portion of the full figure (nineteenth century geometers liked to claim that we see the full shape of geometrical figures in the manner of the shadow's in Plato's cave).

But if that is right then geometry simply cannot be limited to what can be intuited as Kant's philosophy of mathematics requires. Projective geometry was good mathematics and a natural development in geometry; if it offended against Kant then so much the worse for Kant.

The phenomenon of duality in projective geometry further undermines the foundational role of Kantian intuition in mathematical practice insofar as it shows that although it is natural to think of space as grounded in points, one can as easily (at

[7] In his *Traité des proprietés projectives de figures*, Poncelet justifies this domain extension by appeal to the principle of continuity: "if we suppose a given figure to change its position by having its points undergo a continuous motion without violating the conditions initially assumed to hold between them, the... properties which hold for the first position of the figure still hold in a generalized form for all the derived figures" (quoted in Nagel 1939, 204).

least mathematically) take very different geometrical entities to be the primitive elements of geometry. Plücker, for example, argued that although we first understand an equation such as '$ux + vy + 1 = 0$' as involving the constants u and v with variable coordinates x and y, we can equally well read it with the roles of the two sorts of letters reversed, that is, with x and y presumed fixed and u and v variable. Interpreted the first, usual way, the equation specifies a line as a locus of points: with u and v assumed fixed, the equation determines the points that lie on a given line. But if we take instead x and y, the coordinates of some point not further specified, to be fixed, and u and v as variable, then the equation determines instead the locus of a given set of lines, that is, a point. Although we can take points as basic and understand a line as a locus of points, we can as easily, again, mathematically if not intuitively, take lines to be basic and understand a point as a locus of lines. Mathematically, then, neither points nor lines seem intrinsically more basic in this geometry. And the point generalizes. It applies also, for instance, to the equation '$ax^2 + bxy + cy^2 + dx + ey + 1 = 0$'. Taking the letters '$x$' and '$y$' to function as variables, we understand points to be basic and the conic as a locus of points; but we can instead take the letters 'a', 'b', 'c', 'd', and 'e' to function as variables in which case it is instead the conic that is basic.

As these considerations show, dimensionality is a function of how a plane or other space is regarded. The plane is, for example, "two dimensional with respect to points and also with respect to lines; but it is five-dimensional with respect to conics, since five independent coordinates are required to specify completely a conic section. In general the dimension of a manifold is relative to the choice of configuration as element" (Nagel 1939, 232). It follows directly that the principle of duality, first discovered for points and lines and points and planes, applies much more generally. Gergonne had shown already in 1826 that "to each theorem in [projective] plane geometry there necessarily corresponds another, deduced from it by simply interchanging the two words 'points' and 'lines'; while in solid geometry the words 'points' and 'planes' must be interchanged in order to deduce the correlative of a given theorem" (quoted in Nagel 1939, 224). What Plücker shows is that this duality holds of any sort of equation in projective geometry: "when an analytic proof of a theorem is once given, as many different theorems are proved at the same time as there are different systems of coordinates" (quoted in Nagel 1939, 233).[8] Because all have equal status mathematically, the fact that some are intuitively more basic than others seems simply irrelevant, an accidental fact about us that has no mathematical significance:

[8] As we will see in Chapter Seven, Frege's concept-script displays a feature that is very like this new way of understanding equations. Although each primitive sign of the language expresses a particular sense, the question what it designates can be answered only in the context of a proposition and relative to a particular function/argument analysis. Much as the two sorts of letters in an equation can be taken to play either of two sorts of roles in projective geometry, so different sorts of signs in *Begriffsschrift* can be taken now to mark an argument place and now to mark the function that is applied to an argument.

it is an accidental feature of the way we happen to learn about Euclidean geometry that some of its parts appear to us clothed in intuitive garb and some do not. As a result we originally detect the presence of some ideal elements only through the rigid relationships they induce upon the visible objects. But another class of beings might directly intuit the imaginaries, rather than the reals. (Wilson 1995, 128)

The study of projective geometry was first and foremost a mathematical advance, albeit one with profound, if obscure, philosophical significance. In the work of Bolzano, it seems to have been the philosophy that came first. Born in 1781, the year Kant's first *Critique* appeared, Bolzano published in 1810 *The Contributions to a Better Founded Exposition of Mathematics* in which he argues that the very idea of a pure or a priori intuition is incoherent and hence that all appeals to it must be banished from mathematics; mathematical truths are and must be grounded in concepts, that is, proven deductively from previously defined concepts. In 1817 Bolzano illustrated this new view of mathematical truth with his proof of the intermediate-value theorem: he defines the concepts of continuity and convergence, and then proves on that basis that a continuous real function taking values above and below zero must also take a zero value in between. As Coffa (1982, 686) remarks,

a Kantian would probably regard this as trivial: if the point whose path we are considering is moving from the negative to the positive quadrant and the path is continuous it must surely intersect the x axis at some point. Bolzano's problem looks like a problem only to someone who has already understood that intuition is not an indispensible aid to mathematical knowledge, but rather a cancer that has to be extirpated in order to make mathematical progress possible. . . . If Kant had known about Bolzano's paper there can be little doubt that he would have regarded it as a philosophically incoherent effort to prove the obvious. The paper was, instead, one of the landmarks of nineteenth-century mathematics.

It is furthermore worth emphasizing, as Coffa (1982, 683) does, that although Bolzano rejects Kant's notion of pure intuition as incoherent, he continues to think (with Kant) that mathematical knowledge is synthetic a priori, despite its being purely conceptual. Where Kant had gone wrong, on Bolzano's account, was in thinking that only analytic judgments are possible on the basis of concepts; strictly deductive proofs of theorems on the basis of concepts alone, Bolzano thought, *can* be ampliative, real extensions of our knowledge.

Although Bolzano seems to have been philosophically motivated to ground his mathematics solely in concepts, there were, in addition to those provided by projective geometry that we have already considered, familiar reasons internal to mathematics for such a move, reasons having to do with attempts to understand infinitary notions and methods in the higher analysis of Leibniz and Newton and in Euler's theory of sequences. Berkeley had raised philosophical concerns already in 1734, but they had had little effect on the practice of mathematics. Around 1800, Coffa (1982, 685) suggests, "mathematicians themselves started to talk about meaning, to try to figure out what exactly was the meaning of continuity, differentiability, infinitesimal, function, and so on." And they did so, Grabiner (1974, 360) argues, because of "the mathematician's need to teach." Although in the eighteenth century mathematicians were generally attached to royal courts—"their job was to do mathematics and thus add to the glory, or edification, of their patron"—in the nineteenth century mathematicians were increasingly called on to teach undergraduates following the model of the *École Polytechnique*, which was founded in 1795. No longer could one rely on the native abilities of mathematicians to be understood: "A mathematician could understand enough about a concept to use it, and could rely on the insight he had gained through his experience. But this does not work with freshmen, even in the eighteenth century" (Grabiner 1974, 360). As Grabiner notes, Lagrange, Cauchy, Weierstrass, and Dedekind all first did their foundational work in analysis in their lecture courses.

In the case of analysis it was clear what the rules governing the transformations of equations were, that is, what the derivatives of various functions were. It was known that, for instance, the derivative of x^n is nx^{n-1}. The problem was that there seemed no cogent justification for those rules, hence no real understanding of the nature of limit operations. In particular, it was not at all clear what exactly it *means* to say of a difference quotient, $[f(x + \delta x) - f(x)]/\delta x$, that it gives the derivative of a function "at the limit as δx goes to zero." In the case of infinite series it was not even clear what the rules were. Using the rules of analysis it can be shown, for example, that $1/(1 - x) = 1 + x + x^2 + x^3 + \ldots + x^n + \ldots$ from which it follows, for instance, that $1/(1 - 1/2) = 2 = 1 + 1/2 + 1/4 + 1/8 + \ldots$ Euler held that it also follows that $1/(1 - (-1)) = 1/2 = 1 - 1 + 1 - 1 + 1 \ldots$, that $1/(1 - 1) = 1/0 = \infty = 1 + 1 + 1 + 1 \ldots$, and that $1/(1 - 2) = -1 = 1 + 2 + 4 + 8 + \ldots$, from which he concluded that there must be negative numbers equal to infinity. Others were appalled.

In the case of higher analysis, what is wanted is an account of why the familiar laws (such as that the derivative of x^n is nx^{n-1}) hold, which requires, again, an adequate understanding of the difference quotient at the limit as δx goes to zero, that is, at the limit of an infinite process of approach to zero. But precisely because this process must be conceived as an infinite one (otherwise the difference quotient is meaningless), because it involves an *infinite* process of approach, the limit simply cannot be constructed, either computationally as Leibniz aimed to do or by appeal to the motion of a point over time as on Newton's alternative account. What is needed instead, as the work of Bolzano, Cauchy, and Weierstrass shows, is a *limit-avoiding* strategy.[9] The task must instead be conceived as that of proving of some function that it is the function that is wanted: if, for an increment that is as small as you like, the difference between the difference quotient of a function $f(x)$ and some function $g(x)$ is small then $g(x)$ is the derivative of $f(x)$. The task is not to *construct* the limit but instead to *describe*

[9] This is not to say that there were not differences in the mathematical styles of the three. For instance, although Bolzano focused on the analysis of concepts and deductive reasoning, Weierstrass, together with his colleagues in Berlin, continued to work primarily in the more traditional computational style.

the constraints the needed function must satisfy, to provide a conceptual rather than an intuitive (in Kant's constructive sense) solution to the problem. Whereas on the traditional conception of it, a smooth curve is very like a string of very small straight lines, in effect, the limit of endlessly dividing those straight lines into smaller and smaller lengths, on the conception that these mathematicians achieved a smooth curve is not understood on the model of a straight line but is something essentially new. Instead of thinking of a curve as a collection of (very, very small) straight lines strung end-to-end, we are to think instead of a straight line as an exceedingly simple smooth curve, one whose derivative is a constant.

The case of finite and infinite sums is essentially similar. Consider first Zeno who thinks of an infinite sum on the model of a finite sum, and on that basis generates the paradox of Achilles and the Tortoise. We suppose that Achilles can run ten times faster than the Tortoise and that the racecourse is one hundred meters long. The Tortoise is given a ten-meter head start, and the race begins. When Achilles reaches the 10–meter mark, the Tortoise has gone one meter and so is at the 11–meter mark. When Achilles reaches the 11–meter mark, the Tortoise is 11.1 meters from the starting-point. When Achilles reaches 11.1 meters, the Tortoise is at the 11.11–meter mark. And so it goes. Achilles, it seems, can never quite catch up with the Tortoise; at each point, the Tortoise is ahead, albeit by ever smaller increments. Just as in the case of the derivative of a smooth curve, we have an endless process and thereby an end that can never quite be reached by a constructive, algebraic approach. And as in our earlier case, the crucial insight is that the infinite cannot be understood on the model of the finite. An infinite sum is not merely a very long finite sum but instead something essentially new. As an examination of some cases reveals, to understand it one needs to attend not merely to the successive sums but to the *pattern* displayed by those sums taken as a whole.

We begin with the harmonic series: $1 + 1/2 + 1/3 + 1/4 + \ldots + 1/n \ldots$ In such a series, each term is smaller than the one before it; and because it is, one might think, the series should converge at the limit to some finite number. This is demonstrably not the case. Because the third and fourth terms, that is, 1/3 and 1/4, are both equal to or greater than 1/4, it follows that their sum is equal to or greater than one-half. Similarly, the next four terms, 1/5, 1/6, 1/7, and 1/8, are all equal to or greater than 1/8; so their sum is also equal to or greater than one-half. The sum of the next eight terms after that is likewise equal to or greater than one-half, as is the sum of the next sixteen terms after that, the next thirty-two after that, and so on endlessly. By taking ever longer but always finite collections of terms, first two terms, then four, eight, sixteen, and so on, by seeing this *pattern* in the series overall, one sees also that one can conceive the series as involving the successive addition of numbers all equal to or greater than one-half. Because the series is infinite, the sum of the series must be indefinitely large. The harmonic series does not converge.

Compare now this series: $1/2 + 1/4 + 1/8 + \ldots + 1/2^n + \ldots$, which is the basis for another of Zeno's paradoxes because it seems to show that it is impossible to walk

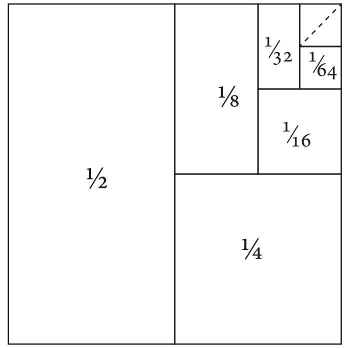


Figure 5.1 Picture proof to show that the series 1/2, 1/4, 1/8,... $1/2^n$,... converges.

across a room: although one can get indefinitely close to the endpoint, by going half the remaining distance at each stage, one can never quite get to the endpoint. In fact, this series does converge to a finite number, namely, one, as the picture proof in Figure 5.1 illustrates.

Because, at each stage, with each successive sum, the square comes closer to being completely filled, in the limit it is filled: the series converges to one. And the reason it does is that this series has a property not shared by the harmonic series, namely, that of getting closer, with each successive sum, to some particular number so that no matter what point in the series is chosen, there is another after which all subsequent sums are closer yet to the limit. Any infinite sum having this property converges to a limit. In particular, that of Achilles in his race with the Tortoise does:

$$10 + 1 + 1/10 + 1/100 + \ldots + 1/10^n + \ldots = 11.1111\ldots = 11\ 1/9.$$

Achilles will catch up with the Tortoise at the eleven and one ninth meter mark. Here again, then, the task is not to construct the limit—that is, actually to produce it using the familiar rules of arithmetic and basic algebra—but to *describe* the property an infinite sum must have if it is to converge to a limit. To understand the infinite, at least in these cases, requires a *conceptual* approach as contrasted with a computational one.

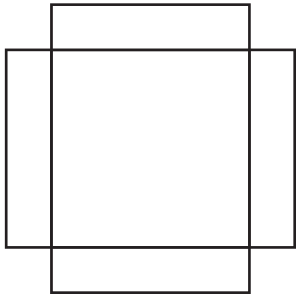
According to Kant, our pure intuitions of space and time furnish truths that form the basis of arithmetic and geometry. Were he right, Euclidean geometry would be the one true geometry; hyperbolic and elliptical geometries, which are founded on the supposition that the parallels postulate is false, would be a mere play of ideas without any foundation in truth. But other advances in mathematics such as the development of projective geometry, the rigorization of the calculus, and the clarification of the nature of convergence, seemed to show that Kant was not right. Mathematics does not need a foundation in pure intuition and in many cases is hampered, even positively misled, by appeals to our intuitions of space and time. But some of this work, for example, that with infinite series and that in projective geometry, seemed to suggest that Kant was nonetheless right about the importance of systems of written marks in mathematics; although one was perhaps no longer reasoning *in* such systems of signs but only reasoning *on* expressions made possible by them (for instance, the beginning, finite parts of infinite sequences), still the expressions were needed. The next step was to see that this new form of mathematical practice relies not at all on any form of representation. In the work of Galois, Riemann, Dedekind, and Hilbert, the demand for a new, *wholly conceptual* approach, first heralded by Bolzano, would profoundly and permanently transform the whole of mathematics, indeed, the very idea of what it is to do mathematics.

5.2 Constructions Banished

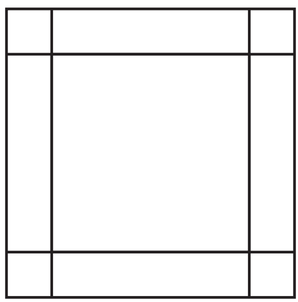
Much as problems that are demonstrated diagrammatically in Euclid can often also be solved algebraically, so problems that have algebraic solutions can often also be resolved using a more conceptual approach. And in both cases, the latter solution can seem, relative to the former, to get to the heart of the matter, to reveal what is really going on mathematically. Insofar as it does, the latter methodology is to be preferred. It was in just this way that, over the course of the nineteenth century, it came increasingly to seem that the constructive methods of eighteenth-century mathematics, even any use at all of systems of written marks, were masking the true nature of various parts of mathematics. This new style of *purely* conceptual mathematics is, again, most closely associated with Riemann and Dedekind following him, but we see it already at work in Galois' proof of the insolubility of the quintic as it contrasts with Abel's proof of the same result. And as Riemann and Dedekind were, Galois is quite explicit in his demand for a conceptual rather than a computational approach, one that wholly eschews the need for any system of representations:

> at a time when the perfection of calculational mathematics was triumphant, Galois declared the self-liquidation of calculation as a method. . . . His own position was that, while useful at certain periods, symbolism and masses of formulas are incidental attributes of mathematics. When attributes become ends in themselves and thus a hindrance, then, like wraps, they must be discarded, and mathematics itself must be permitted to come forward. (Wussing 1984, 103)

The problem is to determine whether there is a general solution formula for the roots of quintics, that is, of equations of the form: $x^5 + ax^4 + bx^3 + cx^2 + dx + e = 0$. We begin with the simplest case, that of the quadratic equation, $x^2 + ax = b$. The task is to provide a general solution for x in terms of the coefficients a and b, and as we might expect, the first solutions were geometrical. What is wanted is the length x such that the square on x plus the rectangle with sides a and x together equal b. What is wanted, that is, the area of the following figure, where the inner square has sides of length x and the four outer rectangles have sides x and ¼ a. The inner square x^2 together with the four rectangles, each measuring x by ¼ a, equals b.



To solve the problem, we first "complete the square."



Clearly, then, $(x + 2(a/4))^2 - 4(a/4)^2 = b$. Simplifying and adding $4(a/4)^2$, that is $(a/2)^2$ to both sides gives: $(x + a/2)^2 = b + (a/2)^2$. So $x + a/2 = \sqrt{}(b + (a/2)^2)$, and x itself is equal to $\sqrt{}(b + (a/2)^2) - a/2$. Such reasoning, at first diagrammatic rather than algebraic, led eventually to a completely general solution for the second degree equation $ax^2 + bx + c = 0$: $x = [-b \pm \sqrt{}(b^2 - 4ac)]/2a$. Because this expression involves appeal only to the four basic arithmetical operations (addition, subtraction, multiplication, and division), together with root extraction, and because ${}^n\sqrt{}A$ is known as a radical, we say that quadratic equations are solvable by radicals.

The obvious next question is whether we can find some such equation for x in the case of cubics, that is, in the case of equations of the form $x^3 + ax^2 + bx + c = 0$, and so on generally for any polynomial equation of the form $x^n + a_1x^{n-1} + a_2x^{n-2} + \ldots + a_{n-1}x^{n-(n-1)} + a_n = 0$, where $n > 2$. That is, can we find, for values of $n > 2$, an algebraic expression involving only the four basic operations and radicals (that is, square or higher roots) that gives the possible values for x? It was known by the end of the sixteenth century that the answer is yes for $n = 3$, the cubic, and also for $n = 4$, the quartic. In 1824 Abel proved that there is no general solution for quintics, that is, fifth-degree equations, though in particular cases there may be a solution. In the early 1830s Galois not only gave a different proof of the same result, he clarified what it is that distinguishes the particular quintics that can be solved by radicals from those that cannot. Indeed, Galois' result generalizes: a polynomial equation can be solved by radicals just in case a particular condition is met, namely, that its Galois group is a solvable group.

The task is to express the roots of the quintic as an algebraic function of the coefficients as we did above for the case of the quadratic. Abel's proof to show that this cannot be done is a *reductio* building on work of Lagrange. Lagrange had seen that the algebraic solutions for the cases $n = 3$ and $n = 4$ involve the introduction of a certain type of function of the roots that satisfies an equation of lower degree (the reduced equation), which can be solved algebraically. Its roots then provide the solution of the original equation. As Lagrange also showed, the fact that the reduced equation is of lower degree can be explained by appeal to arbitrary permutations of its roots. Abel extended these results to the case of the quintic. His proof starts with a polynomial of degree n and the assumption that the polynomial is solvable in radicals. It is then shown that any such solution must have a particular algebraic form; that is, if the equation is solvable then the expression for the roots of the equation will display a certain syntactic pattern. For the case of the quadratic ($n = 2$), the solution has the form: $x = \mathrm{p} + \mathrm{R}^{1/2}$, where p is a simple polynomial and R is a function of the coefficients of the original equation. (The expression '$\mathrm{R}^{1/2}$' means the same as '$\sqrt{\mathrm{R}}$'.) For the cubic ($n = 3$), it has the form: $x = \mathrm{p} + \mathrm{R}^{1/3} + \mathrm{p}_2\mathrm{R}^{2/3}$. ('$\mathrm{R}^{1/3}$' is the cube root of R; '$\mathrm{R}^{2/3}$' is the cube root of the square of R.) In the case of the quintic ($n = 5$), the solution, if there is one, will be of the form: $x = \mathrm{p} + \mathrm{R}^{1/5} + \mathrm{p}_2\mathrm{R}^{2/5} + \mathrm{p}_3\mathrm{R}^{3/5} + \mathrm{p}_4\mathrm{R}^{4/5}$. By a series of reductio arguments on cases for n = 5, Abel showed that the assumption that the quintic is solvable algebraically leads to absurdity.[10] What Abel did not show, and given his approach could not show, was just why the quintic cannot in general be solved by radicals.

Abel's argument is algebraic and computational, that is, constructive, despite being a reductio, because and insofar as it proceeds by reflection on the forms of the expressions involved, what the algebraic expression for the roots of the quintic would have to look like. It is conceptual but not purely conceptual. What it shows is that a certain construction is impossible. Galois builds on Abel's work but does so by transforming the problem into a wholly different *sort* of problem. He solves the problem by proving a general theorem about groups that is independent of any

[10] Further discussion of the ideas of the proof can be found in Pesic (2003).

appeal to algebraic representations; his proof is not algebraic but instead deductive from concepts. It is as different from Abel's algebraic solution as Descartes' algebraic solutions are from diagrammatic demonstrations in Euclid's geometry.

Consider, first, the relationship between the coefficients and the roots of a polynomial, the fact that, for (say) the case of the cubic,

$$(x - a)(x - b)(x - c) = 0,$$

the roots of which are a, b, and c, the cubic equation is this:

$$x^3 - (a + b + c)x^2 + (ab + ac + bc)x - abc = 0.$$

Because both addition and multiplication are commutative (that is, $a + b = b + a$ and $a \times b = b \times a$), it is clear that the order of the roots does not matter. Any permutation of them would leave everything the same—as Abel, following Lagrange, had already seen. What Galois focuses on is not the permutations themselves but instead properties of combinations of permutations, in particular, whether they are commutative.

A quadratic equation has two roots, which are subject to only two permutations: the identity or do-nothing permutation (I) and a switch (S). Combining (or, as we will also say, composing) them, that is, making one permutation and then another, which we mark using an asterisk, directly yields the following:

- I * S = S = S * I, that is, doing nothing followed by a switch is the same as just doing a switch, which is the same as doing a switch and then doing nothing;
- S * S = I, making two switches is the same as doing nothing because it takes you back to where you started; and of course,
- I * I = I, doing nothing twice is just doing nothing.

Clearly, then, the *order* in which we make the permutations, say, S then I, or I then S, is irrelevant to the outcome. What Galois saw is that this fact, that the order of pairs of permutations is irrelevant to the result (that is, that composition has, in this case, the property of being commutative, or, as it is sometimes put in this context, that the group is abelian), is the key to the solvability of the quadratic.

The cubic has three roots, call them a, b, and c, which can be permuted in six different ways: the identity (do-nothing) permutation, I, three sorts of switches, S_{ab}, S_{ac}, and S_{bc}, and two rotations R_{abc} and R_{acb}.[11] But again, we are not interested in the permutations as such but instead in combinations of them, and in particular whether the order of combinations matters, whether it is commutative. Rather than listing all the pair-wise combinations as we did above for the case of the quadratic, we display them in a table, Table 5.1.

Notice that the order in which we make our permutations does matter in this case, as is evident from the lack of symmetry along the diagonal. For example, S_{ac} * S_{ab} is

[11] These are just the symmetries of an equilateral triangle, all the ways it can be reflected or rotated without changing its overall appearance.

Table 5.1 The pair-wise combinations of the permutations on three roots.

*	I	R_{abc}	R_{acb}	S_{ab}	S_{ac}	S_{bc}
I	I	R_{abc}	R_{acb}	S_{ab}	S_{ac}	S_{bc}
R_{abc}	R_{abc}	R_{acb}	I	S_{ab}	S_{ab}	S_{ac}
R_{acb}	R_{acb}	I	R_{abc}	S_{ac}	S_{bc}	S_{ab}
S_{ab}	S_{ab}	S_{ac}	S_{bc}	I	R_{abc}	R_{acb}
S_{ac}	S_{ac}	S_{bc}	S_{ab}	R_{acb}	I	R_{abc}
S_{bc}	S_{bc}	S_{ab}	S_{ac}	R_{abc}	R_{acb}	I

the same as R_{acb} but if we reverse the switches, S_{ab} * S_{ac}, the result is the same as we would get with the different rotation R_{abc}. So, this group is not abelian. But a subgroup of this group *is* abelian: if we consider only the rotations together with the identity permutation, that is, the first three columns and rows, we find an invariant sub-group of the whole for which composition is commutative. Because this symmetry exists, the cubic is solvable.

The case of the quartic ($n = 4$) is more complex yet, and one must dig even deeper to find the needed symmetry. The whole group of combinations of all possible rotations and switches is not abelian; that is, the order in which one makes permutations matters. Nor is there any invariant subgroup of permutations of a certain sort, say, all the switches or all the rotations (together with identity), that is abelian. Nevertheless, there is, for this case, an invariant subgroup involving three particular pair-wise switches (plus identity) that is abelian. Hence, the quartic is solvable. In the case of the quintic, Galois shows, there exists no invariant subgroup, save for mere identity, that is abelian. Hence, the quintic is not solvable in radicals.

As Abel did, Galois begins with Lagrange's idea of permuting the roots of an equation. Unlike Abel, he further considers compositions of the various permutations and shows that the permutations of the roots of a polynomial form a group. He then associates with each polynomial equation the group of the permutations of its roots—what is now known as the Galois group of the equation—and shows that various properties of the Galois group properly reflect the properties of the relevant equation. In particular, if the Galois group is solvable (if, that is, there is some subgroup of the Galois group—other than the trivial identity—that is abelian), then the original equation is solvable by radicals. Because the permutation group on five elements is demonstrably not solvable, it follows that the quintic is not solvable in radicals. Galois theory thereby resolves the problem of the solutions by radicals of polynomial equations by showing not only that these are and those are not solvable but also *why* some equations are solvable and others not. It shows that "all important properties of a given equation—irreducibility, solvability in radicals, and in the case of solvability the degree of the required root operations—can be determined *without reference to the equation* from properties of the Galois group" (Bewersdorff 2006, 94; emphasis added).

Riemann, as already noted, similarly championed a conceptual approach in mathematics. "His style was conceptual rather than algorithmic—and to a higher degree than that of any mathematician before him. He never tried to conceal his thought in a thicket of formulas."[12] For Riemann, "proving is no longer a matter of transforming terms in accordance with rules but a process of logical deduction from concepts" (Laugwitz 1999, 34). With Riemann "the shift to a conception of function that wasn't connected to available expressions becomes systematic and principled" (Tappenden 2006, 121). Rather than thinking of a function as an analytical expression—as for instance, Euler, that great eighteenth-century master of algebraic symbol manipulation, had—Riemann focuses on its intrinsic properties. "Riemann would catalogue the singularities of a function (points where it becomes infinite or discontinuous), note certain properties, then prove that *there must exist* a function with these properties without producing an explicit expression" (Tappenden 2006, 121). That is to say, he would *describe* the essential properties of the function, its behavior at crucial points, rather than finding an algebraic expression for it. "He distanced himself from . . . the concept of a variable, and introduced into analysis a new fundamental notion, that of a mapping. Mappings were determined not by expressions but by conceptual properties such as continuity and differentiability" (Laugwitz 1999, 337).[13] Wilson (1995, 151, n. 42) provides this analogy: "build a bathtub and specify the "sources" and "sinks" where that water enters and leaves the tub. These conditions will completely fix how the water will flow in the rest of the tub. For Riemann a "complex function" automatically corresponds to the "flow" induced by its singularities, et al., whether or not there is any formula that everywhere matches such a flow." For Riemann, "the objects of mathematics were no longer formulas but not yet sets. They were concepts" (Laugwitz 1999, 305). In sum, as all these historians and philosophers emphasize, something essentially new is begun with Riemann, as we have put it following Laugwitz, the tradition of *Denken in Begriffen*, thinking in concepts.

Much as Descartes in the seventeenth century had self-consciously aimed to liberate mathematics from the constraints imposed on it by traditional, diagrammatic methods, so Riemann in the nineteenth self-consciously aimed to liberate mathematics from the constraints imposed on it by the language of arithmetic and algebra that Descartes first introduced. Mathematics was to be freed of *all* the constraints that might limit or hinder it, and that meant that it had to be conceptual rather than intuitive: "investigations which, like those conducted here [in Riemann's Habilitation lecture], proceed from general concepts, can serve only to ensure that this work shall

[12] Quoted, from Freudenthal's entry on Riemann in the *Dictionary of Scientific Biography*, in Tappenden (2006, 108).

[13] As Laugwitz immediately goes on, "the autonomy of the mapping concept was short-lived. It was preempted by set theory, which decreed that a mapping was none other than its graph, and thus a set." We will here strenuously resist this preemptive move.

not be hindered by a narrowness of conceptions, and that progress in the knowledge of the connections of things shall not be hampered by traditional prejudices."[14] As Stein (1988, 252) paraphrases, on Riemann's view "*the role of a mathematical theory is to explore conceptual possibilities*—to open up the scientific *logos* in general, in the interest of science in general."

Dedekind similarly championed this new mathematical practice focused on the development of concepts and reasoning from developed concepts. He too adhered to what has come to be known, following Minkowski, as "the other Dirichlet principle": "to conquer problems with a minimum of blind calculation, a maximum of clear seeing thoughts" (quoted in Stein 1988, 241). As Dedekind explains,

> a theory based upon calculation would, as it seems to me, not offer the highest degree of perfection; it is preferable, as in the modern theory of functions, to seek to draw the demonstrations, no longer from calculations, but directly from the characteristic fundamental concepts, and to construct the theory in such a way that it will, on the contrary, be in a position to predict the results of the calculation. (Quoted in Stein 1988, 245.)

For example, in his work on the real numbers, Dedekind does not define real numbers as cuts, that is, as sets or classes, but instead thinks of them, as he writes in a letter to Weber (quoted in Stein 1988, 248), as "something *new* (corresponding to this class), which the mind creates." And he does so at least in part because, as Stein (1988, 248) notes, cuts have properties that numbers do not have.[15] What Dedekind is after is not numbers conceived as objects that one might construct, say, out of other numbers in the manner of Kronecker, but instead an adequate *concept* of the reals, one on the basis of which to prove theorems.

Similarly, Dedekind describes his theory of ideals as "based exclusively on concepts like that of *field*, *integer*, or *ideal*, that can be defined without any particular representation of number."[16] In effect, as Stein (1988, 247) suggests, "it does not matter [to Dedekind] what numbers *are*; what matters is that they constitute a simple infinite system," that is, that they form a system with certain properties. Dedekind's talk of a kind of free creation by the mind is read by Stein, then, as referring, on the one hand, to the rejection of any algebraic or representational constraints—a rejection that, as Stein (1988, 249) notes, has "Dirichletian resonance, since Dirichlet in his work on trigonometric series played a significant role in legitimating the notion of an 'absolutely arbitrary function', unrestricted by any reference to a formula or 'rule'"—and on the other, to "the possibilities for developing concepts."

Various recent authors have argued that even Hilbert—though he is often taken to be an early champion of the sort of formalism that would become the norm in the

[14] Riemann quoted in Stein (1988, 252). Ferreirós (2006, 80) describes Riemann's Habilitation lecture, delivered on June 10, 1854, as "one of the most celebrated achievements in the history of mathematics."

[15] As is well known, Benacerraf (1965) levels this same criticism against the identification of numbers with sets.

[16] Quoted in Avigad (2006a, 170; see also p. 183).

twentieth century—is better described as rejecting a computational, algebraic approach in favor of a more conceptual one.[17] Hilbert did famously claim that in an adequate axiomatization of geometry "one must always be able to say, instead of 'points, straight lines, planes', 'tables, chairs, beer mugs'."[18] And as Stein (1988, 253) notes, "this view—that the basic terms of an axiomatized system must be 'meaningless'—is often misconstrued as 'formalism'." In fact, Stein (1988, 253–4) suggests, "for Hilbert, axiomatization, with all basic terms replaced by 'meaningless' symbols, is *the elimination of the Kantian 'intuitive'*—the 'proceeding to concepts'." Of course, if one is resolutely Kantian regarding concepts, if, that is, one thinks that concepts without intuitions are empty, then the elimination of the Kantian intuitive just is the move to an empty formalism. But as we will see, one need not be Kantian about concepts. Although nineteenth-century mathematicians may not have been fully clear about what an alternative to the Kantian conception of concepts might amount to, nevertheless a great many nineteenth-century mathematicians, Hilbert among them, thought that their work involving concepts was perfectly contentful despite being independent of intuition in Kant's sense. As Hilbert would write in his *Natur und Mathematisches Erkennen* (1919–20), quoted in Corry (2006, 138): "We are not speaking here of arbitrariness in any sense. Mathematics is not like a game whose tasks are determined by arbitrarily stipulated rules. Rather, it is a conceptual system possessing internal necessity that can only be so and by no means otherwise."

Indeed, not only was Hilbert not a formalist, "he often opposed it explicitly" (Corry 1996, 161).

> Hilbert's own conception of axiomatics did not convey or encourage the formulation of abstract axiomatic systems: his work was instead directly motivated by the need better to define and understand existing mathematical and scientific theories.... In this view, the development of science involves both an expansion in scope and an ongoing clarification of the logical structure of its existing parts. The axiomatic treatment of a discipline is part of this growth but it applies properly only to well-developed theories. (Corry 1996, 162)

For Hilbert an axiomatization is a tool for refining one's understanding of a domain, not so much *what* one knows as *how* one knows it. What one knows is the concepts of modern mathematics together with their discoverable logical relations one to another, and (for reasons that will eventually become clear) an axiomatization of the relevant domain of knowledge can often be a crucial step on the way to such knowledge.

The diagrammatic methods of Euclid furnished a subject matter for geometry, founding thereby a new *science* of geometry, a science that Descartes could then take up and transform in accordance with his newly discovered method. For Descartes, the problems of geometry are one and all construction problems. Hilbert rejects

[17] See, for instance, Corry (1996) and (2006), Ferreirós (2006), Glas (1993), Gray (1992), and Rowe (1989).

[18] According to Stein (1988, 253), following Otto Blumenthal, Hilbert made the remark in 1891, in the course of a mathematical discussion in a Berlin railway waiting room.

constructions in turn in favor of an axiomatization. With Galois, Riemann, and Dedekind, he turns his back on constructions in the symbolic language of arithmetic and algebra, on formulae and on representations generally, to focus on concepts and the relations among them that are discoverable by reason alone.[19] It is with this *purely conceptual* form of mathematics that we are here primarily concerned. The task now is more usefully to characterize it.

5.3 The Peculiar Purity of Modern Mathematics

The practice of mathematics was transformed in the seventeenth century, and transformed again in the nineteenth. The first transformation was a metamorphosis from one way of seeing, that of our everyday mode of intentional directedness, to another, as mediated by the symbolic language of arithmetic and algebra. Space, which is at first conceived bottom-up, as the relative locations of objects, comes to be conceived top-down, as a given whole of possible positions. And what most fundamentally is is not now thought to be perceptible objects with their natures but is to be understood instead in terms of relations that arbitrary quantities can stand in, and in particular, laws that govern the behavior of bodies as from the outside. The second, nineteenth-century transformation is different. It is, in Stein's (1988, 238) words, "a transformation so profound that it is not too much to call it a second birth of the subject—its first having occurred among the ancient Greeks."[20] It obviously does not follow that the work of nineteenth-century mathematicians such as Bolzano, Galois, Riemann, Dedekind, and Hilbert would have been possible independent of the work of Descartes, Euler, Gauss, and others in the seventeenth and eighteenth centuries. Rather, Greek diagrammatic practice could give rise to the mathematics of the nineteenth-century mathematicians we have considered *only* by way of the mathematics of the early moderns. And yet, if Stein is right, early modern, that is, seventeenth- and eighteenth-century, mathematical practice enabled (in the nineteenth century) not merely a metamorphosis of that earlier practice, yet a second transformation in mathematical practice, but instead a second birth of the science of mathematics as a whole. The whole of mathematics was to be set on a new, purely conceptual foundation. Not only are objects of no concern to mathematics, even particular relations are to be set aside. *Nothing* intuitive is to be allowed in this new, purified mathematics; and insofar as it succeeds in banishing all intuition, not only

[19] Hilbert also in his *Grundlagen* realizes a new topic for mathematics, namely metatheory. This development would prove decisive for developments in mathematical logic in the twentieth century but is mostly irrelevant to our concerns here.

[20] Compare Gray (2004, 23–4): "Some historically minded mathematicians seem to have a feeling that mathematics starts again in 1800, a feeling that has more to do with the way knowledge of the real achievements of Euler and Lagrange has lapsed from their awareness than from any insight into the eighteenth century. It is produced by a feeling that mathematics is concept-driven, whereas the eighteenth century was more interested in 'mere' calculation, and this emphasis on concepts was one of the achievements of the nineteenth century."

our intuitive understanding of space and time (whether ancient or early modern) but also our constructive paper-and-pencil modes of mathematical reasoning, this new practice finally realizes the purely rational science of which both Plato and Descartes had caught a glimpse.

Relative to ancient diagrammatic practice, early modern constructive algebraic practice is *deep* insofar as it underlies and explains that ancient practice much as Newton's laws underlie and explain the apparent motions of the heavenly bodies. Mathematics since the nineteenth century is similarly deep relative to early modern practice insofar as it underlies and explains that practice. Although the science of mathematics begins with the visual appearances of things, it then moves on, first to the symbolic formulae that underlie and explain those appearances, and then finally to the fundamental and essential properties of functions that underlie and explain the formulae in which they are first expressed. But although we have a quite robust understanding of how a law can underlie and explain the appearance of something, how, for example, the law that $x^2 + y^2 = r^2$ induces the appearance of a circle in Cartesian coordinates, it is much less clear how "fundamental and essential properties" can underlie and explain a formula.[21] How *can* "Riemann's definitions of functions through internal characteristic properties" be that "from which the external forms of representation necessarily arise," as Dedekind (quoted in Avigad 2006a, 167) suggests? The first step on the way to an answer is to clarify the distinctive purity, the abstract or conceptual character, of modern mathematics.

We have seen that although the work of, say, Abel is conceptual in a sense it nonetheless ineliminably involves appeal to symbolic expressions, formulae. The work of Galois, Riemann, and Dedekind aims by contrast to be *wholly* conceptual. Functions are not to be displayed, exhibited in formulae, but instead described, thought through concepts, and the science of mathematics is in this way to achieve an understanding of its subject matter that is *intrinsic* to that subject matter, purified of all the contingencies that attach to the ways that subject matter manifests or reveals itself to us, whether in drawn diagrams or in the symbolic language of arithmetic and algebra.[22] That, at least, is the aspiration. Leaving aside for the moment obvious concerns one might have regarding the intelligibility of this aspiration, we need to understand what is distinctive both of the subject matter of modern mathematics and of its characteristic means of approach to that subject matter, that is, both its mode of access to its subject matter and its means of working with it.

Although ancient Greek mathematics was taken to be about objects—whether everyday objects conceived in a peculiar way, as Aristotle thought, or peculiar objects,

[21] We have had an intimation of this in our discussion of Galois' proof insofar as it shows how various properties of an equation can be determined on the basis of properties of its Galois group, but clearly more needs to be said.

[22] We saw that this imagery was explicit in the development of projective geometry in the nineteenth century.

as Plato thought—we have seen that in fact it was about concepts of various sorts of mathematical objects, concepts the contents of which could be exhibited in diagrams in a way enabling reasoning to new results about those concepts. With the rise of early modern mathematics the subject matter of the discipline self-consciously shifts away from concern with objects (or concepts of objects) to mathematical relations and functions that can be exhibited, expressed in formulae. The formulae of early modern mathematics do not merely represent or stand in for various mathematical relations and functions; they give those relations and functions, and as diagrams do in ancient Greek mathematical practice, they give them in a mathematically tractable way, in a way enabling one to reason one's way to new results. In the nineteenth century the concern of mathematics shifts again, this time away from particular relations and functions that can be given analytic expression in formulae to concepts.

Both Bolzano's proof of the intermediate-value theorem and Galois' proof of the insolubility of the quintic proceed not from intuitions but from concepts. For Riemann, it is not any individual functions for which an analytical expression can be given but the *concept* of a function conceived as a mapping that is fundamental to the practice of mathematics. And for Dedekind, similarly, what matters is not numbers but that we have an adequate *concept* of number, where the adequacy of a concept is measured by its usefulness in the development of a theory. Because it was generally assumed in the nineteenth century that every concept determines a class or set as its extension, it was common to use the language of sets in articulating these new developments. Nevertheless, as Ferreirós (1999, 47–53) explains, it was the notion of a concept that was primary for nineteenth-century mathematicians such as Bolzano, Riemann, Dedekind, and even, Ferreirós argues (1999, xvii), Cantor.[23] Talk of sets was little more than that, a mere manner of speaking.

This new mathematics was not about things, that is, objects with their properties and relations, nor even about those properties and relations independent of any objects. It was, or at least seemed to its practitioners to be, about concepts: concepts of objects of various sorts, concepts of various sorts of relations and functions, concepts of properties of objects and of functions, and eventually, concepts of a radically new sort of thing, namely, a structure. (Groups, rings, and fields are structures in the relevant sense.) The whole of mathematics was in this way to be reborn as a purely conceptual enterprise, one that unlike earlier mathematics, which was conceptual but unknowingly so, was explicitly and self-consciously conceptual. This new subject matter was further understood to bear no intrinsic or internal relationship to physical reality, that is, the world revealed in sensory experience

[23] As Ferreirós notes, nineteenth-century logicians most often thought of sets not as sets of objects but as sets of concepts. The intension of a concept was given by the concepts it contained (as the concept of a triangle might be thought to contain the concept of a plane figure, of being three sided, and so on); its extension was given by those concepts that fall under it—that is, that are below it in the traditional tree of Porphyry—and so contain it within their intensions (as scalene and isosceles fall under the concept of triangle and contain it within their intensions). This, we may recall, is Kant's view exactly.

(whether or not as it is). The world of the mathematician is rather a self-enclosed, autonomous realm of meaning subject only to its own, internal demands.[24] Because traditional abstractionist conceptions of concept formation can have no place in the domain of mathematics as it is now to be understood, this new subject matter required also a new account of the mathematician's intellectual access to the subject matter of mathematics.

It is very natural to think, as Aristotle did, that our concepts of such things as circles, triangles, and numbers are the result of a process of abstraction that begins with our sensory experience of instances of circles, triangles, and collections of objects. And although the relations and functions of concern to Descartes and eighteenth-century mathematicians following him are not in the same way abstracted from our sensory experience of objects and collections of objects, nevertheless it was possible, as Kant shows by example, to maintain this kind of naïve abstractionism even in the case of early modern mathematics insofar as, at least in principle, relations that are displayed in the symbolic language of arithmetic and algebra can also be exhibited in geometrical figures. One cannot in the same way display the concepts of concern to modern mathematics, for instance, the concept of a function conceived as a mapping or that of a group.[25] And in any case, we have seen, in the work of mathematicians such as Riemann and Dedekind, there was a self-conscious turn away from representations of numbers and functions on the grounds that particular representations introduce something arbitrary and inessential, something merely external, into what ought to be the study of the *internal*, *constitutive* features of things. Thus, although one might first discover, say, the group concept by consideration of cases, by a kind of Aristotelian naïve abstraction, the concept in its mathematical use is instead to be given by a kind of top-down characterization in terms of the properties that are constitutive of a group.[26] The fact that the instances have these properties is to be seen as secondary.

On the view of naïve abstractionism the concept is what it is because the instances are what they are; it is the instances that are explanatorily basic. This is, furthermore, explicit in ancient thought: one can give a definition only of what exists because what the definition serves to do is to set out the essence of things of that kind. It sets out what it is to be that kind of thing. One must then start with instances of the kind as the basis on which to develop one's definition. On the new approach the explanatory order is reversed: the instances are instances of the concept because the *concept* is

[24] This feature of modern mathematics is one of the reasons Gray (2009) offers for thinking that this mathematics, in particular, that of the last decades of the nineteenth century, is not only modern but modernist, part of the same modernist movement one finds in painting, music, and literature.

[25] One can, for course, draw little pictures—say, of two collections of dots with little arrows from the dots in one collection to the dots in the other—that are meant to depict the notion of a mapping. But such pictures are not mathematically tractable; they do not present the notion of a mapping in a way that enables reasoning to new results in mathematics.

[26] Buldt and Schlimm (2010) also suggest something like this, though they develop the idea differently from the way it is developed here.

what it is, constitutively and intrinsically characterized by higher-order properties. The concept comes first, in the order of being if not in the order of knowing. And it must come first if mathematics is to be freed of the limitations that would otherwise be imposed on it by what is (to us as the sort of beings we happen to be) intuitive.

A simple example illustrates the essential point.

One begins in arithmetic by considering cases, particular sums, products, and so on, and on that basis comes sooner or later to notice that the order in which one, say, adds does not matter to the result, that adding two to three is equivalent to adding three to two, and so on for any pair of numbers one cares to consider. Abstracting from the particular numbers one might then say that $a + b = b + a$ holds generally, that is, for any pair of numbers one cares to consider. But now we can invert the order of explanation, see this equation not as expressing a general feature, that is, a property, of particular sums of numbers (all of them), but instead as disclosing a constitutive feature of the *operation* of addition, that, as we in fact say, addition is (that is, has the property of being) commutative.[27] That is, we read the equation not now as about numbers but as ascribing a property directly to the operation of addition; *numbers* do not come into it at all. And once having taken such a top-down approach to addition in terms of its intrinsic properties, we are free to apply the notion in contexts involving numbers that are quite unlike traditional numbers so long as they behave well enough with respect to addition as it is now to be understood.

Consider now not only the commutativity of addition but also other familiar fundamental laws of elementary algebra, namely, these.

1. Commutativity of addition and multiplication: $a + b = b + a$, $a \times b = b \times a$
2. Associativity of addition and multiplication: $(a + b) + c = a + (b + c)$, $(a \times b) \times c = a \times (b \times c)$
3. Distribution: $a \times (b + c) = (a \times b) + (a \times c)$
4. The existence[28] of zero: There is a number 0 such that $a + 0 = a = 0 + a$
5. The existence of unity: There is a number 1 such that $a \times 1 = a = 1 \times a$
6. The existence of additive inverses: For any number a there is a number $-a$ such that $a + (-a) = 0 = (-a) + a$

All these laws, although first formulated for ordinary numbers (the integers, rationals, and reals), soon came to be seen to hold of other systems of numbers

[27] Notice that the expression '$a + b = b + a$' can be read either way, either as a generalization based on, or grounded in, particular instances, or as ascribing a higher-level property, that of being commutative, directly to addition, independent of any reference to any numbers. The equation can be read, that is, either bottom-up, as about all numbers of a certain sort (namely, sums), or top-down, as about the operation of addition that it has the (higher-level) property of commutativity. We will return to this.

[28] Surely here, and in (5) and (6), one might think, one is referring to objects. But this is not necessarily so; what is generally described as asserting existence can instead be taken, as in the other cases, as an ascription of a higher-level property to a lower-level concept, here, as the ascription of the property of having at least one object falling under it to the concept *being a zero*, that is, being such that if added to any number the result is just that number again.

(such as the integers modulo n) as well. Any system S of numbers that is closed under addition and multiplication (that is, if a and b are in S then so are $a + b$ and $a \times b$) and satisfies these laws is a commutative ring with unity. A (simple) ring is a structure that satisfied (1) through (4), save for the commutativity of multiplication, plus (6). And other similar mathematically interesting structures exist as well. Obviously, any theorem that can be proven on the basis of just these basic laws governing addition and multiplication (or a proper subset of those laws), will hold in any system of numbers that satisfies the laws (or subset of laws) from which the theorems are derived.[29]

The concept of a group, the central concept of abstract algebra, takes things a step further. It concerns not the arithmetical operations of addition and multiplication but instead the concept of a collection of elements C together with some operation * on those elements—where C is closed under * (that is, if a and b are elements in C, then $a * b$ is an element of C as well)—that satisfies the following three constraints:

- The operation * is associative: $(a * b) * c = a * (b * c)$,
- There is an identity element **i** in C such that $\mathbf{i} * a = a = a * \mathbf{i}$,
- Each element a in C has an inverse a' such that $a * a' = \mathbf{i} = a' * a$.

That is, much as Descartes abstracts from particular measurable quantities to focus on the relations such quantities can stand in, so in group theory one abstracts not only from any actual collections of elements but also from any particular relations. Ontologically a group is a kind of an object (that is, it is not a property or relation), but it is a peculiar sort of object insofar as it is not merely a thing with a nature or a subject of predicates, a primary bearer of properties. It is instead a kind of *structure*, where a structure is a system of elements in a certain relationship. (Rings and fields are also structures, though more determinate than that of a group.) The concept of a group, as set out above, is the concept of such a system or structure. Although at first abstracted (bottom-up) from particular mathematical instances of groups, the group concept comes in this way to be understood top-down, as given by its constitutive properties, the associativity of its operation and so on.

In the concept of a group there is no reference either to any particular elements or to any particular operations but only to certain properties of collections of elements and of operations. And the elements in the collection need not even be objects, though they can be. In Galois' work, for example, the elements are the various permutations that are possible on roots; and the operation * is composition on permutations, that is, one permutation followed by another, which is associative. The identity element is the do-nothing permutation, and because for every

[29] This observation would prove an important catalyst on the way to the standard model-theoretic conception of language, although as already indicated that way of proceeding is in no way necessary for an adequate understanding of what is going on here. We will return to this.

permutation there is one in reverse, the last condition is satisfied as well. The collection of permutations together with composition thus form a group.

The integers similarly form a group under the operation of addition because: (i) any two integers can be added together to form another integer, (ii) the addition function is associative, (iii) there is an identity element, namely, zero, and (iv) for every integer (positive and negative) there is an inverse (negative and positive, respectively, with zero as its own inverse). If zero is excluded, the rational numbers likewise form a group under multiplication. These facts about the integers under addition and the rationals, excluding zero, under multiplication furthermore explain the fact that linear equations of the form '$a + x = b$' and '$ax = b$' (where, for the multiplicative case, $a \neq 0$) are inevitably solvable. A simple example illustrates the point. We begin with a linear problem, say, $7 + x = 12$. Because seven has an additive inverse, namely, minus seven we can add that number to both sides: $-7 + (7 + x) = -7 + 12$. Because addition is associative, we can infer that $(-7 + 7) + x = -7 + 12$, and by a simple computation get that $0 + x = -7 + 12$. Because zero is the identity element for addition, it follows that $x = -7 + 12$. Simple computing yields the result: $x = 5$. And the same point holds *mutatis mutandis* for the case of the rationals under multiplication. Much as the law that $x^2 + y^2 = r^2$ underlies and explains the characteristic look of a circle so we see here how fundamental properties of groups can underlie and explain characteristic results of early modern mathematics.

The mathematical concept of a group has proved extraordinarily fruitful. Beginning with only a few explicitly defined concepts (e.g., that of a group, a subgroup, and so on), mathematicians have proven a whole range of important, and sometimes surprising, theorems. Group theory has furthermore revealed important connections between different parts of mathematics. Felix Klein, for example, used group theory to show why the quintic equation—which as we saw is not generally solvable by radicals—is solvable if appeal is made to a certain sort of complex function, the elliptical function; and he did so by showing deep interconnections between the quintic, the theory of rotation groups, and the theory of complex functions. Nor is the interest of group theory confined to mathematics. Group theory has important applications in, for instance, physics, chemistry, and engineering. The notion of a group is exemplary not only of the sorts of entities that are studied in modern mathematics but also of its extraordinary fruitfulness and power.[30]

The concepts of modern mathematics, although they may first be discovered by reflection on instances, are to be understood independently of any instances, by appeal only to various properties and relations as set out in definitions. Something is an instance of the concept if, and only it, it has the features stipulated in the

[30] This fruitfulness has been a source of much puzzlement: how can it be that this mathematics that self-consciously turns its back on the empirical world nonetheless provides such powerful tools for understanding that same world? Wigner (1967) is a classic expression of just this puzzlement. We shall have something to say about it in Chapter 9.

definition. This begins to explain why the concepts of modern mathematics have sometimes been described as Kantian ideas, that is, as concepts of reason rather than concepts of the understanding. Hilbert, for example, famously begins his *Grundlagen* with an epigraph taken from Kant's first *Critique* (A702/B730): "All human knowledge thus begins with intuitions, proceeds thence to concepts and ends with ideas." Whereas ancient Greek mathematics, at least on Aristotle's account of it, begins with intuitions, sensory experience of objects, and on Kant's account begins instead with concepts (of the understanding) that can be constructed in pure intuition, Hilbert's mathematics, so Hilbert seems to suggest, begins with (Kantian) ideas, that is, concepts of reason to which no corresponding objects need (or, Kant thinks, can) be met with in experience.

Husserl similarly, writes in his *Ideen* (1913), quoted in Boi (1992, 203), that geometrical concepts are "'ideal' concepts, expressing something we cannot 'see'; their origin, and also therefore their content, differ essentially from those of 'descriptive' concepts.... Exact concepts have as their correlation essences which have the character of 'ideas' in the Kantian sense of the word." What Husserl here refers to as descriptive concepts are Kant's concepts of the understanding, concepts of objects, properties, and relations that can be met with in experience. Geometrical concepts are not, Husserl, suggests, such descriptive concepts but instead Kantian ideas, that is, concepts of reason. Modern geometry thus achieves, according to both Hilbert and Husserl, the standpoint of pure reason; it completes and fully actualizes the science first developed by the ancient Greeks. Its concepts are now explicitly understood to have their contents independent of any and all relation to any object given in intuition.

But Kantian ideas are not only concepts of reason rather than concepts of the understanding. They also, Kant tells us in the *Jäsche Logic* (1800, 590; AK 9:92), "*cannot* be attained *by composition*... for [in the case of such concepts] the whole is prior to the part."[31] That is, as Husserl puts it in the passage just quoted, "their origin, and also therefore their content, differ essentially" from the origins and contents of the concepts of the understanding. Kantian ideas are in this latter regard more like Kantian intuitions than they are like Kantian concepts (of the understanding). In the Transcendental Aesthetic (A25/B39), for example, Kant appeals to the fact that space as a whole is prior to the parts of space to *argue* that space must therefore have the form of an intuition rather than that of a concept, in which the part is inevitably prior to the whole. Kantian ideas, like Kantian intuitions, are wholes that are prior to their parts. Hence, if modern mathematical concepts are Kantian ideas, or at least something very like Kantian ideas, they possess a kind of organic or essential unity

[31] It is worth recalling that Frege held that Kant was wrong to think that concepts can be defined only by composition using simple Boolean operations of union and intersection. In fact, he argues much more powerful means of definition are available and such means, he suggests, help to explain how deductions of theorems from such definitions can extend our knowledge. This idea will be taken up again in Chapter 8.

that is quite distinctive of them. By contrast with the accidental unity of a concept of the understanding, whose parts are intelligible independent of the whole, the parts of a Kantian idea can be understood only through the whole much as, so it would seem, the parts of an essential unity can be understood only through the whole. In fact, the concepts of modern mathematics are neither essential unities, at least in the way that living beings are essential unities according to Aristotle, their parts intelligible only in light of the form of life taken as a whole, nor accidental unities, as Kantian concepts of the understanding are according to Kant. Concepts of modern mathematics are instead *intelligible* unities, wholes of parts that are nonetheless not reducible to their parts. Such unities have independently intelligible parts, as accidental unities do, but as in an essential unity, the whole is more than merely the sum of the parts and cannot be attained by composition, just as Kant says.

Were the concepts of modern mathematics merely composites of parts that are antecedently available (as Kant had thought), then once mathematics is freed of any relation to intuition there would seem to be no constraints (save for methodological ones) on what might count as a concept of modern mathematics; any structure would be as good as any other provided that it could be treated mathematically. And many take this to be the way things actually are in modern mathematics. If mathematicians distinguish, as indeed they do, between "real" mathematics and mere play with made-up, or at least non-mathematical, concepts, then (so it is claimed) that can only be out of some sort of prejudice or some not strictly mathematical sentiment. But again, *mathematicians* do not understand mathematics as a mere game with arbitrarily formed concepts. They treat the concepts of concern to them as intelligible unities whose natures we can discover, analyze, and sometimes fail altogether to grasp. If these mathematicians are right, if the concepts of modern mathematics really are intelligible unities, real wholes of real parts, then mathematical concepts have their own discoverable contents.

Kant thought that in mathematics definitions are synthetic, that one puts together concepts to form new concepts in mathematics rather than having to analyze given concepts, as in philosophy. But as Bolzano seems to have been the first clearly to recognize, the sort of mathematical practice we have been here concerned with requires precisely what Kant had claimed has no place in mathematical practice, namely, the analysis of concepts. Instead of beginning with clearly understood primitives that can then be combined in definitions on the basis of which to construct instances in pure intuition, Bolzano's practice requires that one first analyze the concepts actually in use, concepts such as that of a function, of continuity, and so on, and on that basis form adequate definitions of those concepts, definitions that in turn enable derivations, deductive proofs, of various properties and relations of those mathematical concepts. This by itself does not show that characteristically modern mathematical concepts are intelligible unities, however. Because, according to Kant, all concepts of the understanding, those of philosophy as much as those of mathematics, are composites, the fact that some need to be analyzed into their parts—which for the case of concepts of the understanding is a reductive process, one that reduces the relevant concepts to their parts—rather than built up out of parts, is not enough to distinguish modern mathematical concepts from Kantian concepts of the understanding. If, however, we combine this notion of analysis with another feature of Bolzano's practice, a very different picture begins to emerge.

If, as Bolzano does, one aims to derive theorems on the basis of defined concepts then one needs (for reasons we will eventually need to become clear about) to appeal also to primitive propositions. Bolzano's search for adequate definitions of fundamental mathematical concepts goes hand-in-hand with a search for adequate foundations, a set of primitive propositions on the basis of which to derive theorems. And as Rusnock (1997) argues, for Bolzano the adequacy of both his definitions and his primitive propositions is determined not by their intuitive appeal or their evident truth, but experimentally.

> A search for the grounds of a given proposition will begin with a determination of which concepts it contains. The grounds which one adduces for the proposition should be other propositions involving the concepts contained in it. Then the problem becomes one of reverse mathematics: to find a set of premises using only those concepts from which the given proposition is exactly deducible. At a certain point, one will, Bolzano thinks, arrive at propositions which, due to their degree of simplicity and their deductive relations, appear as grounds but never as consequences of the other propositions of a science; and these are axioms. By their nature axioms cannot be proved. They must nevertheless be justified; not, however, by their self-evidence; rather, because after repeated trials of various possibilities, a given set seems to do the job of supporting the deductive structure of a science best. (Rusnock 1997, 80)

This conception of the nature and role of an axiomatization is resolutely anti-foundationalist and fallibilist.

Riemann in his Habilitation lecture similarly underscores the need for the analysis of concepts in mathematics. The task of the lecture is to set the practice of geometry on a new, self-consciously conceptual foundation and thereby to supersede Kant's account of geometry as a body of synthetic a priori truths about physical space. Riemann's first task, then, is to clarify the concept of a multiply-extended quantity, and this task is "of a philosophical nature" because "the difficulties lie more in the concepts than in the construction" (quoted in Nowak 1989, 26). Indeed, the whole of the first section (of three) of the lecture is primarily "philosophical" insofar as its concern is to clarify the concept of an n-dimensional manifold as the foundation of a general theory of the concept of space.

Riemann was furthermore profoundly and self-consciously historicist in his conception of mathematics; for him "mathematical concepts and axioms are immersed in a historical process of change" (Ferreirós 2006, 94).

> Far from the usual idea that there exists (in some Platonic realm) a ready-made theory of everything, in his view all concepts of natural science, and of mathematics in particular, have

evolved gradually from older explanatory systems. Scientific theories are for Riemann the outcomes of a process of gradual transformation of concepts, starting from the basic ideas of object, causality, and continuity. Development takes place under the pressure of contradictions or else implausibilities [*Unwahrscheinlichkeiten*] revealed by unexpected observations—unexpected in the light of the hypotheses proposed by reflection at some particular stage. (Ferreirós 2006, 77)

That is, as Frege would more exactly put it, not our concepts but instead our grasp of concepts evolves and is transformed. Over time, with further inquiry, we thus can come better to understand concepts that we had been concerned with all along.

Mathematics is not an empirical discipline but it is, for both Riemann and Bolzano, nonetheless in a way experimental. There are no given first principles from which to deduce consequences; rather, it is developments within the discipline that reveal new concepts, and indeed new kinds of concepts, which once they have become available can be refined and their consequences explored. As Bolzano's are, the foundations that Riemann proposes are and must be provisional. They have, as he puts it, the status of hypotheses. Like any other form of intellectual inquiry, mathematics as it came to be practiced over the course of the nineteenth century is in this way constitutively self-correcting. The search for foundations is not, for these nineteenth-century mathematicians, the search either for certainty or for ontological grounds that it would become in the twentieth century, but instead a search for the proper conceptual foundations of the discipline of mathematics as it had by then become.[32]

According to both Bolzano and Riemann, the science of mathematics does not rest on any given foundation but nor is it merely a game. There are concepts that are proper to it as its subject matter, concepts such as that of an infinite-dimensional manifold, of a group, of a Lie algebra, and these concepts are to be intrinsically characterized in definitions on the basis of which theorems are to be proved *or* to be shown to be in need of more careful articulation. It is the course of proof *and refutation* that enables us to extend our knowledge about those concepts, in some cases by revealing connections among them and in others by revealing our errors and misunderstandings, the fact that we had not after all understood them as well as we ought. And this, sketchy though it is, suggests that the concepts of modern mathematics really are quite like Kant's concepts of reason, both in their independence from anything given in intuition or sensory experience and in their characteristic (intelligible) unity. Much more will need to be said to fill out this picture but already we can begin to see how profoundly *different* developments in mathematics in the nineteenth century looked to nineteenth-century mathematicians from the way they would come to seem to philosophers in the twentieth.

[32] See especially Ferreirós (2006, 68–72).

5.4 Prospects for the Philosophy of Mathematics

An adequate philosophy of mathematics ought to clarify the nature of mathematical knowledge, both what it is we know in knowing some bit of mathematics and also how we know it. That is, it ought to explain the inner connection between mathematical truth and mathematical knowledge, to combine a plausible metaphysics of mathematics with a plausible epistemology of it. But as the practice of mathematics is transformed over the course of its history so, we have seen, the nature of the philosophical problem it presents is transformed. For the ancient Greeks, we saw, the problem concerns the ontological status of and our epistemic access to the geometrical and numerical objects that ancient Greek mathematicians and philosophers assumed (justifiably, given their mode of intentional directedness on reality) were the subject matter of mathematics. And many still today take the problem of mathematical truth and knowledge to concern its objects: "the standard view is that there *are* mathematical objects (natural numbers, real numbers, sets)" (Potter 2007, 26).[33] (This may seem to be obvious: if, as is surely true, there is no largest prime number, then does it not *follow* that *there are* prime numbers, indeed, infinitely many of them? In fact it does not, although we do not as yet have the resources to understand why. We make the inference only because we assume a quantificational conception of generality.) The problem, then, is to understand the ontological status of those objects and the nature of our epistemic access to them.

Already in the seventeenth century Descartes came to think that mathematics is not about objects but instead about relations that objects, more exactly, measurable quantities, can stand in. And the way to account for this sort of mathematical knowledge, he thought, was by appeal to God-created clear and distinct ideas that we find innate in us; we discover new mathematical truths by reflecting on those ideas and determining what must be true in light of them. But that, Kant saw, is not only inadequate as a philosophical account of truth and knowledge in mathematics (because it is merely dogmatic), it also falsifies Descartes' actual mathematical practice, which constitutively involves the symbolic language of arithmetic and algebra. Does Kant's alternative account in terms of constructions in pure intuition, in terms, that is, of the *activity* of paper-and-pencil reasoning that is made possible by the forms of inner and outer sense, resolve the problem of truth and knowledge in mathematics? The answer seems obvious. Kant's philosophy of mathematics in terms of the construction of concepts in pure intuition, whatever its virtues, has no application to a mathematical practice in which one reasons directly from concepts.

[33] Recall that with Kant all objectivity is taken to lie in relation to an object. After the practice of mathematics was revealed, over the course of the nineteenth century, to make no essential use of the forms of sensibility and so could no longer be seen as concerned with the forms of objects as Kant had thought, it came to seem (at least to those who remained Kantian in this regard) that mathematics somehow must have its objects from some other source, either that or it is merely formal, a mere game with symbols.

Mathematical practice, as it emerged over the course of the nineteenth century, does not involve the construction (either ostensive or symbolic) of concepts in intuition, but instead deductive reasoning from concepts. But how can one learn anything new by reasoning from concepts? How can a deductive proof extend one's knowledge? Already in 1894, Poincaré raises the essential difficulty. He writes in the opening paragraphs of "On the Nature of Mathematical Reasoning" (Poincaré 1894, 394):

> The very possibility of mathematical science seems an insoluble contradiction. If this science is only deductive in appearance, from whence is derived that perfect rigour which is challenged by none? If, on the contrary, all the propositions which it enunciates may be derived in order by the rules of formal logic, how is it that mathematics is not reduced to a giant tautology? The syllogism can teach us nothing essentially new, and if everything must spring from the principle of identity, then everything should be capable of being reduced to that principle. Are we then to admit that the enunciations of all the theorems with which so many volumes are filled are only indirect ways of saying that A is A?
>
> No doubt we may refer back to axioms which are at the source of all those reasonings. If it is felt that they cannot be reduced to the principle of contradiction, if we decline to see in them any more than experimental facts which have no part or lot in mathematical necessity, there is still one resource left to us: we may class them among *a priori* synthetic views. But this is no solution of the difficulty—it is merely giving it a name; and even if the nature of the synthetic views had no longer for us any mystery, the contradiction would not have disappeared; it would only have been shirked. Syllogistic reasoning remains incapable of adding anything to the data that are given it; the data are reduced to axioms, and that is all we should find in the conclusions.[34]

Notice that Poincaré's concern is not with the starting points, whatever they are, of mathematical reasoning, but instead with the reasoning itself, and the apparent sterility of purely logical reasoning, its inability to realize something new that was not contained already in one's starting points. His concern is that if mathematics were a practice of reasoning from concepts by logic alone then its results would be, at least in Kant's view, which Poincaré clearly seems to endorse, analytic, that is, merely explicative rather than ampliative, which is to say, not knowledge properly speaking at all. How by way of strictly deductive, truth-preserving proof is it possible to discover truths, that is, to extend our knowledge? We will consider four possible answers.

According to Kant, deduction, because it is by logic alone, is merely explicative. Nothing new can arise from such reasoning; one only makes explicit something that was implicit already in one's starting point. One might then argue (modus ponens) that because modern mathematics *is* a practice of reasoning by logic alone, it is merely explicative, not a science, an investigation into the truth of some matter, at all. If there cannot be knowledge by means of deductive proof then, given that mathematics is a practice of deductively proving theorems, what is proven in mathematics

[34] Nothing turns here on the fact that Poincaré takes all logical reasoning to be syllogistic. His concern about the sterility of logical reasoning stands even if we admit much richer forms of reasoning than syllogistic.

does not amount to knowledge. All the mathematician does, on this account, is draw out the logical consequences of whatever axioms and definitions mathematicians care to consider thereby making explicit something contained already implicitly in those starting points. One may seem to learn something new but really it was all there already, one just did not know it. So, while there may be a psychological sense in which one acquires new knowledge, logically speaking one does not because that which has been extracted in the course of the reasoning was already there—"as beams are contained in a house" (Frege 1884, sec. 88). Much as one can dismantle a house and use its beams to build a fence (say), so using logic one can dismantle the premises and put the parts back together in a way to form the conclusion. In that case, all that has been achieved is a reorganization of something one already had. One has not *extended* one's knowledge but merely reorganized it, and useful though this reorganized knowledge may be it should not be confused with *actually* achieving something new.[35]

Alternatively, one could argue (modus tollens) that because mathematics *is* a science, an investigation into the truth of some matter, it cannot proceed merely logically, by deducing theorems on the basis of axioms and definitions, but must involve something other than pure logic. This was Poincaré's view, that mathematical reasoning is not merely logical but instead distinctively mathematical. This is also, though in a different way, what Kant had thought, that mathematics involves a distinctive use of reason, one that is different from reason in its discursive use; according to Kant, what is characteristic of the mathematical use of reason is that it involves constructions in pure intuition. What Poincaré thinks is characteristic of distinctively mathematical reasoning is not that it involves Kantian constructions but instead that it involves, in addition to strictly logical modes of reasoning, also mathematical ones. There are two different ways this might go.

First, it might be the case that although some mode of reasoning *could* be broken down into a series of strictly logical inferences, in mathematical reasoning it is not so broken down but treated as immediately valid. Poincaré seems to suggest such a view in the following passage (quoted in Detlefsen 1992, 361):

Our body is formed of cells, and the cells of atoms; are these cells and these atoms then all the reality of the human body? The way these cells are arranged, whence results the unity of the individual, is it not also a reality and much more interesting?

A naturalist who never had studied the elephant except in a microscope, would he think he knew the animal adequately? It is the same in mathematics. When the logician shall have broken up each demonstration into a multitude of elementary operations, all correct, he will

[35] This is a very common view among philosophers beginning with Russell in 1903, at least for the case of recent mathematics. We read, for example, in Potter (2007, 17): "if I prove something about all groups or all Lie algebras, then what I know *is* just a piece of logical knowledge. The philosophical view known as implicationism (or, less elegantly, if-thenism) is a perfectly adequate explanation of what is going on in these cases." (Implicationism is the view that mathematical truths have the form of conditionals, with the axioms in the antecedent and the theorems derived from them in the consequent.)

still not possess the whole reality; this I know not what which makes the unity of the demonstration will completely escape him.

Although it may be possible to break down a mathematical mode of inference such as mathematical induction into a multitude of elementary and strictly logical steps, much as an animal's body can be broken down into the cells of which it is composed, to so break it down destroys what is in fact an essential unity. One can no more learn about mathematics, or indeed learn *mathematics*, by focusing on the strictly logical inferences of which its modes of inference are in some sense composed than one can learn about an elephant by focusing on the cells of which it is in some sense composed.

Suppose that the mathematician infers some conclusion from a set of premises by means of some form of inference that the mathematician claims is mathematically valid, and that our mathematician can convince other mathematicians that the conclusion does indeed follow. That is, our mathematician can get other mathematicians to see, to grasp, the validity of the inference. Others, those who are not mathematicians we will suppose, are utterly baffled; they simply cannot understand or grasp how the conclusion follows. So the logician breaks the inference down into a series of strictly logical steps, and now even a non-mathematician can understand that the conclusion can be derived from the premises. The problem is that what the non-mathematician is now seeing is not mathematics but only logic. The *mathematics* has been lost in the course of the dissection of its modes of inference into their logical parts. What is distinctive of mathematical ability, on this account, is that it enables one to recognize irreducible mathematical wholes, valid mathematical modes of inference that *can* be broken down into strictly logical steps but *qua* mathematical are essential unities and cannot be so reduced to logic. On this view, what the non-mathematician lacks is the ability to discern these unities in addition to the logical steps they can be broken up into. The non-mathematician is like one who can see only the cells of the elephant, the elephant as a collection of cells, but not also the elephant, that is, the essential unity in virtue of which it is an *elephant*, an animal of a certain sort.[36]

The idea that there is an irreducible unity to a mathematical mode of inference despite the fact that it can be broken down into a series of purely logical steps in much the way there is unity to an elephant despite the fact that it can be broken down

[36] Grabiner (1974, 358), in a discussion of why standards of rigor changed over the course of the nineteenth century, suggests that it was not primarily to avoid errors insofar as "there are surprisingly few mistakes in eighteenth-century mathematics"; and the reason there are so few errors in eighteenth-century mathematics, she suggests, is in large part due to the fact that "eighteenth-century mathematicians had an almost unerring intuition. Though they were not guided by rigorous definitions, they nevertheless had a deep understanding of the properties of the basic concepts of analysis." From such a perspective it might easily seem that developing one's intuitive understanding of the basic concepts of a domain of mathematics is much more important mathematically than trying to spell out in full logical detail the contents of those concepts.

into a mere collection of cells is not merely frivolous. An interesting theorem is (often) just such a unity. We know that in a sense a theorem can be broken down into a series of little steps, that is, that it can be proven. But although we are interested in the proof, all the little steps, we are *also* interested in the theorem. Looking at the theorem is analogous to looking at the elephant because the theorem, in its way—and *qua* theorem, that is, as something that has been proven to be true—holds all those little steps "inside" itself. Looking instead at the proof that gets you from (say) the antecedent to the consequent of the theorem is like looking at the collection of cells that in a material sense makes up the elephant. But although this may help to explain the nature of mathematical understanding, it is not clear that it helps with the issue of mathematical knowledge. If mathematical induction, say, can be broken down into a series of logical steps then it would seem that using the rule of mathematical induction cannot *extend* one's knowledge *because* in fact the conclusion is derived by what has been revealed to be a strictly deductive procedure.[37]

The second way there might be distinctively mathematical modes of reasoning in addition to logical ones is if there were (primitive) modes of material inference in mathematics, for example, ineliminable relations of entailment among the concepts of mathematics. Suppose that being F entails being G, not logically (as would be the case if, say, being F were simply a matter of being G and H), but because of what it means to be F, that is, in the way that one thing's being larger than another entails (in virtue of what 'larger' means, and in particular, that the relation it names is anti-symmetric and irreflexive) that the second is not larger than the first. A mathematical inference would then be an inference that depended in this way on the meanings of the mathematical ideas involved. To understand those ideas would enable one to recognize the validity of the inferences, and although one could always express those relations of entailment in conditionals and treat them as premises, thereby turning one's materially valid inference into a formally valid inference, to do so would again be to lose sight of the mathematics involved. Detlefsen (1992, 369) provides this explanation.

Even though the warrant for 'if **p**, then **c**' may be based on a grasp of a mathematical architecture [or as I have put it, on a principle of material inference] linking **c** with **p**, the modus ponens counterpart nonetheless *abstracts away from* this grasp of architecture itself and focuses instead on its net classical effect; that is, on the classical semantico-epistemic status (e.g., truth, certitude of truth, *a priori* certitude of truth, etc.) and logical form of the belief (viz., 'if **p**, then **c**') which it warrants. . . . It treats grasp of an architecture as merely the means by which the classical semantico-epistemic status of 'if **p**, then **c**' is established while viewing the semantico-epistemic status itself, rather than the means of establishing it, as the matter of primary epistemic importance.

[37] Compare Goldfarb (1988), which argues that Poincaré's anti-logicist arguments are psychologistic, that "the dispute between Poincaré and the logicists amounts, at bottom, to a difference in the conception of the foundational enterprise" (Goldfarb 1988, 70).

Although the materially valid rule that enables one to infer **c** on the basis of **p** shows that knowing that **p** provides a good reason for inferring that **c**, if the inference the rule governs is replaced by another, strictly logical inference involving also the conditional 'if **p**, then **c**', then **p** no longer serves as the ground for **c**. What one now has is a logically valid inference, and what one now knows is only that given that the premises are true the conclusion is also true. One has lost sight of *why* the conclusion is true. That **p** no longer can be seen to serve as one's reason for inferring that **c**. And this happens because what had been functioning as a rule of inference regarding what is a reason for what is reduced to a merely truth-functional conditional, that this is true if that is true, thereby robbing the original premise of its status as a reason. What had been a good *mathematical* ground for inference is reduced to a merely logical ground.

Interestingly enough, Descartes' account in his Second Replies of the analytic and synthetic methods of demonstration in geometry seems to make essentially this point.[38] As he explains, the order of argumentation in the two cases is the same: "the items which are put forward first must be known entirely without the aid of what comes later; and the remaining items must be arranged in such a way that their demonstration depends solely on what has gone before" (AT VII 155; CSM II 110). Whether the method of demonstration is analytic or synthetic, it is deductive, at least in a broad sense ("what comes later depends solely on what has gone before"), and non-circular (what is first put forward "must be known entirely without the aid of what comes later"). Nevertheless, Descartes thinks, the two methods are very different.

Analysis shows the true way by means of which the thing in question was discovered methodically and as it were *a priori*, so that if the reader is willing to follow it and give sufficient attention to all points he will make the thing his own and understand it just as perfectly as if he had discovered it for himself. But this method contains nothing to compel belief in an argumentative or inattentive reader; for if he fails to attend even to the smallest point he will not see the necessity of the conclusion. Moreover there are many truths which—although it is vital to be aware of them—the method often scarcely mentions, since they are transparently clear to anyone who gives them his attention. (AT VII 155–6; CSM II 110)

In the *Meditations*, Descartes tells us, it was the analytic method that was followed, and the method is "a priori" because, as Descartes indicates in his account of synthesis, it is prior to the method of synthesis.

Synthesis, by contrast, employs a directly opposite method where the search is, as it were, *a posteriori* (though the proof itself is often more *a priori* than it is in the analytic method). It demonstrates the conclusion clearly and employs a long series of definitions, postulates, axioms, theorems, and problems, so that if anyone denies one of the conclusions it can be

[38] This passage has been variously interpreted. See, for instance, Gaukroger (1989), ch. 3, and Curley (1986).

shown at once that it is contained in what has gone before, and hence the reader, however argumentative or stubborn he may be, is compelled to give his assent. (AT VII 156; CSM II 110–11)

In a synthetic demonstration everything on which the proof depends, save for the rules of formally valid inference employed, is explicitly stated in advance in axioms, definitions, postulates, (previously demonstrated) theorems, and (previously solved) problems. Because it is, the proof compels assent by anyone, no matter how argumentative or stubborn, who accepts those axioms, definitions, postulates, theorems, and problems. But of course one can set out everything on which a proof depends only after one has the proof itself, only after one has achieved, analytically, knowledge of the "primary notions" and has drawn (materially valid) conclusions from them. It is for just this reason that the analytic method requires, in a way the synthetic does not, the closest attention to each point and enables, in a way the synthetic does not, a reader to "make the thing his own and understand it as perfectly as if he had discovered it for himself." The analytic method employs (unstated) rules of material inference to draw conclusions from evidently true premises. In this case, one is to *see* that the truth of the premises entails the truth of the conclusion, but in order to see that, one must actually think about what the premises mean, and thereby about what they entail. The synthetic method, because and insofar as it relies only on formally valid rules of inference, does compel assent; but because it does not require that one actually think about what is being claimed in the premises, it does not ensure understanding.[39] It follows, Descartes thinks, that the synthetic method "is not as satisfying as the method of analysis," that the analytic method is "the best and truest method of instruction" (AT VII 156; CSM II 111).[40]

According to the view under consideration, there are rules of material inference, that is, relations of entailment among concepts, that although they can be stated as premises instead function in mathematics directly as inference licenses. Again, this is different from claiming that what mathematicians treat as distinctively mathematical inferences can nonetheless be unpacked in ways that reveal them to be purely logical. When one makes a rule of material inference explicit as a conditional in a premise, one does not thereby show it to be purely logical. It is one thing to claim that, say, mathematical induction is a distinctively mathematical mode of inference, though of

[39] We will see an interesting instance of this in Chapter 7. One does not have to struggle to understand Frege's definitions or the strategy of his proof of theorem 133 in the 1879 logic in order to determine that every step is valid in the system. Nevertheless, as we will also see, it takes *significant* work *really* to understand the proof, how it depends on an understanding of its primary notions. It is only through that work that one comes to a fully adequate understanding of the structure of the proof as a whole.

[40] It is worth noting that, according to Detlefsen, what Descartes here calls the synthetic method can nonetheless have an important role to play in mathematics. "In times of epistemic crisis, when it becomes necessary to revise epistemic holdings which, judged from a purely mathematical point of view, seem unimpeachable, generating logically complete bases may prove to be the only, or at least the optimal, way of proceeding. Such a procedure has the advantage of explicitly exposing certain assumptions which mathematical rigor does not bring to light" (Detlefsen 1992, 367).

course one can include it (suitably formulated) as a premise of an inference modus ponens, and something very different to claim that mathematical induction, though it appears to be, and even perhaps is, distinctively mathematical, can nonetheless be broken down into a series of strictly logical steps. But although these are different claims, either way the thought is that while one *can* make everything explicit and hence logically valid in a mathematical proof, either by including as premises all the materially valid rules involved or by breaking all the mathematically valid rules down into their strictly logical parts (or by doing both), this destroys the *mathematical* reasoning, and destroys as well mathematical understanding. Rav (1999) seems to take essentially this view insofar as he argues that the mathematician's proof is *irreducibly* meaningful and so cannot be identified with a derivation in some formal system precisely because such a derivation is merely syntactic. Although, according to Rav, "most of our current mathematical theories can be *expressed* in first-order set-theoretic *language*" (Rav 1999, 20 n. 20), a mathematician's proof cannot be formalized in such a language because it "depends on an understanding and on prior assimilation of the *meanings* of concepts from which certain properties follow logically" (Rav 1999, 29).

Our question was: how can one's mathematical knowledge be extended if one's reasoning is deductive? If we assume with Kant that deductive reasoning cannot extend one's knowledge but only make explicit something that was implicit already in one's starting points, then there are only two possibilities. Either reasoning in mathematics is strictly deductive and hence cannot materially extend one's knowledge, or reasoning in mathematics can extend one's knowledge because it is not strictly deductive, because it involves (in either or both of the ways just outlined) modes of inference that are peculiar to the discipline of mathematics. But we can also question Kant's claim that by logic alone there can be no extension of knowledge. Perhaps one *can* extend one's knowledge by purely logical, deductive reasoning from concepts. Because Kant provides two different characterizations of the analytic/synthetic distinction (aside from it as a distinction between what is explicative and what is ampliative, which is just what is at issue) there would seem to be again in this case two ways to develop the point. One might argue, first, that Kant was wrong to have thought that analytic judgments, by means of reason alone, are inevitably explicative because there can be judgments that are by means of reason alone but in which the predicate is not contained already in the concept of the subject. According to this approach, Kant was right about what logic is but wrong about what it can achieve. Alternatively, one could argue that Kant was wrong to have thought that by reason alone only analytic judgments are possible because there are purely logical ways to "go outside my concept," ways that do not involve any reference to any objects. On this view, Kant was wrong about what logic can achieve *because* he was wrong about the nature of logical reasoning.

According to Kant, the analytic/synthetic distinction exactly lines up both with the explicative/ampliative distinction and with the by-logic-alone/involving-objects distinction: analytic judgments are known by reason alone and are merely explicative, and synthetic judgments are ampliative *because* reference to an object (or its a priori form) serves in such judgments to connect the predicate to the concept of the subject. Perhaps what the new mathematical practice of reasoning from concepts shows is that this picture of the analytic/synthetic distinction is wrong. What it cannot settle is how the picture is wrong. The logical positivists drew our first conclusion. They took it that what modern mathematical practice shows is that some judgments that are known on the basis of meanings by logic and reasoning alone, and hence are analytic, are nonetheless ampliative, not merely explicative, because the predicate in such judgments is not contained already in the concept of the subject. Peirce drew the second conclusion. According to him, the lesson of modern mathematical practice is that even reasoning from concepts by logic alone can involve something like a construction and so is, or at least can be, synthetic, hence ampliative. As we can think of it, although for Kant reasoning that is ampliative involves thereby constructions, and so also reference to objects (or at least forms of objects), the positivist argues that there can be ampliative reasoning without constructions (and so without reference to any objects, or forms of objects). Peirce instead accepts Kant's first move, that ampliative reasoning is constructive, but denies the second, the idea that constructions inevitably involve reference to objects or forms of objects.

According to the positivists, developments in mathematics in the nineteenth century showed that there can be judgments that are true in virtue of the meanings stipulated in one's axioms, and so analytic a priori, but which are far from trivial, which can be proven only from a whole collection of axioms in what is perhaps a quite complex series of steps. Although concepts do often contain contents that can be made explicit in trivial analytic judgments, concepts can also acquire meaning through their logical relations one to another, relations that can be made explicit in an axiomatization. Theorems that can be derived from a set of such axioms, solely in virtue of the stipulated logical relations, thus follow "by meaning alone" and so are analytic in Kant's sense; but they are also, the positivists argued, ampliative, that is, significant extensions of our knowledge. Where Kant went wrong, on this view, was in thinking that there is no difference between what is true by (explicit) definition, and hence trivial, merely explicative, and what is true in virtue of meaning, or on the basis of concepts (as articulated in axioms), which can be ampliative despite being derived on the basis of logic alone.[41]

For the positivists, mathematical knowledge can be ampliative despite being analytic because not all meaning is given by an explicit definition. Meaning can also be determined for a collection of expressions, assumed to be otherwise meaningless, through an axiomatization involving those expressions. What is deduced

[41] Coffa (1991) associates Bolzano with this positivist line of argument. It is not clear that this is warranted.

from such an axiomatization is analytic, by reason alone, but also ampliative, that is, knowledge properly speaking. As already indicated, Peirce's response to nineteenth-century developments in mathematics was very different. His thought was not that what those developments show is that constructions are not needed in mathematics but instead that even logic, even reasoning from concepts alone, involves constructions. He explains in "The Logic of Mathematics in Relation to Education":

> Kant is entirely right in saying that, in drawing those consequences, the mathematician uses what, in geometry, is called a 'construction', or in general a diagram, or visual array of characters or lines. Such a construction is formed according to a precept furnished by the hypothesis. Being formed, the construction is submitted to the scrutiny of observation, and new relations are discovered among its parts, not stated in the precept by which it was formed, and are found, by a little mental experimentation, to be such that they will always be present in such a construction. Thus the necessary reasoning of mathematics is performed by means of observation and experiment, and its necessary character is due simply to the circumstance that the subject of this observation and experiment is a diagram of our own creation, the condition of whose being we know all about.
>
> But Kant . . . fell into error in supposing that mathematical and philosophical necessary reasoning are distinguished by the circumstance that the former uses constructions. This is not true. All necessary reasoning whatsoever proceeds by constructions. (Peirce 1933a, 350)

On Peirce's view, the lesson of the developments in mathematical practice in the nineteenth century is not that mathematics, which can involve reasoning from concepts alone, is for that reason analytic, rather than synthetic as Kant would have thought, nor even that it is ampliative despite being analytic as the positivists argued, but instead that reasoning from concepts alone is, like earlier forms of mathematical practice, synthetic, that is, ampliative (and fallible), albeit necessary (at least when we get things right), because even deductive reasoning can involve constructions. Whereas positivists focus on the starting points of deductive proof to explain their capacity to extend our knowledge, Peirce in this way focuses on the *activity* of (deductively) proving, arguing that such activity can realize significantly new results.

Both the positivists and Peirce sever the connection between deductive reasoning from concepts alone and mere explication that one finds in Kant. But they do so in quite different ways. The positivist argues directly: given that not all reasoning from concepts is merely explicative of their contents, but can be on the basis of a whole collection of axioms that only implicitly define the non-logical concepts involved in those axioms, reasoning from concepts can be at once analytic, by logic alone, and ampliative, a real extension of our knowledge. Peirce's response is different insofar as he severs the Kantian connection between reasoning from concepts and explication only indirectly, by severing the Kantian connection between the activity of construction and intuition of objects. Again, on Peirce's view, even deductive reasoning from concepts involves constructions and so is, or at least can be, ampliative. Indeed, even pure logic is not, Peirce thinks, merely formal but is instead a science, that is, "experimental," and hence inherently fallible.

The possibility of Peirce's response, in addition to the positivist one, has not been sufficiently recognized. Coffa, for instance, seems simply to assume that what the positivists, following on from Hilbert's formalist project in geometry (as they understood it), were claiming about the possibility of knowledge through proof just is what Bolzano, Dedekind, Frege, and others had in mind. But although Bolzano's "old dream" of knowledge by purely conceptual means can be pursued along positivist lines, it need not be. It can also be pursued in line with Peirce's suggestion that reasoning from concepts is ampliative, despite being deductive, because even reasoning from concepts is in some way constructive. And we need to recognize this because, as Quine showed, the positivist response is fatally flawed.

We have seen that according to the positivist the truths of mathematics are ampliative despite being analytic; they are contentful, non-trivial truths that, like any analytic truths, are known by appeal to meanings alone. Quine shows that this cannot be right because if the judgments of mathematics really are analytic, that is, founded on meaning alone, hence incorrigible and unrevisable, then they are not and cannot be true.[42] Alternatively, if they are true (or false), then they are not founded on meaning alone because in that case they can be revised as needed. No (putative) truth is or can be immune to revision, because if it really is *impossible* to get it wrong (save merely by making a mistake in one's formal reasoning, in one's manipulation of signs according to rules), then there is no objective or truth-evaluable content to the claim at all. This is most obviously true of simple analytic judgments such as that all humans are rational. If that judgment is true by virtue of meaning, because being human is by definition being (say) a rational animal, then the judgment that all humans are rational is not a piece of knowledge of any kind. It is nothing more than a substitution instance of the logical schema 'all (A & B) is A', and this substitution instance must not be confused with a truth, however trivial, about humans; for if it were really a *truth* about humans then it would be possible that it be mistaken. And the point holds equally of a theorem that is deduced from axioms. Insofar as it is not possible that one is mistaken (except in the trivial sense of having misapplied some rule governing one's use of signs), it is also not possible that what one has is an item of knowledge.

This point can be hard to see, and it can be hard to see at least in part because there is a perfectly good distinction to be drawn between claims that are believed on the basis of meanings and claims that are believed on the basis of fact. I might, for example, believe that humans are rational on the grounds that in the language that I speak part of what it means to be human is to be rational. Quine's essentially pragmatist point is that either sort of belief, either belief on the basis of meanings or

[42] Quine's critique first appeared in Quine (1937); the most famous and influential formulation is in his (1951).

belief on the basis of fact, can turn out to have been mistaken and require revision. Again, there is no *truth* in virtue of meaning, nothing that is (as Sellars would say) Given, unrevisable in principle.[43] The positivist's idea, that the practice of proof can realize mathematical knowledge because some analytic judgments, true in virtue of meanings, are ampliative, cannot be made to work. There neither is nor can be any *truth* in virtue of meaning.

What then of Peirce's idea that even deductive reasoning can involve constructions and hence be ampliative, albeit necessary? Can the problem of mathematical truth be made to yield to this approach?

Kant's account of the practice of mathematics, in particular, his idea of constructions, provides, at least in certain respects, a very plausible explanation of how and why the practice of mathematics—as it was in Kant's day—*works*, how the mathematician's play with symbols and diagrams enables the demonstration of new truths. Kant saw, as no one before him had, that both Euclidean geometry, and arithmetic and algebra essentially involve systems of marks (that is, symbols or drawn lines) that involve three levels of articulation, and that it is by re-conceptualizing at the second, intermediate level various collections of signs in the display, in an ordered, stepwise fashion, that the truth emerges with the proof.[44] The problem for Kant's account is that since the nineteenth-century mathematics has come to seem to be discursive rather than intuitive in Kant's sense. It does not use any specially devised system of marks in its demonstrations, and so, it seems, Kant's philosophy of mathematics is simply irrelevant to this new practice. We are left with no account of how and why modern mathematics works. Modern mathematics, at least according to its early practitioners, is a kind of thinking in concepts, but given Kant's division of the uses of reason into discursive and intuitive it seems impossible so much as to conceive how such a practice might enable the demonstration of new truths.

Peirce rejects the division. According to him, as we have seen, "all necessary reasoning whatsoever proceeds by constructions." This is quite implausible for the case of natural language reasoning; one does not need to have so much as the idea of writing or of notation generally in order to reason in everyday life. But the case of mathematical reasoning seems to be different (though it has to be admitted that mathematics as it came to be practiced in the nineteenth century is also not essentially written). Much as we can think of Kant's notion of construction as extending the domain of Euclidean diagrammatic reasoning, which in many cases provides *explicit* constructions, to include also computations and calculations in

[43] Of course, at any point much that we believe will be held fixed. And indeed at various points we ourselves will be unable to find any reason at all to doubt various claims, for instance, of logic. It does not follow that such claims are indubitable in principle. For example, given his conception of what a number is, Aristotle took it to be obvious that one is not a number—because it is not a collection of units. We, with our very different conception of what a number is, take it to be obvious that one *is* a number (like any other).

[44] We have, in fact, not yet seen this explicitly for the case of algebraic reasoning but will do in section 6.5 when we consider the proof of Euler's Theorem.

arithmetic and algebra, so we can think of Peirce's suggestion as aimed at extending Kant's notion of construction to include also reasoning from concepts in mathematics. We know that Peirce came to think that an adequate understanding of reasoning in mathematics requires not an *algebra* of logic but instead a more diagrammatic notation such as he aims to provide in his system of existential graphs. As he explains in an essay introducing his system of existential graphs written around 1903, "our purpose... is to study the workings of necessary inference. What we want, in order to do this, is a method of representing diagrammatically any possible set of premisses, this diagram to be such that we can observe the transformation of these premisses into the conclusion by a series of steps each of the utmost possible simplicity" (Peirce 1933b, 346). Peirce's thought seems to be that if we had such a notation we could *see*, as Kant helped us to see for earlier forms of mathematical practice, how reasoning from concepts alone could be ampliative. If Peirce were right, and assuming that such a use of constructions is distinctive of mathematical reasoning, as Kant had thought, then it would follow that Poincaré was right as well, that there are distinctively mathematical modes of reasoning—even in the case in which the reasoning is strictly deductive, that is, truth preserving.

We have canvassed four possible philosophical responses to the problem of truth and knowledge as it arises for the practice of *Denken in Begriffen* that emerged in mathematics in the nineteenth century and remains standard today. None are wholly satisfactory. Although it *appears* to be the case that by reasoning deductively from concepts mathematicians such as Riemann, Dedekind, and others discover significant new mathematical truths, we do not yet have any adequate way of thinking about this. Nonetheless, there has been progress insofar as Poincaré and Peirce, both of whom focus on the *activity* of the mathematician, seem less obviously to be wrong than either the positivists or those who deny that mathematicians ever achieve anything new at all. Poincaré does accept Kant's account of logical inference; what he argues is that mathematical inference is different from logical inference. Peirce instead takes Kant to be wrong about the nature of logical inference. Even logical inference is, he thinks, more like what Kant thinks of as distinctively mathematical, constructive inference. This seems bizarre. In so seeming, it highlights just how profound are the difficulties in thinking about this new form of mathematical practice of reasoning deductively from concepts that became the norm in the nineteenth century.

5.5 Conclusion

"Mathematics," Grabiner (1974, 364) remarks, "is that area of human activity which has at once the least destructive and still the most fundamental revolutions." Nowhere is this more evident than in the nineteenth-century revolution in mathematical practice, the revolution that was, as Stein describes it, a second birth of the subject. Mathematics was not merely to be done in a new and better way, as Descartes had shown with his method that it could be. It was to be raised anew on purely conceptual foundations by means of reason alone.[45] What Kant had declared impossible was now enacted. Indeed, it almost seems in retrospect, that Kant had only to make explicit that early modern mathematics, like its ancient counterpart, constitutively involves something we can call pure intuition for mathematicians to rise up and prove him wrong.

The revolution, I have suggested, unfolded in two distinguishable waves. First, from reasoning in the symbolic language of arithmetic and algebra, mathematicians began self-consciously to reason on expressions in that language—something eighteenth-century mathematicians such as Euler had in fact already been doing, albeit quite unselfconsciously and unsystematically. Indeed, one can find this sort of reasoning here and there already in Descartes' *Geometry*. The symbolism is not only a medium within which to discover new results as Euclid's diagrams are; its equations can also exhibit discernible patterns that can be exploited in the course of mathematical inquiry. At the second stage of the revolution, somewhat ironically, all appeals to symbolisms were to be eschewed. The symbolic language that in the seventeenth and eighteenth centuries had opened up a whole new world of mathematics (and physics) was now to be regarded as providing a merely contingent and external perspective on what now appeared to be the *real* subject matter of mathematics. What had seemed in the eighteenth century to be the thing itself, namely, the formula, had been revealed to be only its outer clothing. What matters is not the formula through which we first gain cognitive access to a function but the *function* and, in particular, the intrinsic properties in virtue of which it is the function it is.

Kant's critical philosophy, although not a prerequisite of the revolution, nevertheless profoundly influenced the articulation both of the nature and fatal flaws of the old regime and of the fundamental character and constitution of the new. Not Kantian pure intuition but Kantian ideas, that is, concepts of reason conceived as intelligible unities, are now to be seen as what mathematics is *really* about. Finally, after over twenty-five hundred years of growth and development, mathematics has, so it seems, become a self-standing discipline, the work of pure reason wholly unfettered by the contingencies of our form of sensibility. *But how can this be?* Surely mathematics needs to answer to something independent of its own activity, and if it does not answer to anything independent of its own activity then it really is, as since the nineteenth century it has so often been taken to be, nothing more than a mere game. How, by reason alone, is it possible to know anything at all?

[45] In section 3.1 we saw that already in the sixteenth century there was a call for a new beginning, what Bacon thought of as a "great instauration," a total reconstruction of all (empirical) knowledge on an adequate foundation. Descartes' new form of mathematics, as we also saw, was critical to the realization of that vision. Interestingly, no one seems to have thought that Descartes' new form of mathematics was likewise a new beginning. It was not Descartes' mathematics in the seventeenth century but instead the new form of mathematics that emerged in the nineteenth century that had the significance of being a new beginning, a starting anew on adequate foundations.

The straightforwardly Kantian answer is, of course, that it is *not* possible to know anything by reason alone. There can be no ampliative deductive proofs on Kant's conception of deduction. But perhaps Kant was mistaken about this. He had been wrong about the role of pure intuition—not, perhaps, its role in ancient and early modern mathematics, but nonetheless wrong about its role in mathematics as it had by now revealed itself to be. Perhaps, then, he was wrong as well about the power of reason. If, as Coffa (1991, 140) remarks,

> Kant was right, concepts without intuitions are empty, and no geometric derivation is possible that does not appeal to intuition. But by the end of the nineteenth century, Bolzano, Helmholtz, Frege, Dedekind, and many others had helped determine that Kant was not right, that concepts without intuitions are not empty at all. The formalist project in geometry [that is, Hilbert's project] was therefore designed not to expel meaning from science but to realize Bolzano's old dream: the formulation of non-empirical scientific knowledge on a purely conceptual basis.

Although one perhaps *begins* with an intuitive grasp of a domain, say, geometry or arithmetic or analysis, that grasp can be refined through a careful analysis of the concepts involved and strictly deductive proofs of the theorems of that domain. Such proofs, it seemed clear, *can* extend our knowledge, despite being (apparently) purely deductive. Because in Kant's logic the very idea of a deductive proof that is ampliative is manifestly incoherent perhaps what is needed is *a new sort of logic*, a pure logic that is universally valid (as Kant's general logic is) but not merely formal, devoid of all content (as Kant's general logic also is). Natorp, a Marburg neo-Kantian, describes such a hoped-for logic in "On the Question of Logical Method":

> the fundamental problem of logic then concerns the conditions of possibility of science in general, of theory in general, of truth in general, of deductive unity—the necessary generalisation of the Kantian question regarding the conditions of the possibility of experience, for this meant . . . the unity of objective lawfulness. . . . The question therefore arises . . . what are the primitive 'essential concepts' from which the essential concept of theory itself is constituted. All logical justification of our concepts must go back to this ultimate foundation. Pure logic then is the theory of theories, the science of sciences. (Natorp 1901, 61–2)

Neither Kant's general logic, which is merely formal, devoid of all content and truth, nor his transcendental logic, which is a logic of truth but not maximally general, will do. What is needed instead is a pure logic that is universally valid without being merely formal. Such a logic, and only such a logic, Natorp thought, can provide what is wanted, the science of truth.

Natorp calls for a logic that is at once universally valid and contentful. This is not, we will soon see, the logic that in fact came to be developed. The logic that would be developed, building on the work of Boole in the mid-nineteenth century and culminating in Tarski's work in the twentieth, is not a science of truth. Instead, as we will see in some detail in the next chapter, that logic is largely an extension of familiar

Kantian themes, just as Russell suggests in *The Principles of Mathematics*. It is more powerful than Kant's monadic predicate logic but otherwise largely Kantian. Little wonder, then, that that logic has, despite a century of effort on the part of some of our ablest philosophers, yielded only very meager insights into the practice of mathematics.[46]

[46] A very striking expression of the failure of our logics to clarify the nature of mathematical reasoning can be found in works in the maverick tradition in recent philosophy of mathematics, for instance, by Kitcher and Aspray (1988) and Tymoczko, ed. (1986), both of whom regard mathematical logic as wholly inadequate to the task of developing a satisfactory philosophical understanding of the practice of mathematics.

6

Mathematics and Language

How does mathematics work? The answer of the mathematical logician is that it works by drawing deductively valid inferences from axioms: a mathematical proof is a sequence of sentences each of which is either an axiom or a consequence of earlier sentences according to some rule of logic.[1] Suppes' *Introduction to Logic* provides a classic exposition of the view: "We begin with a set of formulas which we call *premises*. The object . . . is to apply the rules so as to obtain . . . the sought for conclusion" (Suppes 1957, 20). Because logic provides "a completely explicit theory of inference adequate to deal with all the standard examples of deductive reasoning in mathematics" (1957, xii), any mathematician's proof can be reworked into such a fully formal proof. The only difference between the proofs that mathematicians give in their actual practice and a fully formalized proof in the symbolic language of mathematical logic, on this view, is that the mathematician's proof does not lay out every step of the reasoning but instead makes jumps: "in an informal proof enough of the argument is stated to permit anyone conversant with the subject to follow the line of thought with a relatively high degree of clarity and ease . . . in giving an informal proof, we try to cover the essential, unfamiliar, unobvious steps and omit the trivial and routine inferences" (Suppes 1957, 128).

Surprisingly, this seems not to be right. First, mathematicians do not in their actual practice give proofs, even informal ones, in Suppes' sense, but instead provide *descriptions* of how the reasoning goes, or would go were one to engage in it.[2] Furthermore, and even more puzzling, "fully formalized proofs are usually unintelligible" (Manders 1987, 202). "If written out in full they [fully formalized proofs] are difficult to comprehend, and despite their rigor they are often *unconvincing*, because although they provide *verification* that a result follows logically from given premises, they may fail to convey *understanding* of *why* it does" (Dawson 2006, 271). We saw in section 5.4 that Poincaré goes a step further: a mathematician's proof cannot, he thinks, be formalized at all.[3] Somehow the act of formalization turns a proof—that is, a real proof, the sort of proof a mathematician actually gives—into a non-proof.

[1] This is sometimes referred to as Hilbert's thesis.

[2] See especially Avigad (2006b), which will be examined in more detail in section 6.4. Azzouni's (2006) "derivation-indicator view of mathematical practice" is based on this same phenomenon.

[3] This is also, we saw, the view of Rav (1999).

What can possibly be going on here? It is barely conceivable that the norms governing the reasoning mathematicians actually engage in are simply disjoint from the familiar rules of logical inference. Even if Poincaré were right to think that there are distinctively mathematical modes of inference—either primitive relations of entailment among mathematical concepts or rules of inference that, although they can be broken down into a series of purely logical steps, nonetheless have a characteristic unity in mathematical practice—logic nevertheless ought to be of *some* use in mathematics, for instance, in what Detlefsen (1992, 367) describes as "times of epistemic crisis." But it is not. The mathematical logicians' formalizations are of *absolutely no use* in actual mathematical practice: they do not convince, or even clarify, and most often they are simply unintelligible. We need to understand this.

We have seen that the profound transformations in mathematical practice over the course of the nineteenth century showed that Kant's philosophy of mathematics in terms of the construction of concepts in pure intuition as made possible by space and time as the forms of sensibility was fatally flawed. What those developments seemed further to suggest, given the enormous and pervasive influence of Kant, was that it is not reason in its intuitive use but instead reason in its discursive use that is at work in mathematics, that there are not two uses of reason, as Kant had thought, but only one. Suppes takes just this view: "A correct piece of reasoning, whether in mathematics, physics, or casual conversation, is valid by virtue of its logical form" (1957, xii).[4] But, as Suppes immediately goes on,

> because most arguments are expressed in ordinary language with the addition of a few technical symbols particular to the discipline at hand, the logical form of the argument is not transparent. Fortunately, this logical structure may be laid bare by isolating a small number of key words and phrases like 'and', 'not', 'every' and 'some'. In order to fix upon these central expressions and to lay down explicit rules of inference depending on their occurrence, one of our first steps shall be to introduce logical symbols for them. With the aid of these symbols it is relatively easy to state and apply rules of valid inference.

The way the mathematician reasons, Suppes thinks, is the way anyone else reasons, whether in everyday life or in, say, philosophy or courts of law; reasoning is everywhere the same, and it is performed, or at least expressed, in natural language. But because natural language can mask the underlying logical form of a sentence, the nature of this process of reasoning is to be laid bare by introducing abbreviations, simple signs for the logical particle of natural language ('and', 'or', 'all', 'some', and so

[4] Even Frege seems to have thought that his mathematical work was of general significance for logic and language; he does not seem to have drawn any sharp or principled distinction between reasoning in natural language and reasoning in mathematics. It is nonetheless very hard to imagine that he intended the strange formulae of his two-dimensional notation to be read as nothing more than translations or abbreviations of sentences of natural language, that is, in the way that, we assume, a (linear) sentence written in the standard signs of mathematical logic is a translation or abbreviation of a sentence of natural language.

on, or perhaps slightly regimented versions of such words) together with explicit rules governing what may be deduced from sentences containing such signs.

Although Kant had held that mathematical reasoning is essentially different from reasoning in other contexts, developments in mathematics in the nineteenth century seemed to show that it is not, that reasoning is everywhere the same. But although the mathematical logician diverges from Kant on this point, in other respects the conception of logic and language that we find in mathematical logic remains fundamentally Kantian.[5] Logic, it is thought, is merely formal, without content or truth. And because it is, it seems natural to adopt a model theoretic conception of language according to which there is an absolute distinction to be drawn between, on the one hand, logical form, which is displayed in the uninterpreted formal language, and on the other, the various models or interpretations relative to which sentences of the language have content and are true or false. And because all content and so all truth is taken, following Kant, to lie in relation to an object or objects, generality must be understood quantificationally, in terms of the idea of quantifying over a domain of objects. If all content (and so all truth) lies in relation to objects then there obviously cannot be truths directly about concepts, say, about their logical relations one to another. Although in their mathematical practice mathematicians *appear* to establish such truths, really (so it is thought) mathematics is about objects, at least if it has any content. The failure of Frege's logicism showed that its objects cannot be purely logical; so, it is assumed, there must be other objects that mathematics is about—either that or it really is merely formal, not a science at all, nothing more than a play with empty signs. Almost all work in twentieth-century philosophy of mathematics is founded on just this assumption.

According to the standard view, a mathematical proof is, as any piece of reasoning is, a set of sentences some of which are premises and the remainder of which are derived from those premises according to logically valid rules of inference. Generality is understood quantificationally; language is understood model theoretically; and meaning is understood in terms of truth, what is the case with objects. The task of the first half of this chapter is to examine these last three ideas. We have already seen well enough how the notion of a quantifier arises in the context of Kant's philosophy. Because Kant thinks that concepts are not object involving (as Descartes' ideas are not) but that nonetheless *judgment* must be object involving, he adopts a quantificational conception of generality. Our concern here will be with the very idea of a quantifier, and in particular with some unresolved debates about how to think about them. Model theory as an account of how language in general works is different insofar as it is not a particularly Kantian idea but seems instead to have arisen, at least in part, in response to the developments in mathematics in the nineteenth century.

[5] Again, this is all but explicit in Russell's view that Kant needed to appeal to constructions in his account of mathematics because his logic was merely monadic—but otherwise essentially the same as logic as Russell understood it.

But Kantian ideas are not far away. If one is antecedently committed to a quantificational conception of the generality of, say, the axioms of elementary algebra, then when one realizes that those axioms can be applied also in quite different sorts of cases from those originally intended, it will seem natural to adopt a model theoretic understanding of those axioms—especially if one is also inclined to think of reasoning mechanistically, as a matter of the rule governed manipulation of meaningless signs. We will see that independent of such commitments that conception of language has little to recommend it, that a quite different view of the significance of the relevant developments in mathematics in the nineteenth century is at least as plausible. That meaning as it matters to logic and reasoning in mathematics should be understood in terms of truth is, we will see, also problematic.

According to the received view, reasoning is everywhere the same, and all that logic, as least insofar as it is the study of valid forms of inference, does is to make explicit what the rules are that govern reasoning. Hence, it is assumed, one reasons in natural language in mathematics; any special signs that are used function merely as abbreviations. But this is far from obvious and in fact seems manifestly false of most of mathematics, for most of its history. The fourth section explicitly addresses the question of the role of writing in mathematics. Four positions are outlined: the Kantian view that mathematics is constitutively written in a way that natural language is not; the "documentist" view that language is inevitably after the fact in mathematical practice, serving only to report results achieved independently of it; the view of the mathematical logician that mathematics is done in natural language (or some shorthand version of it); and finally Jourdain's view, which, we will see, combines aspects of both of the first two. Current mathematical practice (we will see) strongly suggests that Jourdain's view of the role of writing in the practice of mathematics is correct. But if it is then although current mathematical practice—by contrast with both Euclidean diagrammatic practice and the practice of seventeenth- and eighteenth-century mathematicians such as Descartes, Euler, and Gauss—has no specially devised system of signs within which to work, we can nonetheless ask what would be required of such a system of written signs. What would a Leibnizian universal language, designed for the current mathematical practice of reasoning from concepts, need to be able to do? Taking our cue from the symbolic language of arithmetic and algebra, and from aspects of the way interesting new results such as Euler's Theorem are derived in that symbolic language, the fifth section aims to provide an answer.

6.1 Quantifiers in Mathematical Logic

Natural language, we have seen, is inherently object involving; we speak first and foremost of perceptible things that matter to us. Mathematics is not (at least directly) about such things and after Descartes it is not even ostensibly about objects at all. Cartesian ideas, paradigmatically those of mathematics, are not, as everyday concepts are, constitutively object involving. Kant inherits this Cartesian conception: Kantian concepts are also not constitutively object involving. But, Kant thinks, at least *judgment* must be object involving. One very obvious way to combine Kant's idea that concepts do not give objects but are only that through which objects are thought with the idea that for thought to have content is for it to be in relation to an object is with a quantifier, where a quantifier just is a means of referring to objects in general sentences. We read, for instance in the *Jäsche Logic* (1800, 607; AK 9:111), that an example of an analytic judgment is this: "To everything x, to which the concept of body $(a + b)$ belongs, belongs also *extension* (b)." Kant clearly does not think of a sentence such as that all bodies are extended as predicating of bodies, all of them, that they are extended but instead introduces something that appears to be very like a quantifier to provide something, objects, for the conditional predicate extended-if-a-body to be about. Because all content and so all truth lies in relation to an object on Kant's account, the meaning of a general sentence, whether in mathematics or in everyday conversation, is to be understood in terms of what is the case with objects if it is true. This constraint, that one's account of generality is to apply indiscriminately to our everyday talk about ordinary things in the environment and to the inherently general (and a priori, necessary) judgments of mathematics, generates, we will see, enormous technical and philosophical difficulties.[6]

Frege, one is often told, is the founder of modern logic; it was Frege, we are told, who first discovered the quantifier that we employ in mathematical logic. This is wrong on two counts. First, as we will see in more detail in Chapter 7, Frege's notion of generality is not that of quantificational logic but instead a notion designed specifically for the needs of mathematics, and in particular the needs of the nineteenth-century practice of reasoning deductively from concepts. But even if Frege had developed the notion of a quantifier that Peirce (together with his student Mitchell) introduced in 1883, it was Peirce's notion, not Frege's, that as a matter of historical fact we have inherited. It was Peirce, not Frege, who, as Putnam (1982, 297) argues, "seems to have been known to the entire world logic community": "first-order logic (and its meta-mathematical study) would have existed without Frege." "Although [Putnam thinks] Frege discovered the quantifier in 1879 and Peirce's student Mitchell independently discovered it only in 1883, it was Mitchell's discovery (as modified and disseminated by Peirce) that made the quantifier part of logic" (Putnam 1982, 290). Quine (1995, 24) makes the same point: "Peirce, not Frege, was indeed the founding father [of quantification]; for Peirce's influence was continuous through Schröder's work, with side channels into Peano, and culminating in *Principia Mathematica*."

We can trace out a chain of development of logic from Boole up through De Morgan, Mitchell, Peirce, Schröder, Peano, Russell, and Whitehead, to logic in its modern estate. Where the

[6] It also begins to explain why it took so many decades, and the work of many, many brilliant minds, finally to achieve a fully technically adequate conception of the quantifiers.

inception of mathematical logic comes in this chain is in 1883, when quantification becomes clearly articulated by Charles Sanders Peirce. Even the terms 'quantifier' and 'quantification', thus applied, are his. (Quine 1995, 23)

It is also interesting to note, as Quine does, how arduous is the path to the notion of a quantifier in Peirce, although what is ostensibly that same notion seems to spring "full-grown" from Frege in his *Begriffsschrift*. This would be much less surprising if they were articulating quite different notions, as in fact they are. The idea that Frege is the founder of quantificational logic is simply false. It is due, at least in part, to Russell's (sometimes deliberate) falsification of the history of logic.[7]

The basic idea of quantification is very simple. Much as 'Fa' is true just in case the object denoted by the letter 'a' has the property F, so '(∀x)Fx' is true just in case all objects—more exactly, as we have to say to avoid falling into a contradiction, all objects in the domain of quantification—have the property F and '(∃x)Fx' is true just in case at least one object (in the domain of quantification) has that property. But, although the basic idea is very simple, and appears already in Kant's late lectures on logic, working it up into a theory powerful enough to deal with all cases would be achieved by Tarski only in 1933, fifty years after its first official introduction by Peirce. Nor were there only technical difficulties. Philosophical debates about the precise nature of the quantifiers also sprang up; and although they no longer command any interest, this is not because they have been resolved but instead because they have come to seem unresolvable.

Natural language is exceptionally versatile; anything that can be said can be said, one way or another, in natural language. But some things, in particular, truths of mathematics and logic, seem most aptly expressed in written language using the literal notation. For example, we use letters in logic to express the law of identity, that $a = a$, though we can also express that thought in English as that everything is self-identical. We similarly use letters in arithmetic to express, say, the commutativity of addition, that $a + b = b + a$, though this too can be expressed in English, most naturally, I think, as that the operation of addition is commutative, that is, as ascribing a property to the arithmetical operation of addition. Numbers do not seem to come into it at all, any more than any objects are referred to when one rehearses the law of identity, that $a = a$. Accidental generalities are very different. If I claim that every saucer in the cupboard is chipped, to use an example of Dummett's (1973, 514), then my claim is true just in

[7] See Anellis (1995) for an extensive review of available data. His conclusion is "that Russell would deliberately distort the history of logic in order to play up his own role in that history—or even for greed—while attempting to cover his tracks in case anyone . . . might otherwise find him out" (Anellis 1995, 313–14). van Heijenoort—in particular his (1967) as well as his extremely influential *From Frege to Gödel: A Source Book in Mathematical Logic 1879–1931*, which appeared also in 1967—is also to be blamed. As Peckhaus (2004, 7) notes, "van Heijenoort simply ignored the extensions and revisions of the Boolean calculus due to Charles S. Peirce and his school, and to the German algebraist Ernst Schröder, whose work had, unlike Frege's logic, a decisive influence on early 20th century developments. . . . These shortcomings have clearly been recognized by van Heijenoort's critics."

case each and every saucer in the relevant cupboard is chipped. The claim that, say, all cats are mammals seems to be different again insofar as we take it to be necessary that cats are mammals. Whereas an accidental, merely contingent generality such as that every saucer in the cupboard is chipped authorizes only what Sellars (1958, 297) has called a subjunctive identical, that if anything were (identical with) one of the saucers in the cupboard then it would be chipped, a lawful generality authorizes a subjunctive conditional. If there is a lawful connection between being a cat and being a mammal then it is true that if anything were a cat then it would be a mammal. As the point might be put (perhaps misleadingly), in this case, by contrast with the case involving the saucers, we are talking not merely about all the actual cats but about all possible cats, all the cats there are or could be. Mathematical logic treats all these sorts of cases in the same way; in particular, it treats a generality in mathematics such as that $a + b = b + a$ in just the same way that it treats an empirical, merely accidental generality such as that every saucer in the cupboard is chipped. Both are taken to involve a universal quantifier and so to refer (somehow, accounts differ) to objects. And as already indicated, it is easy to understand how, given the developments in mathematics in the nineteenth century and the enormous and pervasive influence of Kant, this might have seemed the right way to go. What we need now to see is how the very different demands of, on the one hand, the empirical, merely accidental or contingent case, and on the other, that of mathematics, pull in different directions.

We saw in section 4.2 that Euler and Venn diagrams encode two very different conceptions of judgment. Euler diagrams encode a pre-modern conception according to which judgment is a matter of predicating of objects assumed already given that they (some, all, or none) are thus and so. On this conception of judgment, which is quite natural in our everyday understanding, one can predicate only of something that actually exists. If there are no Ss then one can make no judgment of the form 'S is P', which is why it is valid to infer, in Aristotle's logic, that some S is (not) P given that all (no) S is P. Venn diagrams (we saw) function very differently; they encode instead a Kantian conception according to which judgment acts instead on two concepts that are not presumed to have any instances but are conceived merely as predicates of possible judgment, and relates them to objects by way of something like a quantifier, that all objects are P if S (that is, that nothing is S and not P), that some object is both S and P, and so on. These two conceptions of judgment give rise in turn to two very different intuitions about quantifiers, the intuition that they are inherently restricted and the intuition that they are inherently unrestricted.

According to the defender of restricted quantification, quantifiers function as determiners enabling the expression of thoughts that are about all, or some, or exactly *n*, objects of a certain kind. (Notice that, on the most natural reading of this idea, it seems to require that concepts themselves are, as on the ancient conception, already object involving: even when the answer is 'none' it would seem to be about some objects in particular.) As Brandom has put it, "quantifiers quantify; they specify, at least in general terms, *how many*" (Brandom 1994, 439). As Brandom immediately goes on, the question 'how many?', however, "depends (as Frege's remarks about playing cards indicate) on *what* one is counting—on the sortal used to identify and individuate them." Now in *Grundlagen*, Frege is talking not about generality but about numbers.

If I give someone a stone with the words: Find the weight of this, I have given him precisely the object he is to investigate. But if I place a pile of playing cards in his hands with the words: Find the Number of these, this does not tell him whether I wish to know the number of cards, or of complete packs of cards, or even say of points in the game of skat. To have given him the pile in his hands is not yet to have given him completely the object he is to investigate; I must add some further word—cards, or packs, or points. (Frege 1884, sec. 22)

On Brandom's view, this point about numbers applies also to quantifiers. Because the role of the quantifier on this account is to determine, in general terms, how many—for example, some, or all, or at least twenty-three, or most, or exactly one—objects of a certain kind have the relevant property, and because the question 'how many?' is meaningless in the absence of a sortal determining what is to be counted, it follows that a quantifier is inherently restricted. Given that it is, "unrestricted quantification [is] a dangerous and often unwarranted extrapolation based on a misunderstanding of the way pseudo- and prosortals such as 'thing', 'object', 'one', and 'item' function" (Brandom 1994, 439). 'Thing', 'object', and so on do not individuate objects; one cannot count things or objects as such. But if quantifiers serve to specify (either in general terms or for a particular number *n*) how many, then unrestricted quantification involves just such pseudo-sortals. The apparently unrestricted quantifier is in fact restricted; it is restricted by a pseudo-sortal such as 'thing' or 'object'. Restricted quantification, then, is logically prior to unrestricted quantification. The latter is to be understood in terms of the former.[8]

It is quite natural, at least in some cases, to think of a general sentence of natural language as answering the question 'how many?' and so to think of the quantifier as a determiner. Question: How many people now in the room are wearing glasses? Answer: All of them.[9] In other cases, the idea is less plausible. The judgment that

[8] Gupta offers a related argument for the conceptual priority of restricted quantifiers. The intelligibility of unrestricted quantification rests, he claims, on the intelligibility of Aristotelian essentialism, that is, on the thesis that for everything there is a unique answer to the question what it most fundamentally is: "if Aristotelian essentialism is meaningful, then unrestricted quantification makes sense and there is (or can be) a use of 'thing' in which it functions as a genuine common noun" (Gupta 1980, 91–2). For Gupta as for Brandom, then, unrestricted quantification can only mean quantification that is restricted by a sortal such as 'thing' or 'object'; and such terms can function as proper sortals, Gupta points out, only if Aristotelian essentialism is true. Since a quantified sentence (as Gupta understands it) is meaningless independent of an associated sortal, the only question that the idea of unrestricted quantification can raise is the question whether 'thing' or 'object' can play the role that is required of it. On this view, all sentences of the form $(\forall x)(Fx \supset Gx)$ or of the form $(\exists x)(Sx \,\&\, Px)$ implicitly involve a sortal, or pseudo-sortal such as 'object' or 'thing'; they in fact have the forms, respectively, $(\forall Ox)(Fx \supset Gx)$ and $(\exists Ox)(Sx \,\&\, Px)$.

[9] Notice that only in certain contexts would this count as a good answer. If one wanted to know, say, how many glasses cases to bring, and did not already know how many people were in the room, it would not help to be told that all were wearing glasses.

all red things are colored, for example, is not easily taken to provide an answer to a 'how many?' question. There is no number that is the number of red things; so it would seem that there is no sense to the question how many red things are colored. And yet, it seems obviously true that all red things are colored. Or consider the mathematical truth that every ordinal has a successor. There is no number that is the number of ordinals, and yet we do know that every ordinal has a successor. Similarly for the case of a logical truth such as that everything is self-identical. Here again, it does not seem plausible to take this truth to involve a determiner and associated sortal, that is, as answering the question 'how many things are self-identical?'. In these cases it is the Venn style depiction of judgment rather than the Euler that seems most natural, and with it a conception of quantifiers as unrestricted.

Russell, in his *Principles*, seems to have the Venn conception in mind, and he does so at least in part because he is thinking not of everyday examples but of mathematical ones. He argues that quantifiers are inherently unrestricted.

> It is customary in mathematics to regard our variables as restricted to certain classes: in Arithmetic, for instance, they are supposed to stand for numbers. But this only means that *if* they stand for numbers, they satisfy some formula, i.e. the hypothesis that they are numbers implies the formula.... Thus in every proposition of pure mathematics, when fully stated, the variables have an absolutely unrestricted field: any conceivable entity may be substituted for any one of our variables without impairing the truth of our proposition. (Russell 1903, 6–7)

The 1906 essay "On 'Insolubilia'" makes the point more generally:

> a statement, such as ϕx, which is true under an hypothesis can only be stated to be true under that hypothesis if the statement that the hypothesis implies ϕx can be made without any limitation on x. Any limitation on x is part of the whole which is really asserted; and as soon as the limitation is explicitly stated, the resulting implication proposition remains true when the limitation is false. Thus a variable must be capable of *all* values. (Russell 1906, 205)

Russell argues that any sortal restriction can be formulated as a condition on the truth of the generality. The sentence '$a(b + c) = ab + ac$', for instance, which is true no matter what numbers are put for 'a', 'b', and 'c', can be expressed unconditionally as 'if a, b, and c are numbers then $a(b + c) = ab + ac$'. This sentence is true no matter what object names are put for 'a', 'b', and 'c'; hence, Russell concludes, "a variable must be capable of *all* values." Whereas Brandom argues that restricted quantification is conceptually basic on the grounds that a quantifier requires an associated sortal, so that unrestricted quantification is actually restricted, restricted by the dummy sortal 'object' or 'thing', Russell argues that unrestricted quantification is conceptually basic on the grounds that any associated sortal can be treated instead as a condition on the truth of the sentence leaving the quantifier wholly unrestricted in its scope.

Thinking of a quantifier as a determiner is especially plausible in the case of an accidental regularity. In the case in which a sentence of the grammatical form 'all (no) S is P' is true in virtue of the chance concurrence of facts, that this S is (not)

P and that S is (not) P and so on for all the Ss there actually are, it is very plausible to take the sentence to have the form of an ascription (or denial) of the predicate P to the Ss, that is, to each and every one of them. For the ground of the truth of 'all (no) S is P' is, in such a case, the truth of the particular instances. But the case of a law, say, a law of algebra, seems essentially different. The thought that $a(b + c) = ab + ac$ is not grounded in arithmetical facts such as that $3(2 + 4) = (3 \times 2) + (3 \times 4)$. Although one might first discover the law by reflection on cases, one does not examine cases to determine that the law is true. In this sort of case the generality seems to be logically prior to the instances (even if, in the order of knowing, it is posterior). Whereas in the empirical case the truth of the generality is grounded in the truth of the instances, in the mathematical case, the reverse would seem to be true: the generality seems to be the ground of the truth of the instances. It is for just this reason that the idea that the quantifier is functioning in this context as a determiner is so implausible, and for the case of a sentence such as that every ordinal has a successor is simply unworkable. The conception of a quantifier as a determiner, while it is clearly cogent for the case of an accidental generality, seems altogether wrong as an account of how quantifiers function in general laws such as the distributive law of elementary algebra or the law that every ordinal has a successor.

The Russellian account, which takes quantifiers to be unrestricted, seems more plausible for the mathematical cases—though of course it will lead to contradiction unless a domain of quantification is antecedently specified. But the Russellian account seems most implausible if applied, say, to the claim that every saucer in the cupboard is chipped. As Russell already saw, to make the extension to the empirical case requires that "in addition to particular facts . . . there are also general facts and existence-facts, that is to say, there are not merely *propositions* of that sort but also *facts* of that sort" (Russell 1918, 234–5). In the case of the saucers, for example, the strategy requires not only facts such as that this saucer, which is in the cupboard, is chipped, and that saucer in the cupboard is chipped, and so on, but also essentially general facts such as that nothing else but this, that, and the other saucer is a saucer in the cupboard. The idea that there are essentially general, and yet merely empirical facts of this sort, contingent facts that are nonetheless not reducible to singular facts about particular objects, is the price Russell must pay to extend his analysis to the empirical case.

Like Frege's question in *Grundlagen* whether the units collected in a number are identical or different, the question whether quantifiers are inherently restricted or inherently unrestricted seems to admit of no clear answer. In our case, just as in the arithmetical case, "each side supports its assertions with arguments that cannot be rejected out of hand" (Frege 1884, iv). If it is assumed both that reasoning is everywhere the same and that even truths of mathematics (such as that every ordinal has a successor) are in some way about objects, that all generality is to be understood quantificationally, then quantifiers must be at once restricted (in order to deal with accidental generalities without resorting to essentially general contingent facts) and also unrestricted (in order to account for the sorts of lawful cases that arise in mathematics). Are quantifiers perhaps, then, *barely* restricted, restricted but only by the pseudo-sortal *object* (as Jevons's units, discussed in Frege's *Grundlagen*, are barely different)? Russell's paradox shows that that way out leads to contradiction. We are left, finally, with an irresolvable dilemma. In mathematical logic as it is currently practiced, we do use restricted quantifiers, that is, we specify a domain of quantification, but we do so in order to avoid paradox, not because we think we understand what quantifiers really are, whether restricted or unrestricted.

A second debate that our quantificational understanding of generality has generated concerns two very different semantic accounts that can be given of quantified generalities, the objectual and the substitutional. According to the objectual interpretation, a universally quantified sentence '(∀x)Fx' is true just in case all objects (in the domain) have the property F, and an existentially quantified sentence '(∃x)Fx' is true just in case at least one does. On the substitutional interpretation, a universally quantified sentence is true just in case every substitution instance is true, and an existentially quantified sentence is true just in case at least one substitution instance is true. An adequate semantics for first-order logic can be given either way. (See Camp 1975 and Kripke 1976.)[10]

On the objectual interpretation, a universally quantified sentence is true just in case all objects in the domain of quantification have the relevant property; on the formally adequate objectualist account due to Tarski, the truth of quantified generalities are understood in particular by appeal to the technical notion of satisfaction. On the substitutional interpretation, quantified generalities are instead understood truth functionally. Universally quantified sentences are to be taken to be logically equivalent to conjunctions, possibly infinite, of instances, and existentially quantified sentences are to be taken to be logically equivalent to disjunctions, possibly infinite, of instances. (See, for example, Dummett (1973, 521), and Bonevac (1985, 243).) On the substitutionalist view, we are to analyze "the indeterminate reference of quantifiers and variables in terms of the determinate reference of names" (Bonevac 1985, 229).

> The truth of quantified sentences will be defined in terms of the truth of their instances; formulas with free variables will receive no interpretation at all. Thus the reference of variables, and in general the quantificational apparatus will depend on the individual constants. (Bonevac 1985, 231)

According to the substitutional interpretation, a quantified generality means the same as a truth-function of singular sentences. Its truth-conditions are just those of a truth-function of its instances.

[10] van Inwagen (1981) argues that we can make sense of the substitutional quantifier only in terms of the objectual. The basic idea is that we know that '(Σx) x is a dog' is true, where '(Σx)' is the existential quantifier interpreted substitutionally, just in case '(∃x) x is a term and ⌈x is a dog⌉' is true, where '(∃x)' is to be interpreted objectually. Because the defender of the substitutional interpretation denies that this is what the quantifier means on the substitutional view, van Inwagen concludes that no one knows what it means. This, we will soon see, is simply wrong.

On the objectual interpretation of a quantified sentence such as '$(\forall x)(Fx \supset Gx)$', the letter '$x$' functions as a variable ranging over the domain of quantification. The sentence is true just in case the sentential function '$Fx \supset Gx$' is satisfied by all objects in the domain. The substitutional reading takes 'x' to function schematically, to indicate the structure of the sentences that go to make up the relevant conjunction or disjunction. Sellars (1948a, 429) puts the point this way:

> if by 'language' is meant a symbolic system in which all individual constants and predicates are explicitly listed without the use of such devices as '...' or 'and so on', a system, that is, in which the expressions which are substitutable for variables are explicitly listed, then it is clear we do not speak such a language, but rather the schema of a language. Only an omniscient being could effectively use such a language.... The symbol structure we employ... is almost completely schematic as far as individual constants are concerned. We are obliged to make use of general propositions in talking about the world.

We are obliged to make use of general propositions in talking about the world, but an omniscient being—a being able explicitly to state the (possibly infinite) conjunctions and disjunctions that our quantified generalities are logically equivalent to on the substitutional view—would not. Because we are not omniscient, we employ schemata of sentences to achieve the same end.

According to the substitutional interpretation, quantified sentences are to be understood as truth-functions of singular sentences, and hence to be about objects in much the way singular sentences are. One does not list all the cases; nevertheless, the truth of the sentence is to be understood as a function of the truth of those cases. The objectualist begins instead with the satisfaction of predicates by objects or sequences of objects. Objectually interpreted, the truth of a quantified sentence is understood not by reference to the truth of its instances but instead by whether or not the relevant predicate is satisfied by some or all objects in the domain. In an adequate formalization of the two approaches this difference takes the form of the fact that, by contrast with a substitutional account, the objectual interpretation requires appeal to a denotation function in the definition of the truth of a quantified generality. In a substitutional account, open sentences, that is, formulae with free variables, are not assigned any semantic interpretation. Just as Kripke (1976 330) says, on the substitutional interpretation "we have no satisfaction, only truth." Later we will see why this debate arises. For now we turn to the technical details, Tarski's 1933 account of quantifiers objectually interpreted. (Bonevac (1985) provides a structurally analogous account for the substitutional interpretation.)

According to the objectual interpretation, a universally quantified claim ascribes a property to all objects in the domain of quantification and an existentially quantified claim ascribes it instead to some objects, at least one, in the domain of quantification. But this alone is not sufficient for an account of multiply embedded quantifiers. The sentence 'everybody loves somebody', for example, is expressed quantificationally as '$(\forall x)(\exists y)Lxy$' and is true just in case everyone has the property of loving someone, that is, quantificationally, just in case every object in the domain of quantification has the property $(\exists y)Lxy$. But what is this property? Even if we know what all our singular terms designate and know the semantic values assigned to each of the n-ary predicates in the language, this will not determine the semantic value of '$(\exists y)$ Lxy'. Because, moreover, there are an endless number of such complex predicates—formed from singular terms, predicates, the connectives, and the quantifiers—we need a recursive account. Tarski's (1933) definition of truth in formalized languages serves admirably.

The task is to extend the simple model theoretic account of the truth of atomic and truth-functionally compound sentences to a general account of arbitrarily complex quantified sentences. The means, on the Tarskian account, is the notion of the satisfaction of an open sentence, that is, a sentence containing one or more free variables, by a sequence of objects. (Although neither variables nor sequences of objects are needed in an account of the semantics of atomic sentences and of truth-functions and monadic quantifications of them—though one does need a mechanism for marking the argument places of n-ary predicates—the technical notion of a variable is indispensible in the classic Tarskian account of the semantics of the polyadic predicate calculus.) We formulate the truth definition for our language as follows.[11]

1. Names: 'a' designates a; 'b' designates b; and so on.
2. Predicates: object α satisfies 'F' iff α is F; α satisfies 'G' iff α is G; and so on.
3. A sequence S satisfies a (closed) sentence 'Φn' iff the object n satisfies Φ.
4. A sequence S satisfies an open sentence 'Φx_k' iff the k^{th} member of S satisfies Φ.
5. A sequence S satisfies '$(\exists x)\Phi x_k$' iff 'Φx_k' is satisfied by some sequence S' that is like S except perhaps in the k^{th} term.
6. A sequence S satisfies '$(\forall x)\Phi x_k$' iff 'Φx_k' is satisfied by all sequences S' that are like S except perhaps in the k^{th} term.
7. A sentence is true iff it is satisfied by all sequences.

Obviously we need to say something more about the notion of the satisfaction of an open sentence 'Φx_k' by a sequence.

First, again, an open sentence is not merely a predicate expression; it is a predicate expression all of whose argument places have been filled by variables. Because we will need to correlate our variables with objects in our sequences, we suppose that the variables, x, are numbered 1 through n for arbitrarily large n. That is, we have variables, x_1, x_2, x_3, and so on, and open sentences such as 'Fx_1', '$Fx_1 \supset Gx_2$', and '$(\exists x_1)Rx_1x_2x_3$' (in which only x_2 and x_3 are free). Now we assume that we have a function from the natural numbers to objects that generates (infinitely long) ordered sequences of objects. The first variable x_1 is assigned the object in the first place in a

[11] I here follow the account developed in Platts (1997, 21).

given sequence, the second variable is assigned to the object in the second place in the sequence, and so on. A sequence S will satisfy the open sentence 'Φx_k' just if the object at the k^{th} place in S satisfies Φ, just as in (4) above.

But there are of course a vast number of objects about which we might wish to judge. Indeed, there would seem to be non-denumerably many of them given that we make judgments about, say, the real numbers. To guarantee that there are enough sequences to deal with all the cases, the following three conditions are stipulated:

1. There is at least one denumerable sequence.
2. For every sequence S, for every natural number I, and for every individual y, there is a sequence S' which differs from S in at most the I^{th} place and whose I^{th} member is y.
3. For any finite sequence S there is an infinite sequence that results from iterating the last term of S.

The truth conditions of quantified sentences of arbitrary complexity are thereby fixed. We have our semantics.

Now we need to consider the kind of logical complexity this semantic theory exhibits.

We know that there are significant logical differences between, first, simple universally quantified sentences, ∀-sentences, then, at the next higher level of complexity, sentences that embed (ineliminably) an existential quantifier within the scope of a universal quantifier, so-called ∀∃-sentences, and finally, sentences that further embed (ineliminably) a universal quantifier within the scope of the embedded existential quantifier, that is, ∀∃∀-sentences. The monadic predicate calculus, for instance, is essentially simpler than a polyadic predicate calculus precisely because any ∀∃-sentence in the language of the monadic predicate calculus, for example, '$(\forall x)(\exists y)(Fx \supset Gy)$', can be rewritten in such a way that no quantifier is within the scope of another, in our example, '$(\forall x)Fx \supset (\exists y)Gy$'. All quantifier dependence can be eliminated in such a language, and correspondingly, any set of axioms expressible using only the monadic predicate calculus has a finite model.

∀∃-sentences in which the quantifier embedding is ineliminable are logically more complex than quantified sentences containing no (or only eliminable) embedded quantifiers. As just noted, axioms expressible without quantifier embedding have finite models. There are only infinite models for axioms that are ineliminably ∀∃ in form, such as, for instance, the axiom that every number has a successor conceived quantificationally. ∀∃∀-sentences, which involve the level of quantifier dependence required in, for instance, the expression of various limit operations (again, assuming we understand them quantificationally), are different again. As Friedman (1992, 73) explains, in the case of ∀∃ quantifier dependence, for example, in the generation of points on a line by appeal to an axiom to the effect that for every point there is another to its left (read quantificationally), although the total number of points generated is of course infinite, each particular point is generated by a finite number of iterations: each point is determined by a finite number of previously constructed points. In generating or constructing points by a limit operation [which involves the logical complexity exhibited in ∀∃∀-sentences], on the other hand, we require an infinite sequence of previously given points: no finite number of iterations will suffice.

Limit operations, Friedman concludes, "involve a much stronger and more problematic use of the notion of infinity than that involved in a simple process of iterated construction." Tarski's account involves, we will see, just this "stronger and more problematic use of the notion of infinity." Assuming it is appropriate to interpret the general sentences of his theory quantificationally—as it must be if the model theoretic conception of language is to be understood, as it most often is, as a general account of any and all language—Tarski's account makes essential appeal to ∀∃∀-sentences in providing a semantic theory of the classical quantifiers.[12]

We saw that Tarski's account requires the notion of the satisfaction of a sentence by a sequence with the stipulation that for every sequence S, natural number I and individual y, there is a sequence S' which differs from S in at most the I^{th} place and whose I^{th} member is y. To insure that there are enough sequences to yield the right result in all cases, this condition must be satisfied. But, as the "at most" indicates, this condition has the form of an ∀∃∀-sentence: $(\forall S,I,y)(\exists S')(\forall I^* \neq I)$ the I^{*th} member of S is identical to the I^{*th} member of S' and the I^{th} member of S' is y. Quantifiers, we should not be surprised to discover given that they are to be appealed to in understanding any and all general sentences, both those that are merely accidentally true and mathematical theorems, say, about (all) real numbers, involve something on the order of the logical complexity of limit operations. Quantifiers simply must, if they are to do the job they are called on to do in mathematical logic, quantify over infinite, even non-denumerable, domains of objects, and Tarski's account deals with that fact essentially as nineteenth-century mathematicians dealt with limit operations in calculus. To account even for a simple universally quantified sentence Tarski must appeal to a condition that has the form of an ∀∃∀-sentence. Mathematically, there is no difficulty with this. Difficulties arise only if Tarski's account is taken, as he himself did not take it, to provide a philosophical analysis of generality for any sort of language, mathematical or natural.[13]

[12] The same is true of Bonevac's (1985) alternative analysis for the cases of the substitutional interpretation.

[13] It seems to have been Montague and Davidson who were most instrumental in popularizing the idea that Tarski's theory of truth could provide the basis for a theory of meaning for language generally. Montague, a student of Tarski's, published "The Proper Treatment of Quantification in Ordinary English," in 1973. As explained in an earlier work, "Universal Grammar" (Montague 1970, 22), one of Montague's basic premises is that "there is . . . no important theoretical difference between natural languages and the artificial languages of logicians; indeed I consider it possible to comprehend the syntax and semantics of both kinds of languages with a single natural and mathematically precise theory." Davidson outlines his well-known account of meaning in terms of Tarski's convention T in the first five essays of Davidson (1984).

The quantificational conception of generality aims to provide an account of logical generality that applies both to accidental, empirical generalities and to the demonstrably true generalities we find in mathematics. Intuitively, however, the two cases are very different; indeed, as I have repeatedly urged, in the latter case objects do not come into it at all. Thus, although in the case of a contingent generality such as Dummett's about the saucers in some cupboard it is quite natural to think of a quantifier as a determiner and hence as inherently restricted, in the mathematical case, if the notion of a quantifier is to be brought to bear (as Russell held), then it seems instead more natural to treat it as inherently unrestricted. Neither view, we have seen, has much intuitive plausibility as an account of both sorts of cases. The objectual/substitutional debate has a somewhat different shape; as we will later see in more detail, it arises because although the truth conditions of a quantified sentence and the corresponding truth function of instances are identical, the inferential consequences are not. We saw, finally, that in order to have an account that is technically adequate for all cases, those that arise in mathematics as much as those that occur in everyday life, something structurally identical to what we find in the case of limit operations is needed. This is unsurprising given that mathematical generalities often concern what, quantificationally, must be thought of as infinitely large, even non-denumerably large, domains of objects.

Tarski's work, as an account of how generality works in general, would seem to be obviously incoherent insofar as it explains what an $\forall$-sentence means by appeal to an $\forall\exists\forall$-sentence. Alternatively, what it can be taken to suggest is that we need to distinguish logically between accidental, merely contingent generalities, which really are about objects, and the apparently very different case of generalities in mathematics. The needs of mathematics, and in particular mathematics since the nineteenth century, which manifestly has moved a considerable distance from the needs and concerns of everyday life, perhaps should be addressed independently of the resources that are made available by our everyday talk about the things around us. Given that most mathematical truths of any interest are general, and that generality has a central role to play in mathematical reasoning, it is unlikely that we will understand how mathematics works as a mode of inquiry until and unless we have a more adequate conception of the nature of the generality that is exhibited in its judgments—however things work in the case of our everyday talk about things.

6.2 The Model-Theoretic Conception of Language

According to the mathematical logician, language, any language, involves two essentially different and opposed elements, logical form, which alone matters to the goodness of inference, and content that is given by an interpretation or model. More exactly, on this conception, a (first-order) language involves four essentially different sorts of signs that Hodges (1985, 144) characterizes as follows.

First, there are the *truth-functional connectives*. These have fixed logical meanings. Second there are the *individual variables*. These have no meaning. When they occur free, they mark a place where an object can be named; when bound they are part of the machinery of quantification. . . . The third symbols are the *non-logical constants*. These are the relation, function, and individual constant symbols. Different first-order languages have different stocks of these symbols. In themselves they don't refer to any particular relations, functions or individuals, but we can give them references by applying the language to a particular structure or situation. And fourth there are the *quantifier symbols*. These always mean 'for all individuals' and 'for some individuals'; but what counts as an individual depends on how we apply the language. To understand them, we have to supply a *domain of quantification*.

The result is that a sentence of a first-order language isn't true or false outright. It only becomes true or false when we have interpreted the non-logical constants and the quantifiers.

We have already examined the notion of a quantifier that is invoked here, and have seen why it needs also the notion of a variable. Now our concern is with the idea of a non-logical constant.

Why should we conceive language model theoretically, and in particular its predicates as non-logical constants that are meaningful only given an interpretation or relative to a model? One obvious answer is the Kantian one that without relation to any object given in intuition, one is left with a mere form. Of course Kant thought that mathematics has a kind of content in virtue of the pure forms of sensibility, but suppose we reject, as nineteenth-century mathematicians did, the idea of such forms—while leaving in place Kant's conception of cognition overall. Then we are left only with empty logical form, on the one hand, and empirical content on the other. Already in 1844 in his *Ausdehnungslehre* Herman Grassman develops such a view. He distinguishes between formal sciences, which "do not go outside the domain of thought into some other domain, but remain completely within the field of combinations of different acts of thought" (quoted in Nagel 1939, 216), and real sciences, which are empirical. As Nagel (1939, 218) explains, for Grassman:

pure mathematics was to be developed entirely with the help of logic alone. Moreover, in order to proceed formally, without a reference to the contents of any science of nature, he recognized that he could not build his system upon axioms (i.e., some set of true *propositions*); and required instead what he called "definitions": these "definitions" implicitly defined the various elements and operations which he introduced, so that Grassman was one of the first mathematicians who explicitly recognized that mathematics is concerned exclusively with formal structures.

Grassman's formal theory involves concepts without intuitions. But on a Kantian understanding that means that it is without any content, or truth, at all. Grassman's theory is, then, purely formal in Kant's sense of formal.

Peacock in the 1830s provides what seems, at least on the surface, to be a somewhat different motivation for distinguishing between an uninterpreted language and an interpretation of it. His concern is with the symbolic language of arithmetic and algebra, and it is imperative, he thinks, to distinguish between arithmetical (meaningful) and symbolical (purely formal) algebra on the grounds that although in symbolical algebra it is invariably possible to transform, say, the equation $x + b = a$ into $x = a - b$, in arithmetical algebra the expression '$a - b$' has meaning only if a is greater than or equal to b. (See Nagel 1935, 180.) Symbolic algebra is, Peacock thinks, purely formal; the only constraint on permissible manipulations of its signs is that provided by the rules. Arithmetical algebra, because it concerns numbers, has additional constraints that are set by its subject matter, for example, the constraint that a larger number cannot be subtracted from a smaller.[14]

De Morgan similarly distinguishes between what he calls technical or symbolic algebra, which is an uninterpreted system of signs together with rules of their combinations, and what he calls logical algebra in which the signs are interpreted. He also takes a step further than Peacock in suggesting that a symbolic algebra might be interpreted in more than one way. In illustration of the idea, he supposes that we have three symbols, M, N, and +, and one rule: that M + N is the same as N + M. "Here," De Morgan writes (quoted in Nagel 1935, 186), "is a symbolic calculus: how can it be made a significant one?" He offers the following five interpretations:

1. M and N may be magnitudes, + the sign of addition of the second to the first.
2. M and N may be *numbers*, and + the sign of multiplying the first by the second.
3. M and N may be lines, and + the direction to make a rectangle with the antecedent for a base, and the consequent for an altitude.
4. M and N may be *men*, and + the assertion that the antecedent is the brother of the consequent.
5. M and N may be *nations*, and + the sign of the consequent having fought a battle with the antecedent.

The first three interpretations are especially interesting insofar as Viète had already in the sixteenth century held a similar view about the symbolic language of algebra: in itself it was an uninterpreted calculus, one that could be interpreted either arithmetically, as concerned with numbers, or geometrically, as about figures such as lines and rectangles. And, we saw, it had to be so conceived because for Viète as for ancient Greek mathematicians one cannot operate with lines the way one operates with numbers: multiplying one number by another gives a number but multiplying one line length by another gives instead a rectangle. 'Multiply' means one thing in the case of numbers and something very different in the case of line lengths; it has to be interpreted differently in the two cases.

[14] Notice that if it is assumed that all content is empirical then this will seem obviously right. If numbers, insofar as they have any content at all, are numbers of empirical objects (of stars, apples, playing cards, and so on) then of course it will be true that one cannot take away more of these things than one had to begin with.

Boole takes up de Morgan's idea and applies it to the "laws of thought" in his *Mathematical Analysis of Logic* (1847). As he explains in the introduction:

They who are acquainted with the present state of the theory of Symbolical Algebra are aware, that the validity of the processes of analysis does not depend upon the interpretation of the symbols which are employed, but solely upon the laws of their combination. Each system of interpretation which does not affect the truth of the relations supposed, is equally admissible, and it is thus that the same process may, under one scheme of interpretation, represent the solution of a question on the properties of numbers, under another that of a geometrical problem, and under a third, that of a problem of dynamics or optics. (Quoted in Nagel 1935, 166.)

It is this idea that is to be extended now to the realm of thought, to logic. In particular, algebraic properties of multiplication and addition that were first formulated for the natural numbers are now to be shown to hold also for the intersection and union of classes and for the logical relations of conjunction and disjunction between propositions. The signs are not, then, to be understood as signs for arithmetical operations, or for operations on classes, or for logical relations among propositions. They are determined only by the rules governing their manipulation, and can be given a variety of interpretations. As Boole shows, by manipulating those signs according to the rules one can derive valid conclusions from classical categorical premises.

And here we find a crucial difference between Viète's idea of an uninterpreted formal language and De Morgan's idea as developed by Boole. For Viète, the formal, uninterpreted language is nothing more than a useful tool for solving problems in arithmetic and geometry. For Boole, the formal uninterpreted language and the possibility of different interpretations of it together reveal something fundamental about the nature of thought: thought has two distinct and separable parts, a formal part and a material part the difference between which is mirrored in the distinction between an uninterpreted symbolism and an interpretation of it. Reasoning is, in essence, the mechanical, rule-governed manipulation of empty signs. It is nothing more than the workings of a kind of mechanism, something that could as well be achieved by a mere machine. Boole is often looked upon as the founder of computer science for just this reason.[15]

An apparently different motivation can seem to pull in the same direction, that is, towards a model theoretic conception of language, namely, a concern for rigor, and in particular, the thought that if one's reasoning really is fully rigorous, that is, completely gap-free, then it will not be necessary to attend to meanings at all. Pasch—"the father of rigor in geometry" according to Freudenthal (1962, 237)—writes in his *Vorlesungen über Neuere Geometrie* (1882; quoted in Nagel 1939, 237):

If geometry is to be really deductive, the deduction must everywhere be independent of the *meaning* of geometrical concepts, just as it must be independent of the diagrams; only the *relations* specified in the propositions and definitions employed may legitimately be taken into

[15] Viète could not have had this conception insofar as it relies on the notion of a law that would be clarified only by Descartes.

account. During the deduction it is useful and legitimate, but in *no* way necessary, to think of the meanings of the terms; in fact, if it is necessary to do so, the inadequacy of the proof is made manifest.

Nagel's (1939, 238) characterization of Pasch's view is even more revealing:

Everyone is aware, he [Pasch] pointed out, of the dangers which threaten the geometer who uses diagrams and other sensory images; but few seem to be equally aware of the traps which we lay for ourselves when we employ common words to designate mathematical concepts. For such words have many associated meanings not relevant to the task of a rigorously deductive science, and these associated meanings sway us to the detriment of rigor. To avoid these handicaps it is therefore desirable to *formalize* the set of nuclear propositions; that is, we ought to replace them by a series of expressions in which the "geometrical concepts" of the propositions have been replaced by arbitrarily selected marks, whose sole function is to serve as "places" or blanks to be filled in as occasion may warrant. The result of such a formalization is an "empty frame," which expresses the structure of the set of nuclear propositions and which alone is relevant to the task of pure geometry.

In pure geometry, according to Nagel, all meaningful words have been replaced by meaningless signs, which insures that one draws no inference grounded in meaning. Pure geometry, like pure mathematics in general, is to be conceived as nothing more than the manipulation of empty marks according to rules.

But rigor does *not* entail formalism in this sense. Rigor requires only that one make explicit all one's presuppositions, everything on which one's reasoning depends. And yet Nagel clearly seems to think that the (only) way to avoid being "swayed" by "associated meanings" is by jettisoning meanings altogether. Why? Why does Nagel slide so easily from the demand for rigor to the formalist requirement that all meaningful (that is, non-logical) expressions be replaced by empty signs? A plausible answer is that he associates rigor with mechanism: to be really rigorous is to think like a machine impervious not only to associated meanings but to any meanings at all. We have seen already that this is a characteristically modern idea: whereas the ancients modeled even the workings of inanimate nature on the animate, the modern turn was to model the animate on the inanimate, that is, the merely mechanical and law governed. Descartes famously held that non-rational animals are nothing more than complex machines. Perhaps even minds—at least insofar as they are *logical*—are nothing more than complex machines. Only so, as far as I can see, can someone like Nagel take it to be *obvious* (as he clearly does) that rigor can be achieved only by formalism. But if so, it is not the needs of mathematics that is driving the push towards the model theoretic conception of language but instead tacitly held assumptions about what it means to be rational at all.

We turn, finally, to the most directly mathematical motivation for the model-theoretic conception of language, namely, the new use to which axiom systems are put in modern mathematics, the fact that different systems of objects can be shown to be isomorphic, and hence can serve as models for one and the same set of axioms.

This is explicit, for instance, in Weyl's discussion in his *Philosophy of Mathematics and Natural Science* (1949; quoted in Shapiro 1997, 160):

> an axiom system [is] a *logical mold of possible sciences*. . . . One might have thought of calling an axiom system complete if in order to fix the meanings of the basic concepts present in them it is sufficient to require that the axioms be valid. But this ideal of uniqueness cannot be realized; for the result of an isomorphic mapping of a concrete interpretation is surely again a concrete interpretation. . . . A science can determine its domain of investigation up to an isomorphic mapping. In particular, it remains quite indifferent to the "essence" of its objects. . . . The idea of isomorphism demarcates the self-evident boundary of cognition. . . . Pure mathematics . . . develops the theory of logical "molds" without binding itself to one or other among possible concrete interpretations. . . . The axioms become *implicit definitions* of the basic concepts occurring in them.

We have seen already that a fundamental idea in the progress toward abstract algebra was the realization that the familiar axioms of elementary algebra although formulated first for ordinary numbers can be applied to other systems as well. Because whatever theorems can be proved on the basis of the axioms must hold also for any system that satisfies the axioms, it came to seem that the axioms of algebra should be understood as mere forms and its non-logical constants as uninterpreted signs that could then be assigned various interpretations. But, as already indicated, that is not the only way to think about what is going on here.

Consider again the familiar axioms of algebra, for instance, these:

1. $a + b = b + a$
2. $(a + b) + c = a + (b + c)$
3. $a \times b = b \times a$
4. $(a \times b) \times c = a \times (b \times c)$
5. $a \times (b + c) = (a \times b) + (a \times c)$.

Given these axioms governing permissible rewritings, one can further derive various theorems, which similarly govern permissible rewritings, for instance, this: $(a + b)^2 = a^2 + 2ab + b^2$. The question is how exactly we should we think of these axioms. One way is quantificationally, that is, in terms of the conception of generality that we find in standard logic. On this way of thinking, the axioms are implicitly universally quantified claims about ordinary numbers, basic truths about numbers, all of them. But we know that these axioms can also be interpreted differently, that is, applied to different systems of things. Although we *can* read the axioms as truths about numbers, as on the quantificational reading, those same axioms can also be interpreted differently, as truths about quite different sorts of objects. So long as the axioms are satisfied in a given domain then the derived theorems will hold in that domain. And this is, of course, an important, and importantly mathematical, insight. What it suggests, if one is working with the quantificational conception of generality, is that we instead understand the axioms model-theoretically. We find just such a line of reasoning, for example, in Hodges (1985, 146), who argues that "for mathematical purposes, we need non-logical constants whose interpretation is fixed in a structure but not fixed in the language." "We want to say, for example, that two structures have some property in common. One method is to write the property as a first-order sentence and say that it is true in both structures" (Hodges 1985, 145). And that is indeed one way to achieve the desired result. But there is also another.

On our first, quantificational reading, the axioms, and the theorems derived from them, are conceived as expressing truths about numbers, facts about how things stand with those objects. On the second, model theoretic reading, they are conceived instead as uninterpreted stipulations for which many models might be given. The third way to conceive those same axioms, and the theorems derived from them, is as basic (and derived) truths about the *operations* of addition and multiplication, as ascribing properties to those operations, here, commutativity, associativity, and distribution. On this reading, the axioms and theorems are perfectly meaningful (as they are not on the second, model theoretic reading), but they are in *no way* about numbers (as on the first, quantificational reading). What the axioms and theorems concern on this third reading are certain arithmetical operations and particular properties of those operations. On this reading, the axioms set out basic properties of those operations and on that basis one derives theorems that ascribe other, non-basic properties to those same operations.[16] These properties serve, in turn, to ground and explain the goodness of various inferences. It is, for instance, because addition is commutative that the inference from, say, $7 + 5 = 12$ to $5 + 7 = 12$ is a good one.

This third reading of the axioms (and theorems derived from them) as ascriptions of higher-level properties to the operations of addition and multiplication helps to clarify the first two readings as follows. First, if, as the third reading has it, addition has the property of being commutative then it obviously follows that for any two particular numbers, 3 and 4, say, $3 + 4 = 4 + 3$. It follows, that is, that the quantificational reading of that same axiom is true. But now, as we can see, that universally quantified generality is not known to be true directly. (How could it be given that there are infinitely, indeed, non-denumerably many numbers to which it applies?) Instead that quantified generality is known to be true *because* addition is commutative. It is not merely an accidental truth about numbers that as it happens for any two of them the first plus the second is equal to the second plus the first. It is necessary that this be so. And this necessity at the level of individual, particular numbers can be explained by the fact, one level up—that is, at the level of operations on numbers rather than at the level of the numbers themselves—that the operation of addition has the property of being commutative.[17]

[16] On this reading, again, the axioms make no reference of any kind to any objects; the letters instead serve as a means whereby a second-level property is ascribed to a first-level operation.

[17] It is worth recalling in this context that a fundamental feature of the new mathematical practice that developed in the nineteenth century was its top-down definitions of concepts in terms of various intrinsic

The second, model theoretic reading is also explained because what that reading can be seen to reflect is not a distinctively model theoretic insight, but instead the fact that valid inferences are instances of something essentially general that can be applied to other cases. Suppose, for example, that I infer from the fact that Felix is a cat that Felix is a mammal. If that inference is a good one, that is because it is good in general to infer from something's being a cat to its being a mammal. Any actual inferences (that is, steps of reasoning in actual cases) are like this: they are instances of something more general, a rule that can be applied also in other cases. Applied now to the case of our axioms, the basic idea is this. If, as on our third reading, we take our axioms and theorems to be ascriptions of (higher-level) properties to the operations of addition and multiplication, then we can see as well that the inferences from the axioms to the theorems are *not* good in virtue of the fact that they are about addition and multiplication. They are good because having those particular properties that are ascribed in the axioms entails having certain other properties as well. That is just what the derivations of the theorems from the axioms shows. It follows directly that any other function or operation having the same particular properties as are ascribed in the axioms (commutativity, associativity, and so on) will also have the properties that are ascribed in the theorems that are derived from the axioms. The insight that the axioms and theorems can be applied in other cases *does not show* that the model theoretic conception of language is the right conception to have. That insight is simply a reflection of a fundamental feature of inference, the fact that any particular inference is an instance or application of a general rule that can be applied also in other cases.

On our third reading, the axioms of elementary algebra are contentful truths about the operations of addition and multiplication from which other truths follow. All these truths function in turn to license inferences in particular cases, that is, in demonstrating theorems, for instance, this: if two numbers are each a sum of two integer squares then their product is also a sum of integer squares. We begin by formulating the starting point in the symbolic language: we have two numbers that are each of them a sum of two integer squares, which we express in the symbolic language thus: $a^2 + b^2$ and $c^2 + d^2$. What we must show is that their product is also a sum of integer squares. So we write down the product of our two sums thus: $(a^2 + b^2)(c^2 + d^2)$. Notice what we have done here. We began with a certain mathematical idea, the idea, or as we can also say, the concept, of a sum of two integer squares. The content of that concept, what it is to be a sum of two integer squares, was then expressed in the language of algebra: $a^2 + b^2$. This expression, '$a^2 + b^2$', does not merely abbreviate the English phrase—though one could imagine so using the signs (perhaps thus: $+ 2\ \mathrm{I}^2$)—but instead formulates in a specially designed system of signs

properties. (We saw this in section 5.3.) This new form of definition was to replace the bottom-up naïve abstractionism that had before been assumed to provide our mode of access to mathematical concepts. What matters for the purposes of nineteenth-century mathematics is not that addition takes pairs of numbers to give numbers but that it has various properties, including the property of being commutative.

the content of that English phrase, what it means mathematically. To express the idea that we have two such sums we then use different letters in place of 'a' and 'b', and on that basis we are able to exhibit the content of the idea of a product of two numbers that are, each of them, a sum of two integer squares: $(a^2 + b^2)(c^2 + d^2)$. Having thus formulated the content of the concept *product of two sums of (two) integer squares* in an arithmetically articulated complex of signs in the language, we can now apply the rules given in the axioms and derived theorems.

Leaving out obvious steps—steps that we could easily put in, tedious though it would be to do so—the fifth axiom licenses the move from

$$(a^2 + b^2)(c^2 + d^2)$$

to

$$a^2c^2 + a^2d^2 + b^2c^2 + b^2d^2.$$

Notice, first, that although the rule applies to a certain form of expression it is still perfectly possible to keep the content expressed in view as we apply the rule. As an indication of this we can express the rule in axiom 5 in (meaningful) words: if you have a product of a number and a sum then it can be rewritten as a sum of the products of the number together with each of the summands. It is also worth remarking that although we generally think of inferential articulation as something that involves a relation *between* concepts, here we are dealing with an *internally* articulated idea. It is the whole expression that sets out the idea with which we are concerned, the idea of a product of two sums of integer squares; and because that expression is internally articulated, we can apply the rewrite rules of elementary algebra to it.

Now we need to reorder to get

$$a^2c^2 + b^2d^2 + a^2d^2 + b^2c^2. \qquad .$$

At this point a student might wonder why that was a good move to make. The reason, however, becomes clear at the next step in which we both add and subtract $2abcd$ (with the letters appropriately reordered) to give, after some reorganization,

$$a^2c^2 - 2acbd + b^2d^2 + a^2d^2 + 2adbc + b^2c^2. \qquad .$$

And now even the student ought to be able to see that this last expression can be rewritten (by appeal to familiar derived rules) as

$$(ac - bd)^2 + (ad + bc)^2. \qquad .$$

But that, we can see, is just what was wanted, a sum of two integer squares. We have the desired result.[18]

[18] As ought to be evident, had we proceeded slightly differently the result would have been instead $(ac + bd)^2 + (ad - bc)^2$. There are two different sums of integer squares equal to any given product of sums of integer squares.

By formulating the content of the concept with which we began, the content of the concept *product of two sums of integer squares*, in the formula language of algebra, we were able to reason in the language, by putting equals for equals, to obtain eventually the result that was wanted, that that product is a sum of integer squares. That reasoning was, or at least could have been made to be, fully rigorous, every step licensed by a basic or derived rule in the system, but it was also fully contentful. In proving the theorem that the product of two sums of integer squares is also a sum of integer squares, using the language of elementary algebra, one does not abstract from content; instead one *expresses* content in a mathematically tractable way, in a way enabling reasoning in the system of signs. Of course the beginning student will operate with the language in a fairly mechanical way, much as the beginning chess player operates with chess pieces in a mechanical way in the course of a game. It takes time and practice to become literate in the system, to learn to see the meanings in the signs, the contents they express, and this is not surprising. The symbolic language of elementary algebra is quite unlike natural language. It is a specially designed system operating on its own distinctive principles, and it takes practice, and some skill, to learn to use it to its full potential—just as it takes practice, and skill, to become a really good chess player, to learn to see the opportunities and hazards in particular configurations of chess pieces.

There is a further lesson in this example as well. As Avigad (2006b, 115), from whom the example is borrowed, notes, "the proof uses only the commutativity and associativity of addition and multiplication, the distributivity of multiplication over addition and subtraction, and the fact that subtraction is an inverse to addition; hence it shows that the theorem is true much more generally in any *commutative ring*." The little chain of reasoning rehearsed above in the symbolic language of elementary algebra is valid, and so we know that it is an instance of a rule that can be applied also in other cases provided that the requisite properties hold in those cases. This chain of reasoning does not show that we are, in this case, not in fact reasoning about products of sums and sums of squares. What it shows is that the reasoning can also be applied in other cases as well. Again, *any* actual inference is an instance of something more general, something that can be applied in other cases as well. It is just this that explains the fact that, as Avigad notes, our theorem about integer squares holds in any commutative ring.

We saw in Chapter 3 that Viète understood the language of algebra as an uninterpreted formalism because although it could be interpreted either arithmetically or geometrically, that is, as about numbers (conceived as collections of units) or as about geometrical objects such as squares and rectangles, there seemed from Viète's essentially pre-modern perspective no way to give a unified treatment of the signs, one that would embrace both arithmetic and geometry. Descartes, by contrast, was able to give a unified reading of the language because according to him mathematics, at least universal mathematics (*mathesis universalis*), is not about objects, whether numbers or geometrical figures, but instead about relations that such objects can stand in. What an equation expresses is, in his view, an arithmetical relation. By moving everything up a level in this way, from consideration of objects to consideration of relations that objects can stand in, Descartes is able, as Viète is not, to provide a univocal meaning for the signs of the language.

Similarly, if one thinks of a general sentence such as an axiom of algebra quantificationally, as about all objects in the domain of quantification then it will seem impossible to give a unified treatment of the signs employed in our axiomatization above, one that will embrace all the models to which those axioms can be applied. If one thinks that what the axioms are about are objects, that they have certain properties and relations, then it will seem that—given that different objects, properties, and relations are also correctly described using sentences of the same form—we can, and should, treat the form separately from the various structures that have that form, and the language as a whole model theoretically. The alternative, corresponding to the move Descartes makes, is to move everything up a level again, to conceive the axioms not as about objects but instead as about the operations designated by '+' and '×', as ascribing to them various properties on the basis of which to draw inferences. This is made explicit in abstract algebra, in the definition of, for instance, a group. In that definition various properties are stipulated of an operation not further specified, and various things are shown to follow from that definition, perhaps together with others.

Much as the subject matter of mathematics was changed in the seventeenth century so it was changed in the nineteenth century. And in both cases something new had to be recognized as a locus of the truth of mathematical judgments, first-level relations and functions in early modern mathematics, and higher-level properties and relations in the case of contemporary mathematics. And in both cases one might, for whatever reasons, resist the move—or simply fail properly to understand it. If, for example, one thinks (with Kant) that all content and all truth lies in relation to an object then, given that the axioms hold in different domains of objects, it will be natural to distinguish the *forms* of the axioms conceived as collections of uninterpreted signs from their *contents* on an interpretation. But we have seen that there may be other forces at work as well, in particular, an essentially reductive, mechanistic conception of thinking and reasoning. If reasoning really is rule governed, on this line of thought, then it can have nothing whatever to do with meanings, and must be something even a mere machine can do. The alternative, that reasoning is a capacity of a certain sort of living being in whose nature it is to be rational, is not so much as considered. We are left with the model theoretic conception of language, a fundamental opposition of form and content, and no prospect of understanding the striving for truth in the practice of mathematics.

6.3 Meaning and Truth

We have seen that modern mathematics does not require that we adopt a model theoretic conception of language. Nor is it obvious that we should adopt the quantificational conception of generality—though the two ideas are in certain respects mutually reinforcing. They are also both fundamentally connected to a third, the idea that meaning is to be understood in terms of truth, ultimately, given our understanding of quantifiers, in terms of truths about objects: to know the meaning of a sentence is to know what is the case with objects if it is true. But this apparent truism leads us also to accept something that is less obviously true, and in fact, we will see, false: that sentences that have the same meaning in the sense of truth conditions, what is the case if the sentence is true, thereby have the same meaning in the sense of inference potential, what follows if the sentence is true. Because we equate the two notions, we think that a language that tracks truth conditions provides everything that is needed in inference. This is codified in the mathematical logician's relation of logical consequence: a sentence is a logical consequence of some other sentence or sentences just in case if the latter sentence or sentences are true then the former is as well. This seems unproblematic in the case of the truth-functional connectives. That is, it is not unreasonable to suggest that the inferences from, say, '$A \& B$' to 'B' and from '$\sim A \vee B$' and 'A' to 'B' are good because if '$A \& B$' is true then 'B' must be as well, and similarly for our second inference from '$\sim A \vee B$' and 'A' to 'B'. And if one interprets generality quantificationally then all inferences should be explicable this way; whether one interprets the quantifiers objectually or substitutionally, inferences involving them should be, as our simple inferences above are, merely truth functional. Thus it comes to seem that in order to express everything necessary for a correct inference it is enough to track truth conditions, what is the case if a sentence is true. We will see that both the familiar debate between Millians and descriptivists about proper names, and the debate outlined above between objectualists and substitutionalists about the quantifiers, suggest instead that two different notions are at work, a notion of meaning in terms of truth conditions and another very different notion of meaning as it matters to inference.

Meaning is understood in terms of truth in quantificational logic, and because it is, the notion of sense that we inherit from Frege cannot be conceived as a properly logical notion at all. It must (on the quantificational reading of Frege's notation) concern instead the cognitive value of a sentence for a thinker. Two sentences that have precisely the same truth conditions can differ in cognitive value for a thinker if that thinker, without manifest irrationality, can assent to one but not the other. Sameness and difference in semantic value, in truth conditions, is a logical difference, a difference in the meaning expressed by a sentence; sameness and difference in cognitive value for a thinker is not a logical difference. This much is clear. What is less obvious in particular cases is whether a difference is semantic or whether it is instead cognitive, whether, that is, it is a logical difference properly speaking at all.

Consider, first, the case of proper names. It is very natural to think of a proper name merely as a label for an object, as a means of referring to the thing named in order then to go on to predicate something of it. This is the Millian view of names. That it is the correct view is suggested, first, by the fact that one's beliefs about a given object can be radically revised over time while remaining thoughts about one and the same object, and also by the fact that two different people can have wholly different beliefs that are nonetheless about one and the same object. Both these considerations suggest that names have no descriptive content, that they are merely labels. Furthermore, where two ordinary proper names are in fact names of one and the same object, whether or not anyone knows this, then the truth conditions of two simple sentences that differ only in their involving one or other of the two names will be identical: if '*a*' and '*b*' name the same object then '*a* is F' and '*b* is F' will both be true just in case the object named has the property of being F. One and the same object is the locus of the truth of both sentences and one and the same property is ascribed; the two sentences have the same truth conditions. A thinker may not realize that it is one and the same object that is the locus of the truth of the two claims, nevertheless, it seems correct to say that where two names designate one and the same object, sentences involving the two names that are otherwise identical have the same truth conditions.

But what, then, are we to make of the fact that it does not immediately follow from, say, the fact that Hesperus is a planet that Phosphorus is a planet? The inference from 'Hesperus is a planet' to 'Phosphorus is a planet' is not valid. Nor of course is it valid to argue that Hesperus is Hesperus, therefore Hesperus is Phosphorus. What is needed to turn such invalid arguments into valid arguments is precisely the identity, that Hesperus is Phosphorus. The explanation for this, the descriptivist argues, is that names are really disguised descriptions, and (generally) different descriptions in the case of different names. That is why the sentence 'Hesperus is Hesperus' is trivial, although the sentence 'Hesperus is Phosphorus' can constitute a real extension of our knowledge. The argument is often attributed to Frege, but Frege, we will eventually see, is not a descriptivist about proper names, though Russell (1905) is, at least about ordinary proper names.

We can understand this debate between the Millians and the descriptivists as follows. We assume for the purposes of the argument that if two sentences have the same truth-conditions then they have the same inferential significance, the same consequences, and use as our example the familiar pair of sentences, 'Hesperus is a planet' and 'Phosphorus is a planet'. According to the Millian, this pair of sentences shows that proper names are merely labels for objects because, after all, both sentences refer to one and the same thing, namely, Venus, and say the same thing about it, that it is a planet. One might not know that they have precisely the same truth-conditions, but they do. The differences between them, for instance, the fact that one does not infer one from the other, is to be explained by appeal to the notion of cognitive value. It is possible to know that Hesperus is a planet without thereby knowing that Phosphorus is a planet because, although the two sentences mean the same thing, they can have different cognitive values for a thinker. In sum, on the Millian view the two sentences have the same meaning, the same semantic value, but can have different cognitive values to a thinker.

Where the Millian argues modus ponens from our assumption, argues that is, that the two sentences have the same truth-conditions and hence the same inferential consequences, the descriptivist argues modus tollens: the two sentences do not (*pace* the Millian) have the same inferential consequences, as is shown by the fact that there is no valid inference from one to the other without the additional premise that Hesperus is identical to Phosphorus, and hence the two sentences also, the descriptivist concludes, do not have the same truth-conditions. The difference between the two sentences, in other words, cannot be merely cognitive but is semantic, a difference in meaning, in what is the case if the sentences are true. And yet the Millian seems to be *right* about the truth-conditions of the two sentences: *they are identical*. The descriptivist nonetheless seems to be right about the *inferential consequences* of the two sentences: they are *different*. But if so, then it is the conditional that both agree on that is wrong: a specification of truth-conditions (what is the case if a sentence is true) is not thereby a specification of inference potential (what follows if a sentence is true).

The second debate has just the same structure but concerns the nature of the quantifiers, in particular, whether a universally quantified claim is logically equivalent to a conjunction of instances, and an existentially quantified claim logically equivalent to a disjunction of instances (as the substitutionalist claims), or whether they are logically different (as the objectual interpretation has it).

Let us stipulate that '$\forall$x' and '$\exists$x' be given the objectual interpretation and 'Πx' and 'Σx' the substitutional interpretation. It is clear that in the case in which the domain is finite and surveyable ($\forall$x)Fx iff (Πx)Fx and ($\exists$x)Gx iff (Σx)Gx; in both cases the two sentences are materially equivalent, the one true just in case the other is. Indeed, it can be argued that no matter what the domain, the truth conditions of the two sentences are the same. Although in different ways, they ascribe the same properties to precisely the same objects. So, the substitutionalist argues, a quantified generality is logically equivalent to a truth function of instances. Only they do not seem to be if one considers also their role in inference. It is a matter of logic that ($\forall$x) Fx implies (Πx)Fx, which is the schematic form of the relevant conjunction of instances, and correlatively that (Σx)Gx, schematic for a disjunction of instances, implies ($\exists$x)Gx. If a universally quantified sentence objectually interpreted is true then one can validly infer any conjunction of its instances one cares to name; and if a disjunction of instances is true then one can validly infer the relevant objectually interpreted existentially quantified sentence. That much is clear and uncontroversial. The problem is that the converse inferences seem to be invalid, or if not invalid then enthymematic: in both cases a further premise would seem to be needed to the effect that the given cases are all the cases there are. "'($\forall$*x*) *Fx*' is stronger than any conjunction that can be formed of sentences of the form '*Ft*', while '($\exists$*x*) *Fx*' is weaker than any disjunction of such sentences" (Parsons 1971, 65). If that is right, then a quantified generality cannot be logically equivalent to a conjunction (or disjunction) of instances as the substitutional interpretation claims it is.

The substitutionalist, seeing that the truth-conditions are the same in the two cases, argues that there is only a cognitive difference between a quantified sentence and the relevant truth-function of instances. The objectualist, seeing that the inferential consequences of the two are different, argues that there must then be a semantic difference between the two, a difference in meaning, in the truth-conditions in the two cases. Only there does not seem to be, and so neither side is able to convince the other. Again one wants to conclude that both are half right: the *truth-conditions* of the quantified sentence and the truth-function are identical though the *inferential consequences* are not. Again, it is the assumption that sameness in truth-conditions entails sameness in inferential consequences that seems mistaken.

There is a further wrinkle. The substitutionalist holds that there is only a difference in cognitive value between a quantified generality and the relevant truth function of instances. And this, we saw, seems plausible if one is focused on what is the case if the generality is true, on its truth conditions. If the substitutionalist is right, then, the two sorts of theories, the substitutionalist and the objectualist, are themselves merely cognitively different, two ways of saying the same thing, though one might not realize that they are. The objectualist, focused instead on what follows from a quantified generality as contrasted with a truth function of instances, takes there to be a semantic difference between the two; because the inferential consequences are different in the two cases, the objectualist infers that the truth conditions must also be different in the two cases. For the objectualist, then, there is much more at stake in the debate. From this perspective, the substitutional interpretation, while it may be technically adequate, is simply wrong about how quantifiers work. It may yield the results that are wanted but its methods obscure what is really going on.

The language of standard quantificational logic was designed to track truth conditions. Indeed, as Quine (1970, 35-6) remarks, a good notation of logic, as he understands logic, is "designed with no other thought than to facilitate the tracing of truth conditions." And it is tacitly assumed that in tracing truth conditions the language will trace everything necessary for a good inference. But if, as the above considerations suggest, the notion of meaning as it matters to truth needs to be distinguished from the notion of meaning as it matters to inference, then we need to *ask* what our logical language ought to trace, whether what is the case if a sentence is true, or instead what follows if it is true. Given that the concern of logic is inference, one would presume that it is the latter notion that is needed.

6.4 The Role of Writing in Mathematical Reasoning

We saw that Suppes assumes, as is common, that mathematical reasoning is just like reasoning in everyday life. This is a surprising thesis if one thinks of the long history of mathematics. Certainly diagrammatic reasoning seems very different from the sort of reasoning one engages in when one is trying to come to some conclusion about, say, the relative merits of two candidates for a position in one's department or what sort of car one ought to buy. The computations of seventeenth- and eighteenth-century mathematicians similarly seem quite unlike everyday reasoning. For *most* of its long history mathematics has been distinguished by its use of written marks, marks that do *not* seem merely to abbreviate words of natural language; and much of mathematics would be paralyzed without those systems of marks. One does not need any system of written marks to reason in everyday life. Everyday reasoning did not wait on the development of a written language. But as we have seen, the mathematical practice that emerged over the course of the nineteenth century is also not constitutively written. Indeed, the new mathematical practice of reasoning from concepts championed by Riemann aims quite self-consciously to be freed of the constraints of the symbolism of seventeenth- and eighteenth-century mathematics. What then *is* the role of written marks in mathematical practice?

We know already that according to Kant writing broadly conceived as the inscribing of marks is a constitutive feature of mathematical practice. By contrast with the practice of philosophy, which makes a "discursive use of reason in accordance with concepts," the practice of mathematics makes "an intuitive use [of reason] through the construction of concepts" (A719/B747). And constructions, as Kant conceives of them, essentially involve some sort of writing, actual or imagined. "I construct a triangle by exhibiting an object corresponding to this concept, either through mere imagination, in pure intuition, or on paper, in empirical intuition, but in both cases completely a priori, without having to borrow the pattern for it from any experience" (A713/B741). Similarly, in elementary algebra, "mathematics . . . chooses a certain notation for all construction of magnitudes in general (numbers), as well as addition, subtraction, extraction of roots, etc., and . . . thereby achieves by a symbolic construction equally well what geometry does by an ostensive or geometrical construction (of the objects themselves)" (A717/B745). Reason in its discursive use through concepts, Kant thinks, does not in the same way rely on a system of written marks. Indeed, Kant thinks that reason in its discursive use does not rely (at least in a certain sense) on *any* sort of language. As he explains in the *Inquiry* (1764, 251; AK 2:278–9),

> the signs employed in philosophical reflection are never anything other than words. And words can neither show in their composition the constituent concepts of which the whole idea, indicated by the word, consists; nor are they capable of indicating in their combinations the relations of the philosophical thoughts to each other. Hence, in reflection in this kind of cognition, one has to focus one's attention on the thing itself.[19]

Unlike the written marks that mathematicians use, the words of natural language, whether spoken or written, are of no use in philosophical reasoning Kant thinks; the philosopher must consider the universal *in abstracto*.

[19] At this point, Kant clearly thinks that there can be no system of written signs for discursive reasoning from concepts. (See also the *New Elucidation*, Scholium to Section I, proposition 2, and §2 of the First Reflection in the *Inquiry*.) Later, Kant seems to change his mind. See his letter to Schultz (Kant 1788).

Kant's idea that writing, inscribing marks, is somehow constitutive of mathematical practice has its defenders still today. Rotman, for example, argues in *Mathematics as Sign* (2000, 44) that mathematics "is a form of graphism, an inscriptional practice based on a system of writing." According to him,

> mathematics is essentially a symbolic practice resting on a vast and never-finished language... One doesn't speak mathematics but writes it. Equally important, one doesn't write it as one writes or notates speech; rather one "writes" in some other, more originating and constitutive sense. (Rotman 2000, ix)

> Mathematical signs do not record or code or transcribe any language prior to themselves. They certainly do not arise as abbreviations or symbolic transcriptions of words in some natural language.... The symbol "√" is not a mathematical sign for "is the square root of," neither is "=" the sign for "is equal to"; rather, these English locutions are renderings of mathematical notions prior to them. (Rotman 2000, 44–5)

Others, we will see, maintain just the opposite; they hold that mathematical symbols *are* merely convenient shorthand—much as Suppes holds that the signs of logic are a convenient shorthand.

A second view of the role of writing in mathematical reasoning is what Rotman (2000, 47–8) calls the documentist view, which he ascribes, for instance, to Husserl. This is the view that language, whether written or spoken, is inevitably after the fact in mathematics, serving only to document or report results obtained independently. This documentist view of the role of language in mathematical reasoning is, that is, essentially Kant's view of the role of language in reason's discursive use. Although words can be used to *report* a finding of reason in its discursive use, the words are of no help in the reasoning itself. Thurston (1994) seems to defend such a view of the role of language in (current) mathematical practice insofar as it is, he suggests, one thing to *do* mathematics, quite another to *communicate*, or at least to try to communicate, one's mathematical ideas, whether verbally or in writing. The way mathematicians write is not, Thurston argues, the way mathematicians think: there are, he suggests, "big differences between how we think about mathematics and how we write it" (Thurston 1994, 44). And as he indicates, it is just this that explains why mathematical ideas can be communicated so much more effectively in face-to-face conversation than in writing:

> one-on-one people use wide channels of communication that go far beyond formal [written] mathematical language. They use gestures, they draw pictures, they make sound effects and use body language.... With these channels of communication, they are in a much better position to convey what is going on, not just in their linguistic faculties, but in their other mental faculties as well. (Thurston 1994, 43.)

The practice of mathematics, the *doing* of it as Thurston understands it, is clearly not essentially written.

Yet a third view, common among philosophers and assumed also by some mathematicians, is, as already mentioned, that mathematics is not merely reported, after the fact, in some written (or spoken) language as Thurston suggests, but actually *done in* natural language (or in the convenient abbreviations of standard symbolism), and hence is not essentially written at all.[20] According to Benacerraf (1973, 410), for example, mathematics is done in a "sublanguage" of English. For the later Wittgenstein (1976, sec. 18) similarly, the languages of mathematics are a "suburb" of the town that is language as a whole, one distinguished only by its "straight regular streets and uniform houses." And Suppes, we have seen, takes this view in his *Introduction to Logic*. It is also rehearsed in some college-level mathematics textbooks. Mathematics, we are told in one such text (Schumacher 2001, 1), "is done using a specialized dialect of English," that is, in natural language. "The symbols are simply a convenience: It is easier to write 'x^2' than 'the square of x', and '$x \in A$' is more compact than 'x is an element of the set A'. In each case the meaning is the same" (Schumacher 2001, 5). On this view, which manifestly contradicts not only the Kantian view of mathematics defended by Rotman but, along a different dimension, Thurston's documentist view as well, all the signs that are used in mathematics are merely shorthand. Though it would be more tedious, one could do all of mathematics in natural language.[21]

We have distinguished three views of the role of writing in mathematical practice, the Kantian view according to which mathematics (by contrast with natural language) is constitutively written, the documentist view according to which writing or indeed any public record or communication of one's results is essentially after the fact, and what we can think of as the mathematical logician's view that mathematics is done in natural language or alternatively in some system of signs that functions to abbreviate the words of natural language. A fourth view, that of Philip Jourdain in his little book *The Nature of Mathematics*, combines the first two. According to Jourdain (1912, 16),

> it is important to realize that the long and strenuous work of the most gifted minds was necessary to provide us with simple and expressive notation which, in nearly all parts of mathematics, enables even the less gifted of us to reproduce theorems which needed the greatest genius to discover. Each improvement in notation seems, to the uninitiated, but a small thing; and yet, in a calculation, the pen sometimes seems to be more intelligent than the user.

Jourdain claims that a good—that is, simple and expressive—mathematical notation, although not necessary to the practice of mathematics, enables even the less gifted of

[20] This may be too quick. Although natural language is not inherent written, it may be that one nonetheless needs the idea of writing in order to "objectify" one's thought as required in order to do contemporary mathematics. It is nevertheless true on this view that one reasons in natural language, or abbreviations of it.

[21] On the role of writing in mathematical reasoning see also Cajori (1929, 327–36), which collects together various historical sources on the role of notation in mathematics, beginning with a review (1909) that at once emphasizes the central importance of notation in mathematics and laments the failure of researchers to consider it.

us to reproduce theorems. But if so, then a collection of signs in a good mathematical notation does not merely *record* results as on the documentist view; it can actually *embody* the relevant bit of mathematical reasoning, put the reasoning *itself* before our eyes in a way that is simply impossible in written natural language.

We saw already in Chapter 2 that even in Greek mathematical practice we find chains of reasoning, for example, that to show that there is no largest prime, that are reported in natural language and make no essential use of any written marks. Such a proof depends, as we noted, not on the capacity to write and read but on the capacity to reflect on ideas. It involves, that is, reason in its discursive use in Kant's sense. Unlike, say, a calculation in Arabic numeration, the (written or spoken) proof that there is no largest prime does not embody or show the reasoning but only reports it. Although the words do communicate the argument, one is not reasoning *in* natural language in this case; it is not the *words*, written or spoken, that one attends to—not in the way one attends to a drawn diagram in working through a demonstration in Euclid, or to an equation in elementary algebra, or to Arabic numerals in the course of an arithmetical calculation. What one attends to are the relevant *ideas*, for instance, the idea of a number that is the product of a collection of primes plus one. The proof can be recorded and communicated in natural language, conveyed by means of it, but the proof is not *in* the words; the words that are used to convey it do not put the proof itself before one's eyes (or ears).

The ancient proof that there is no largest prime shows that mathematics is not essentially written. That proof could have been discovered, perhaps was discovered, long before there was any written natural language. And this is in any case something we might have expected. Systems of written signs such as the symbolic language of arithmetic and algebra that have been devised for mathematics were devised for mathematics that already existed; it would be impossible to design such a notation for mathematics without knowing at least some of the mathematics that the notation was designed to capture. But it also seems clear that at least some of the systems of signs that have been devised in mathematics—paradigm among them, Euclidean diagrams, Arabic numeration, and the symbolic language of Descartes' algebra—are enormously powerful vehicles of mathematical reasoning.[22] Although it is manifestly possible to discover significant mathematical results, that is, solutions to mathematical problems and proofs of theorems, independent of any system of written signs within which to work, it is also evident that such systems of signs *are* developed in mathematics and can enable results that could not (or could not so easily) be discovered without them. Systems of written marks seem, then, to be neither essential nor irrelevant to mathematical practice, at least if it is taken overall.

[22] Although Arabic numeration and Descartes' symbolic language are clearly designed, the figures one finds in Euclid do not in the same way seem designed. In this case, it seems more correct to say that ancient Greek geometers *discovered* how to use diagrams in demonstrations than that they *devised* a system of signs for the purpose.

Throughout history there have been "natural" calculators able to identify even very large primes or the solutions to quite difficult arithmetical problems without appeal to any sort of written language. Most of us need the Arabic numeration system, or something comparable, to perform such tasks. Similarly, although various algebraic results were discovered by gifted mathematicians before (in some cases long before) the development of an adequate symbolism of elementary algebra, most of us can understand those results only when they are expressed in the symbolic language of arithmetic and algebra, only when we can *see* the reasoning in the formula language. In these cases, mathematics that at first can only be reported, conveyed in natural language together with whatever else seems helpful such as sound effects and drawings, comes later to be displayed in a specially devised system of written signs. It seems furthermore clear that what the systems of signs capture in these cases is not, at least in general, the way the mathematicians themselves were thinking. Natural calculators do not calculate as one calculates in Arabic numeration—albeit somehow "naturally"; that system was not devised to mimic the reasoning of such people.[23] Instead the system of Arabic numeration provides a kind of rational reconstruction of the mathematics. It shows not how natural calculators reason, how they discover their results, but how the mathematical reasoning itself works (or can be seen to work), at least in this particular corner of mathematics. It lays out *the* reasoning as contrasted with some person's (processes of) reasoning.[24]

Mathematical languages such as the symbolic language of arithmetic and algebra enable non-mathematicians, and in some cases even mathematicians, to understand mathematics that would otherwise be wholly inaccessible. The point is not merely that the mathematics is harder without a system of signs within which to work but nonetheless perfectly possible. Such systems of signs in mathematics do not merely make things easier. They make things easier because they *embody* processes of mathematical reasoning—again, not processes of mathematicians' thinking but of mathematical thinking itself. *They display how the mathematics works*. And if that is right, then we can reconcile Rotman's and Thurston's views. Rotman is right: one can do mathematics, actually perform the reasoning, at least as a public, observable act, only in a specially devised system of written signs. But so is Thurston: in areas of mathematics for which an adequate expressive notation has not yet been devised, language, that is, natural language, whether written or spoken, supplemented with whatever signs from mathematics seem helpful, is invariably after the fact, serving

[23] We do not currently know how natural calculators achieve their results. But even if we did, this would be of no interest to mathematics, or to philosophy. The question how natural calculators do it is a question for psychology and cognitive science, not for philosophy. For a discussion of the phenomenon of "natural" calculation and some of the literature, see Fitzgerald and James (2007, ch. 1).

[24] Depending on the system, the given collection of marks may need to be supplemented with a commentary. We find this, for instance, in a demonstration in Euclid's *Elements*. The diagram is not in general fully intelligible (as a calculation in Arabic numeration is) without the accompanying text, even though, as we saw in Chapter 2, one reasons *in* the diagram in Euclid.

only to document or communicate results obtained independently. In such cases, although gifted mathematicians will still be doing mathematics, the act will not be public and observable. Mathematicians will not be able to show us how it goes but can only tell us, describe in words, what they have discovered and why it is true.

We have been exploring the idea that although mathematical reasoning can only be reported after the fact in natural language, whether spoken or written, that same reasoning can be embodied (at least in some cases) in a specially devised system of written signs, one that, as Jourdain notes, can require "the long and strenuous work of the most gifted minds" to be developed.[25] The way mathematicians actually write their proofs in current mathematical practice, together with the fact that formalizing such a mathematical proof in standard notation is unhelpful and often unintelligible, provides strong evidence that Jourdain's account of the role of writing in mathematical practice is indeed correct. Our mathematical logic, designed to abbreviate words of natural language, is not a language within which to reason, not a mathematical notation in Jourdain's sense at all.

On the mathematical logician's view, the only difference between a proof as formulated by a mathematician in actual practice, on the one hand, and that same proof fully formalized, on the other, is that the former has jumps in the reasoning whereas in the latter, formalized proof the jumps have been replaced by small steps that are manifestly instances of the rules of formal logic. On the alternative view now under consideration, a mathematician's chain of reasoning, although it can be reported after the fact in natural language, can be embodied, actually displayed as the reasoning it is, only in a specially devised system of signs. Already in Chapter 2, we distinguished between two different ways in which one might see for oneself in mathematics, either by thinking through the chain of reasoning for oneself (for example, in a bit of mental arithmetic) or by working it out for oneself in some paper-and-pencil reasoning. And this distinction in turn allowed us to distinguish between *reporting* a chain of reasoning (for instance, in mental arithmetic) in natural language and *displaying* some mathematical reasoning in a specially devised system of signs, as one does in Euclidean diagrammatic practice, or in a paper-and-pencil calculation in the positional system of Arabic numeration. Mathematical proofs as they are given today are, as already noted, generally reports of reasoning rather than displays of reasoning.[26] That is, instead of giving a chain of reasoning, whether or not one that has gaps or jumps in it, mathematicians describe how the proof goes, or

[25] It is important to keep in mind that, as we saw in Chapter 3, two systems of signs that can seem to be mere notational variants, such as Viète's notation and Descartes', can in fact reflect profoundly different conceptions of the role of the notation in the hands of a user. For Viète, the notation is a kind of convenient formalism; for Descartes it is an embodiment of a radically new form of mathematical reasoning. Similarly, our logical notations are a kind of convenient formalism. Frege's notation, which can seem on superficial inspection to be a mere notational variant, is (we will see) instead the embodiment of the radically new form of mathematical reasoning that was developed in the nineteenth century.

[26] Some examples of displays in contemporary mathematical practice, together with an analysis of how the notation is functioning, are provided in Carter (2010) and Carter (2012).

would go were one to work through it. As evidence Avigad (2006b, 131) offers this list of typical descriptive phrases taken from the opening chapter of a well-known undergraduate textbook, Birkhoff and Mac Lane's *A Survey of Modern Algebra* (1965):

- ... the first law may be proved by induction on n.
- ... by successive applications of the definition, the associative law, the induction assumption, and the definition again.
- By choice of m, $P(k)$ will be true for all $k < m$.
- Hence, by the well-ordering postulate ...
- From this formula it is clear that ...
- This reduction can be repeated on b and r_1 ...
- This can be done by expressing the successive remainders r_i in terms of a and b ...
- By the definition of a prime ...
- On multiplying through by b ...
- ... by the second induction principle, we can assume $P(b)$ and $P(c)$ to be true ...
- Continue this process until no primes are left on one side of the resulting equation ...
- Collecting these occurrences ...
- By definition, the hypothesis states that ...
- ... Theorem 10 allows us to conclude ...

Proofs written using such phrases are not themselves the reasoning but instead descriptions of how the reasoning would go were one actually to reason from the starting point to the desired theorem.

Thurston suggests that the way mathematicians think is different from the way they write. And as just indicated, this is borne out by the way they actually do write. The proof itself, the actual chain of reasoning, is not given in the writing but only described. And it can, if Thurston is right, be *best* described using all of the very wide variety of channels of communication that are available in real-time interpersonal communication, not only all the richness and nuance of natural language but also gestures, drawings, sound effects, and whatever else might help to convey the relevant ideas. The language of mathematical logic has, by design, none of this communicative richness and power. Its virtue lies instead in making perspicuous certain logical forms that natural language sentences can exhibit and in codifying rules of inference concerning those forms. It is not surprising, then, that the language of mathematical logic is woefully inadequate as a language within which to communicate mathematical ideas and to describe chains of mathematical reasoning. Nor, for reasons that are not yet clear, is it a good mathematical notation in Jourdain's sense, a notation within which to reproduce results by embodying the reasoning. Again, formalized proofs are unconvincing and generally unintelligible.

According to Thurston (1994, 47), actual mathematical reasoning is "qualitatively different" from the sort of rule-governed symbol manipulation that is involved in a proof in mathematical logic.

> Mathematicians can and do fill in gaps, correct errors, and supply more detail and more careful scholarship when they are called on or motivated to do so. Our system is quite good at producing reliable theorems that can be solidly backed up. It's just that the reliability does not primarily come from mathematicians formally checking formal arguments; it comes from mathematicians thinking carefully and critically about mathematical ideas.

Although one can formalize a mathematical proof in standard logical notation, such a formalization does not seem to capture the mathematician's reasoning, and as Thurston indicates, that is because the mathematician's reasoning is based on mathematical ideas. Rav (1999, 29) similarly argues that a mathematician's proof cannot be formalized because it "depends on an understanding and prior assimilation of the *meanings* of concepts from which certain properties follow logically." If this is right then a formalization in our standard logical notations cannot embody the reasoning because what needs to be expressed in the case of such reasoning is not merely logical form but *content*, the mathematical *ideas* that are of concern to the practicing mathematician. But what do *ideas* have to do with the validity of inference? According to the mathematical logician, the answer is: absolutely nothing.

To say that an inference is valid in virtue of form is at a minimum to say that it is an instance or application of something more general, of a schema or rule that applies not only in the given case but also in other cases that are relevantly like it. To use again our earlier example, the inference from 'Felix is a cat' to 'Felix is a mammal' is good, if it is good, because one can infer generally from something's being a cat that it is a mammal. The inference is not good in virtue of being about Felix. Though one does infer something about Felix, the validity of the inference is explained not by reference to Felix but by appeal to a rule, something to the effect that being a cat entails being a mammal. Similarly, the inference from 'John is in the kitchen or the den' and 'John is not in the den' to 'John is in the kitchen' is good, if it is good, because one can infer generally from something's being this or that and its not being that to its being this. The inference is not good in virtue of being about John and his whereabouts. Though one does infer something about John and his whereabouts, the validity of the inference is explained not by reference to John but by appeal to a rule, that known as disjunctive syllogism. Whereas in our Felix example the rule concerns valid inferences that rely on the meaning of 'cat', in the John example the rule concerns valid inferences that rely on the meanings of 'or' and 'not'. And just the same is true in all other cases of actual inference, whether material or formal; any actual inference is an instance of a rule that applies also in other cases.[27]

[27] Both Peirce (1992, 131) and Ryle (1950) emphasize this fundamental point.

Mathematical logic is committed to something *much* stronger than this, namely, a model theoretic conception of language according to which logical form is opposed to semantic content. One immediate consequence of that commitment is the idea that the Felix inference is not really a valid inference at all. The meaning of 'cat' plays, on this view, an essentially different role in reasoning from the meaning of 'not' or 'or'. More exactly, it plays no role at all in reasoning. To write a sentence in standard logical notation is, on this view, perspicuously to display its logical form, where logical form is alone what matters to inference. The particular contents (for instance, the content of the concept *cat*) do not, and cannot, come into it at all. Logical contents, such as those given by 'or' and 'not', are not (on this view) really contents at all but only form. Contents do not, and cannot, matter to the validity of a proof in language conceived model theoretically. But, again, particular contents *do* seem to matter in mathematical reasoning. Though any inference is valid in virtue of its form, mathematical reasoning seems to be reasoning from the contents of concepts, from mathematical ideas. If we are to understand how this is possible, we need to keep in mind that although there is a clear sense in which inferences are valid in virtue of their form—because they are instances of something more general, of a rule that can be applied also in other cases—it does not follow that language should be conceived model theoretically, in terms of a thoroughgoing distinction between logical form and content, that is, semantic content, content as it matters to meaning and truth.

The mathematical logician's view of the mathematician's proofs as merely gappy chains of reasoning seems not to be right, and it seems not to be right at least in part because one does not reason *in* natural language in the same sense in which one reasons *in* the essentially written systems of signs that have been developed for mathematical reasoning. Of course there is a sense in which an English speaker reasons in English, a Chinese speaker in Chinese, and in general that we reason in the natural languages that we speak. But as Kant's distinction between two uses of reason, a discursive and an intuitive use, already indicates, the sense in which one reasons in natural language is very different from the sense in which one reasons in, say, Descartes' symbolic language. In the *latter* sense of reasoning in a language one does not reason *in* natural language at all but only reports reasoning. Current mathematical practice seems to suggest exactly that.

6.5 The Leibnizian Ideal of a Universal Language

We began with the idea that mathematics is done in a (perhaps somewhat regimented) natural language, that reasoning is everywhere the same and can be codified in the abbreviations of standard quantificational logic. We have since found reason to call into question not only the assumption that reasoning is everywhere the same but also a constellation of ideas that are generally taken for granted in this logic: that generality, whether in mathematics or in the empirical claims of everyday life, is to be understood quantificationally; that language is to be understood model theoretically;

and that meaning as it matters to inference is given by truth conditions, what is the case if a sentence is true. We turn now to an idea that is in some tension with the thought that the reasoning in mathematics is done in natural language, namely, the thought (explicit in Russell 1903) that standard mathematical logic finally realizes the old Leibnizian ideal of a universal language. Nothing, we will soon see, could be further from the truth.

As is well known, Frege describes his *Begriffsschrift*, or concept-script, as realizing the Leibnizian idea of a language that is at once a *calculus ratiocinator* and a *characteristica*. These expressions have since been employed in a very influential essay (van Heijenoort 1967) in an attempt to clarify some central strands in the history of the development of modern mathematical logic. Because, as is slowly becoming known, these expressions do not mean in that essay what they meant either for Frege or for Leibniz before him, we begin with the Leibnizian idea, at least as Frege understood it.

The idea of a universal characteristic, which Leibniz first explores in his "Dissertation on the Art of Combinations" (1666), goes back at least to Descartes.[28] As Descartes explains in a letter to Mersenne, written on November 20, 1629, "in a single day one can learn to name every one of the infinite series of numbers, and thus to write infinitely many different words in an unknown language. The same could be done for all other words necessary to express all the other things which fall under the purview of the human mind" (CSM III 12; AT I 80). To learn the system of Arabic numeration is to learn to write any natural number and although such a written numeral will be read, that is, pronounced, differently in different languages—for instance, the numeral '84' is read in English as 'eighty-four' but in French as 'quatre-vingt-quatre'—it will be *written* the same way for all. So, Descartes suggests, the same could be done for all the words of natural language. But, he cautions,

> the discovery of such a language depends upon the true philosophy. For without that philosophy it is impossible to number and order all the thoughts of men or even to separate them out into clear and simple thoughts.... If someone were to explain correctly what are the simple ideas in the human imagination out of which all human thoughts are compounded... I would dare hope for a universal language. (CSM III 13; AT I 81)

The realization of a universal language not only of our ideas of numbers but of all our ideas depends on an adequate analysis of those ideas into their primitive elements, the "simples" of thought, and hence, Descartes thinks, on "the true philosophy."

The ways in which words of natural language are formed from sounds and from other words clearly bear no internal, systematic relationship to the ways the concepts or ideas signified by the words are formed from simpler concepts or ideas. But perhaps a system of written signs could be devised in which there was such a

[28] For references to similar ideas in other seventeenth-century thinkers see Capozzi and Roncaglia (2009, 113).

relationship. Such a system of written signs would involve simple signs for primitive concepts and complex signs (that is, simple signs in various combinations) for other concepts that would show in their composition how the contents of those (non-primitive) concepts depend on, or can be seen to be built up out of, the contents of the primitive concepts. The crucial idea here is that of *showing* a content in a composition of signs. The task is not merely to say what is the case if the concept applies, that is, set out necessary and sufficient conditions for its correct application, but to show its content, to display it much as, I have argued, a drawn circle in a Euclidean diagram displays the content of the concept *circle* (as that concept is understood in Euclid), and as '$a^2 + b^2 = c^2$' displays a relation among arbitrary quantities in Descartes' language. Consider, for example, the concept *prime number*. The content of this concept is, let us say, that no other number (save the number one of course) will divide it without remainder. But, again, the task is not merely to *say* what this content is, either in natural language or in some abbreviated version of it, but to *show* that content in signs, to show it as we show the (arithmetically articulated) content of the concept *product of two sums of integer squares* in elementary algebra: $(a^2 + b^2)(c^2 + d^2)$.

This expressive project is, however, only half the story, as Leibniz clearly saw. A universal language properly speaking has not one but two fundamental, and fundamentally related, aspects.

Consider again the positional, decimal system of Arabic numeration. This system of signs not only shows how to formulate the contents of numbers in a systematic way by combining signs for the "primitive" numbers, zero to nine, it also provides rules governing the manipulation of signs in an arithmetical calculation. A universal language as Leibniz envisages it similarly would have two fundamental aspects. It would show how to formulate the contents of (non-primitive) concepts in a systematic way by combining signs for the primitive concepts, and it would provide rules governing the manipulation of signs in a chain of reasoning. It would in this way be at once a *characteristica*, a language within which to display the way concepts are formed from other concepts, and a *calculus ratiocinator*, a calculus of reasoning. As Korte (2010, 286), following Trendelenburg, Květ, and Exner, puts the essential idea, "because structures of expressions in a *lingua characteristica* characterize structures of concepts, it makes possible to construct a mechanical calculus, a *calculus ratiocinator*, in which signs could be used as substitutes of concepts, thus turning thinking into calculating."[29]

[29] In 1843 Franz Exner published a study of Leibniz's *Universal-Wissenschaft*, and in 1857 F. B. Květ's *Leibnitzen's Logik* appeared. But it was Friedrich Adolf Trendelenburg's essay "Über Leibnizens Enwurf einer allgemeinen Characteristik," ("Leibniz's Sketch of a Universal Characteristic") first presented in 1856 at the Berlin Academy of Science, then published in 1857, that would prove most influential. Trendelenburg's essay seems to have been the source, both for Frege and for Schröder, of the Leibnizian idea of a universal language. See Peckhaus (2009).

Korte (2010, 286), following Trendelenburg, makes another important observation as well, namely, that the *calculus ratiocinator* of a proper universal language needs a *characteristica* in order "to express premises from which proofs of its theorems could advance." Korte (2010, 287) quotes from Trendelenburg explaining Leibniz's idea:

> Every proof presupposes definitions. As a matter of fact, the ultimate principles are definitions and statements of identity, i.e., judgments which are proved analytically from the identity of concepts. The important thing is that we form suitable definitions by using sign formulas [of the characteristica] so that they can form a foundation to the inferring calculus, calculus ratiocinator.

What is meant by a definition here is not a set of axioms. The idea that axioms might be treated as (implicit) definitions is a late nineteenth-century development, not something one might find in Leibniz. For Leibniz, axioms are primitive truths, not definitions. Hence, we need to take the reference to definitions in this passage from Trendelenburg to be a reference to the more traditional notion of definition in which the meaning of a concept is laid out. The claim, then, is that proofs begin with definitions as contrasted with axioms, that is, with explicit definitions; it is definitions, not axioms, that provide the premises of proofs, at least on this view.

It is also worth noting that although Kant held in the *Inquiry* (1764) that there could be no such thing as a concept-script of the sort that Leibniz intended but only an "intuition-script" such as the language of arithmetic and algebra (as Kant understands it), Kant later seems to have had second thoughts. In a letter to his trusted expositor Johann Schultz written on August 26, 1783, Kant writes:

> This [the fact that the third category is derivable on the basis of the first two in Kant's table of the pure concepts] and other properties of the table of categories that I mentioned seem to me to contain the material for a possibly significant invention, one that I am however unable to pursue and that will require a mathematical mind like yours, the construction of an *ars characteristica combinatorial*. If such a thing is at all possible, it should have to begin principally with the same elementary concepts. . . . Perhaps your penetrating mind, supported by mathematics, will find a clearer prospect here where I have only been able to make out something hovering vaguely before me, obscured by fog, as it were. (Kant 1999, 207; AK 10:351)

Having, so he thinks, identified the fundamental concepts of the pure understanding through his reflections on the forms of judgment (in the *Critique of Pure Reason*), Kant thinks he has identified the primitives on the basis of which to develop a Leibnizian *characteristica*. What Kant is clearly finding it much harder to envisage is a way to construct signs and modes of their combination as required fully to realize an *ars characteristica combinatorial*, that is, a Leibnizian universal language.[30]

Whether or not Kant took arithmetic to be the model for a universal language, Descartes certainly did, and so did Leibniz. Complex signs for (complex) concepts

[30] The Kantian Ludwig Benedict Trede apparently tried to complete this project in his *Proposals for a Necessary Theory of Language* (1811). See Sluga (1980, 51).

were to be constructed out of simple signs for (simple) concepts as signs for larger numbers are constructed out of signs for smaller numbers. Those signs are then to be manipulated according to rules as numerals are manipulated in an arithmetical calculation. Reasoning was thus to be conceived as a kind of reckoning—which can but need not be conceived mechanistically. But arithmetic has no definitions, and definitions, we have seen, are critical to the Leibnizian idea. Because they are it is not simple arithmetic but instead eighteenth-century mathematics that provides a good model of what is needed in a Leibnizian universal language.

The primitive signs of the language are the ten digits, the signs for the basic arithmetical operations (addition, subtraction, and so on), the sign for equality, letters lending generality of content (however exactly they work to do that), and parentheses. Familiar axioms (commutativity and associativity of addition and multiplication, distribution, and so on) set out the basic rewrite rules in the system, and theorems derived from those axioms provide further, derived rewrite rules, for instance: $(a + b)^2 = a^2 + 2ab + b^2$. We also assume that we have the familiar rewrite rules of the calculus, for instance, that the derivative of x^n is nx^{n-1}. Complex signs for functions are to be constructed out of the primitive signs of the language and set out in explicit identities that provide the starting point for proof. To see more concretely how this works, we consider (a sketch of) a proof of what is known simply as Euler's Theorem: $e^{ix} = \cos x + i \sin x$.[31]

We begin by defining the trigonometric functions sine and cosine as power series.

$$\sin x = x - x^3/3! + x^5/5! - x^7/7! + \ldots$$
$$\cos x = 1 - x^2/2! + x^4/4! - x^6/6! + \ldots$$

This is not how such functions were originally conceived; but given our original understanding of those functions, it makes good mathematical sense to think of them as given in these identities.[32] The signs on the left, the definienda, are simple names for the relevant functions, which are, of course, of antecedent mathematical interest. These same functions are given again in the definientia on the right, but this time through complex, arithmetically articulated collections of signs. (Of course, this content is not fully expressed insofar as the two series are infinitely long; but enough of them is given that one can see how to extend them arbitrarily far.) Furthermore, because the definientia are complex in the particular ways they are, the rewrite rules of algebra as codified in its axioms and theorems can in principle be applied to them in the course of a problem or proof. Obviously no such rules can be applied to the

[31] This theorem is the basis for Euler's famous equation that $e^{i\pi} + 1 = 0$; Euler proves it in chapter 8 of his *Introductio in analysin infinitorum* (1748). The proof sketched here follows Smith (2007, ch. 3), "Proof of Euler's Identity."

[32] Strictly speaking, these identities are not thought of as definitions in mathematical practice, but instead as theorems. This mathematical point does not affect the philosophical point aimed at here. For our purposes, what matters is that we have an identity that involves both a simple sign for a function and also a complex sign designating that same function.

simple signs on the left because there is no arithmetical articulation in those signs. The expressions 'sin x' and 'cos x' serve merely as labels for certain trigonometric functions.

What we want to establish is the truth of Euler's theorem that $e^{ix} = \cos x + i \sin x$, universally regarded as a paradigm of an interesting and significant mathematical result. One way to prove it is this. We know that the number e is, by definition, the number such that the derivative of e^x is just e^x. (It can be shown that this number must exist.) Given that e^x is its own derivative (and certain other assumptions that will not concern us here), it follows that:

$$e^x = 1 + x + x^2/2! + x^3/3! + x^4/4! + \ldots + x^n/n! + \ldots$$

And we can see that this must be right because if we take the derivative of the infinite series on the right we will end up with just the same series again. The derivative of the first term is zero, of the second is the first, of the third is the second, and so on. Because there are infinitely many terms, the derivative of the whole will simply be the same series again. If, now, we replace 'x' in this equality with 'ix', where i is by definition such that $i^2 = -1$, we get:

$$e^{ix} = 1 + ix + (ix)^2/2! + (ix)^3/3! + (ix)^4/4! + (ix)^5/5! + \ldots$$

Now we do some standard algebraic manipulations as governed by the axioms and theorems of elementary algebra:

$$e^{ix} = 1 + ix - x^2/2! - ix^3/3! + x^4/4! + ix^5/5! - x^6/6! - \ldots$$

And rearranging things a bit, by collecting together the terms that contain i and separating i out (again, as permitted by the rewrite rules), gives:

$$e^{ix} = (1 - x^2/2! + x^4/4! - x^6/6! + \ldots) + i(x - x^3/3! + x^5/5! - \ldots).$$

The two infinite series for our trigonometric functions just *pop up* when we rewrite as needed.[33] Because the first series is identical with the cosine function and the second is identical with the sine function, putting equals for equals gives us Euler's famous theorem relating the exponential function to these trigonometric functions.

As this example illustrates, mathematical identities involving both simple signs and arithmetically complex signs can play a *crucial* role in a demonstration. Without the simple signs 'e^x', 'sin x', and 'cos x' we would not be able to recognize our result *as* a result involving the exponential function and the two trigonometric functions; and without the power series with which these three functions were identified, we would

[33] Here, again, we clearly can see the three levels of articulation at work in the system of signs: first, the primitive signs, then, the complex expressions designating functions, and finally, the whole formula. It is because one can rewrite parts of formulae, much as one reconceives parts of Euclidean diagrams into new wholes, that new complex expressions for functions can pop up in algebraic reasoning—much as figures do in Euclidean diagrammatic demonstration.

be unable to reason our way *to* the desired result. The example thus illustrates the way in which finding a fruitful articulation of some mathematical notion, one that will enable the proof of some result, can constitute a real mathematical advance. Of course, the simple signs are eliminable in the demonstration in the sense that replacing all instances of them in the course of reasoning with the relevant complex signs will not affect the validity of the reasoning. But equally obviously, doing that destroys the interest of the demonstration insofar as it is no longer possible to see the demonstration as a demonstration involving the exponential and trigonometric functions. What one would end up with in that case would be a trivial identity, one with the same power series on both sides of the identity sign. Euler's theorem is mathematically significant *because* it reveals a connection between two apparently independent mathematical domains, that of exponential functions and that of trigonometric functions. But we can *see* this only if we have the identities of the exponential function and of the trigonometric functions with which we began, *both* the complex signs, the power series, *and* the simple signs 'e^x', '$\sin x$', and '$\cos x$'.

By displaying the contents of mathematical notions of antecedent interest in new and mathematically tractable ways in a Leibnizian universal language, identities enable demonstrations involving those notions, and they can do because, as we have just seen, in the identities those contents are displayed in complex signs in ways that enable reasoning. In these identities, both the simple sign and the complex sign designate or mean one and the same thing; they have the same *Bedeutung*. But, again, they do not express the same sense; they have *quite* different roles to play in the process of reasoning. As we can think of it, although the two signs, one simple and one complex, have the same meaning in the sense of truth conditions, they do not have the same meaning in the sense of inference potential. Different things follow from the simple and the complex signs precisely because the one is a simple sign and the other is a complex sign with a lot of internal articulation that can be utilized in the proof. We thus have here yet one more instance of an intelligible unity, something that has independently meaningful parts but is *also* a whole that is not merely reducible to its parts.

I have suggested that we think of axioms, for instance, those of elementary algebra, as ascriptions of basic properties to arithmetical operations such as addition and multiplication. Theorems derived from those axioms ascribe further properties to those operations, and both those axioms and the theorems derived from them can then be treated as basic and derived rules governing valid inferences. We have looked at two sorts of cases, one that did not involve any identities of simple and complex signs (our little demonstration, in section 6.2, of the theorem that if two numbers are both sums of integer squares then their product is also a sum of integer squares) and another in which such identities have an essential role to play, namely, in the demonstration just rehearsed of Euler's theorem. In both sorts of cases the reasoning is, in principle, fully rigorous; every step in the reasoning is justified by some antecedently specified (or specifiable) rule. But the reasoning is also fully contentful. The formula language of arithmetic and algebra enables the expression of a content in a way that is mathematically tractable, that is, in a way enabling one to reason (on the basis of content) *in* the system of signs. It illustrates in this way just how a Leibnizian universal language, one that is at once a *characteristica* and a *calculus ratiocinator*, is to work and thereby how a fully rigorous chain of reasoning can be fully compatible with reasoning on the basis of mathematical ideas.

Taking our cue from the formula language of arithmetic and algebra as it is employed in the proof of Euler's theorem, we can now ask what would be required of such a language for the practice of reasoning deductively from defined concepts, *Denken in Begriffen*, thinking in concepts, that arose in the nineteenth century. We know that because there was no mathematical language, no system of written signs within which to do this work, proofs had to be reported in natural language together with whatever help could be provided by the formula language of arithmetic and algebra. But it is nonetheless possible to say what such a system of signs would have to do. First, and most obviously, it would have to formulate, set out or display, the inferentially articulated contents of the concepts of mathematical interest in a way that enables mathematical reasoning. And we know that the content must be *inferentially* articulated, rather than arithmetically articulated as in our Euler example, because we aim to reason *deductively* from that content. Furthermore, again because the reasoning involved in this case is deductive (rather than computational and algebraic, or diagrammatic), the contents of those concepts would need to be displayed in complex signs in a way enabling rules of inference to be applied to them. That is, much as in elementary algebra we introduce a few primitive signs for the basic arithmetical operations, so here we would need to introduce a few primitive signs for the basic logical relations. We would then need to formulate axioms, corresponding to the axioms of algebra, that would set out the basic properties of those primitive logical relations, and to derive some theorems from those axioms. As in algebra, the axioms and derived theorems would then function as the rules governing inferences in the system. Definitions of concepts of antecedent mathematical interest could then be formulated in the system and, as in the proof of Euler's Theorem, those definitions could provide the starting point for proofs. The proofs would use the basic and derived rules of inference (as codified in the axioms and theorems) to proceed step by step from the definitions to the desired theorems. The system would be at once a *characteristica* insofar as the contents of (non-primitive) concepts would be set out in complex signs of the language and a *calculus ratiocinator* insofar as all steps in the reasoning would be governed by rules, either primitive rules as formulated in axioms of the system or derived rules expressed in theorems derived from the axioms. Frege's *Begriffsschrift*, we will see, is just such a language, at once a *characteristica* and a *calculus ratiocinator*.

6.6 Conclusion

Our aim is to understand the practice of mathematics, both in general and as it has come to be practiced since the nineteenth century in particular. What we are after is insight into how mathematics works as a mode of inquiry, which requires in turn a cogent account of mathematical truth in relation to mathematical knowledge. And for much of the twentieth century it was thought that developments in mathematical logic had provided the essentials of such an account, that what remained was only to secure the foundations. It has become increasingly clear that this is not so. We have here tried to clarify *why* that logic cannot provide what is needed.

The problems do not lie in the rules of inference. Instead the problems arise because we make substantial metaphysical assumptions in logic, both about the nature of thinking and about the nature of language. It is these assumptions that once made explicit can be shown to be problematic. The model theoretic conception of language, with its notion of a non-logical constant, its conception of generality, and its understanding of meaning in terms of truth, is not obvious: it took many, many decades and a great deal of hard work on the part of many brilliant minds to hammer out this conception of language. And even now that it has been fully worked out, it is hardly intuitive as an account of language. Technically it has many virtues. Philosophically it has proven a dead end. Mathematical logic does not provide, or even enable, the understanding we seek.

Although developments in mathematics in the nineteenth century seemed decisively to show that Kant's account of mathematical practice in terms of the construction of concepts in pure intuition was wrong, those developments did not show precisely how Kant's account was wrong. One alternative, the less radical, was to think that what was needed to understand mathematical practice was not Kant's account of the intuitive use of reason, but instead something akin to his understanding of (general) logic and deductive reasoning. Although not understood in just those terms, this was the dominant path of philosophers in the twentieth century; although not for Kantian reasons (at least not overtly), most work over the course of the twentieth century was based on fundamental assumptions that are essentially those we find in Kant: an absolute distinction between logical form and semantic content, and an understanding of meaning in terms of truth—and by way of an understanding of quantifiers, of truth ultimately about objects. And one consequence of all this, beginning with Boole's treatment of logic, has been an essentially mechanistic conception of reasoning, an understanding of it as nothing more than the rule-governed manipulation of signs with no regard for meaning. But this idea too has come, over the course of the twentieth century, more and more to be called into question. As we have seen, meanings and mathematical ideas seem to be essential in actual mathematical practice. Perhaps, then, *real* mathematics is not what we can build a computer to do but something only a rational animal can do. To recognize this possibility is to give up the standpoint of *Verstand*, to adopt instead the standpoint of *Vernunft*.

Reason

Our aim is to understand our capacity for knowledge of things as they are in themselves, first and foremost in mathematics. And we now have almost all the resources we need in order to achieve this. All that remains is to introduce the mathematical language that Frege devised for the new mathematical practice of reasoning deductively from concepts that arose in nineteenth-century Germany, and to take the needed philosophical lessons from it.

We have seen that whereas ancient mathematical practice poses for philosophy the problem of the being of its objects, and early modern mathematics poses instead the problem of the nature and possibility of synthetic a priori judgments, the nineteenth-century mathematical practice of reasoning deductively from concepts poses the problem of the nature and possibility of ampliative deductive proof. The task, in each case, is the same: to understand the characteristic unity, first, of mathematical objects, then, of mathematical propositions, and finally, of mathematical proofs, where this unity is neither an essential unity, the parts of which can be understood only in relation to the whole, nor an accidental unity, the parts of which are fully intelligible prior to and independent of the whole. What we are concerned with, in each case, is an *intelligible* unity, a whole of independently intelligible parts that is nonetheless not reducible to its parts.

Consider, one last time, a Euclidean equilateral triangle the construction of which is given in the first demonstration of the *Elements*. The triangle is a real whole, one figure, the parts of which are nonetheless independently intelligible. The side, for instance, is a straight line and straight lines are of course intelligible independent of triangles. But the straight line *is also* a side, and like the parts of an essential unity, the side *qua* side is intelligible only in light of the triangle of which it is a side. Similarly, we can think of an analytic judgment in Kant as an essential unity insofar as the predicate is contained already in the concept of the subject; and we can think of an a posteriori judgment as an accidental unity insofar as there is no internal relation between the concept of the subject and the predicate in an a posteriori judgment. Synthetic a priori judgments are different again; they are real wholes in virtue of the necessary relation of the predicate to the concept of the subject, but they also have real, independently intelligible parts. And so it is for the case of reasoning that now concerns us. Where reasoning is merely explicative, a matter of making explicit something contained already in the starting points of the proof, the whole chain of reasoning forms what we can think of as an essential unity. Where the reasoning is instead non-deductive, as in, say, inference to the best explanation, the unity of the whole is only accidental insofar as the steps of reasoning are not intrinsically truth-preserving. In the case of interest, then, we have a deduction, each step truth preserving, but not a deduction in which the conclusion is contained already in the premises. If we are to understand this, clearly we cannot simply assume a containment model of deduction, as if all deduction were strictly logical deduction.

Much as Kant's conception of the synthetic a priori required him to replace Hume's division of all knowledge into relations of ideas and matters of fact with two different distinctions, the analytic/synthetic distinction and that of a priori and a posteriori judgments, so to understand the possibility of ampliative deductive proof, we will see, requires a distinction of distinctions. In place of Kant's division of concepts and intuitions, Frege will put two distinctions, both that of *Sinn* (sense) and *Bedeutung* (meaning or signification), and that of (Fregean) concept and object. That is, much as from Kant's perspective, the ancient notion of a term conflates the logical function of referring with the logical function of predicating, so from Frege's perspective, Kant's division of intuitions and concepts conflates, on the one hand, the notion of an object with that of objectivity, and on the other, the notion of a concept with that of cognitive significance. It is just this distinction of distinctions that we need in order to understand the nature and possibility of ampliative deductive proof. And to show in this way that and how ampliative deductive proof is possible, we will see, *just is* to show that pure reason has been fully realized as a power of knowing. Pure reason, from being merely a critically reflective capacity, has become the power to know things are they are in themselves.

And once we have all this, we will be in a position, finally, to articulate a stable conception of our cognitive relationship to things in the world more generally. To distinguish as Frege does between, on the one hand, the *Sinn*/*Bedeutung* distinction, and on the other, the division of concept and object, and properly to understand their relationship one to the other, just is to achieve the standpoint of reason and thereby fully to realize the aspirations of modernity in an intentional directedness on reality that explicitly takes the form of a mediated immediacy, both in our everyday experience of empirical reality and in our scientific investigations into the fully objective nature of that reality. It is fully to realize the power of reason as a power of knowing and finally to understand the striving for truth.

7

Reasoning in Frege's *Begriffsschrift*

Over the course of the nineteenth century, mathematics was, for the second time in its long history, fundamentally transformed to become, as it remains still today, a practice of deductive reasoning on the basis of defined concepts.[1] Such a practice clearly does not need any written system of signs within which to work. Indeed, this is evident already in Euclid's *Elements*, for instance, in the proof that there is no largest prime. But we have also seen that absent such a system of signs the reasoning can only be reported after the fact. It cannot be displayed. Indeed, it is not at all obvious that in such a case there *is* anything that is *the* reasoning as contrasted with particular private acts of reasoning on the part of individual thinkers. And insofar as there is nothing that is the reasoning (as opposed to my or your reasoning) there is nothing of which it might be asked how it works as reasoning. If we are to understand the new mathematical practice that emerged over the course of the nineteenth century we need to be able to see it at work, that is, make it public and observable. And for this we need what according to Jourdain any good mathematical notation provides, a means of reproducing theorems *in* the system of signs.

We are already familiar with two such systems within which to reason in mathematics, namely, Euclidean diagrams and the symbolic language of arithmetic and algebra. Gottlob Frege, a nineteenth-century German mathematician working in the Riemannian tradition (see Tappenden 2006), provides us with a third. Frege's *Begriffsschrift*, or concept-script, was explicitly designed as a notation within which to reason deductively from concepts in mathematics. Modeled on the formula language of arithmetic, as Frege tells us in the subtitle of the 1879 logic, his system of written signs was to do for the newly emerged practice of *Denken in Begriffen* what earlier systems of signs in mathematics had done; it was to enable one to set out on paper the reasoning that is involved in actual mathematical practice. If we are to understand the practice, we need to learn to read the language that displays it.

We have seen that a principal aim of nineteenth-century mathematics, explicit in, for instance, Bolzano's work, was to show that intuition in Kant's sense has no role to play in arithmetic and analysis. And for Frege, as for many of his contemporaries,

[1] This is not to say that there is not still work being done in the older, computational style using the formula language of arithmetic and algebra. Nevertheless, the paradigm of current mathematical practice is that of deductive reasoning from concepts in the style of Riemann and Dedekind.

banishing intuition from arithmetic and analysis seemed at the same time to suggest that arithmetic is after all a purely logical discipline, that its most basic principles are purely logical and all its concepts definable by appeal only to strictly logical primitives.[2] But Frege provides two other reasons as well for thinking that logicism is true. The first concerns the domain of application of arithmetic, the fact that "the truths of arithmetic govern all that is numerable. This is the widest domain of all; for to it belongs not only the actual, not only the intuitable, but everything thinkable" (Frege 1884, sec. 14; cf. 1885b, 112). If arithmetic, like logic, governs the widest possible domain then perhaps it really just is logic. Frege's second reason for thinking that logicism is true is the fact that, unlike the axioms of geometry, which can be denied without contradiction (however much that denial might conflict with our intuitions about the nature of space), it is not possible in the same way to deny a basic law of arithmetic: "try denying any one of them, and complete confusion ensues. Even to think at all seems no longer possible" (Frege 1884, sec. 14). Again it seems natural to conclude that perhaps arithmetic is, then, really logic. Frege's self-appointed task was definitively to show that arithmetic is "simply a development of logic, and every proposition of arithmetic a law of logic, albeit a derivative one" (Frege 1884, sec. 87).

But logicism requires more than showing that the fundamental axioms of arithmetic are purely logical. It requires showing as well that "there is no such thing as a peculiarly arithmetical mode of inference that cannot be reduced to the general inference-modes of logic," and that all concepts of arithmetic are "reducible to logic by means of definitions" (Frege 1885b, 113 and 114). Indeed, Frege suggests, it is only by such a reduction of the concepts of arithmetic to the concepts of logic that it is "possible to fulfill the first requirement of basing all modes of inference that appear to be peculiar to arithmetic on the general laws of logic" (Frege 1885b, 114; cf. 1884, sec. 4, and 1914, 209). In order, for example, to show that the law governing mathematical induction is a purely logical law, one needs to define the notion of following in a sequence by appeal only to strictly logical primitives (which Frege does in the 1879 logic). Frege's task was to complete such a reduction generally, for all axioms, all rules of inference, and all concepts that appear to be peculiar to the science of arithmetic, algebra, and analysis.

Frege's logicist program failed. But what undermined it was not the attempted reduction of the axioms, inference rules, and concepts of arithmetic, algebra, and analysis. What wrecked Frege's logicist program was Frege's attempt to introduce logical objects to serve as numbers. Because he thought he needed logical objects for numbers to be—although in fact contemporary mathematics requires only *concepts* of numbers of various sorts—Frege introduced an axiom in *Grundgesetze*, Basic Law V,

[2] As Ferreirós (1999, 17) remarks, this "logicism was typically a German trend that makes full sense against the background of a 19th-century epistemology permeated by Kantian presuppositions. Logicism was a reaction against the specific Kantian theory of the origins of mathematics, a reaction based upon and favoring, the abstract tendencies that became quite evident after mid-century."

that entails (or would entail were it true) that corresponding to every logically permissible concept—that is, to every concept that has "sharp boundaries" in the sense of delivering a truth value, the True or the False, no matter what is given as argument—there is a course of values, which is an object. Courses of values were to be the logical objects that are numbers. Russell's paradox shows that Basic Law V leads to a contradiction. There are no logical objects but only logical concepts.[3]

Frege introduced Basic Law V in the first volume of his *Grundgesetze* (published in 1893) and in connection with it also made various alterations to the notation that he had introduced in the 1879 monograph *Begriffsschrift*. To jettison Basic Law V is to return to the earlier system of notation, that of the 1879 logic, and to the form of reasoning it supports.[4] Our concern here is to understand how reasoning in that notation works. And we will understand this, according to Frege, only if we understand the reasoning of Part III of *Begriffsschrift*. In Part I Frege introduces and explains his primitive logical signs, the concavity, the conditional and negation strokes, and the sign for identity. He gives a preliminary characterization of the peculiar logical roles of the content and judgment strokes, and indicates the distinctive role his Latin italic letters play in lending generality of content. He also sets out his one rule of inference.[5] Part II introduces nine axioms of pure logic: three concerning the conditional stroke, three concerning the negation stroke, two involving the sign for identity (equivalence), and one setting out the fundamental logical significance of Frege's concavity used in the expression of generality. Frege also, in Part II, derives various theorems on the basis of those axioms using his one rule of inference. As he later indicates, and is in any case apparent in *Begriffsschrift*, the theorems that are derived in Part II are made with an eye to their employment in the derivation in Part III of theorem 133. With a few small exceptions "to cater for Aristotelian modes of inference" (Frege 1880, 38), Frege derives in Part II only those theorems of logic that are needed in the Part III derivation. Four definitions are introduced in Part III and they provide the basis for Frege's proof of theorem 133 exhibiting a logical relation among three of Frege's four defined concepts: if the procedure f is single-valued, and m and y follow x in the f-sequence, then y either belongs to the f-sequence beginning with m or precedes m in the f-sequence. According to Frege, it is this derivation together with the various subsidiary derivations that it involves that "are meant to give a general idea of how to handle" his concept-script (1879, sec. 23). It is not Frege's derivations of theorems on the basis of axioms of logic in Part II of the 1879 logic but instead Frege's derivation of theorem 133 from his definitions in Part III that illustrates how reasoning

[3] See chapter 5 of my (2005) for discussion of Russell's paradox and what it shows in relation to Frege's work.

[4] It is sometimes claimed that the failure of Basic Law V infects also Frege's system of logic, but this is simply not true. Such claims are based on a mistaken (if very widespread) understanding of the notion of logical generality that is in play in Frege's language.

[5] These various features of Frege's concept-script are explored at length in my (2005).

works in his language. If we are to understand Frege's system of signs, we need to understand this derivation, and how it differs from the derivations of Part II.

7.1 The Idea of a *Begriffsschrift*

Frege's *Begriffsschrift* was to be at once a Leibnizean *characteristica* and a *calculus ratiocinator*, "both, with equal emphasis" (Frege 1897, 242). We have seen already, at least in outline (in section 6.5), what such a language would have to do. It would have to set out the inferentially articulated contents of mathematical concepts in a mathematically tractable way, that is, in a way enabling rigorous deductive reasoning in the system of signs. Frege's language was to do just this and so to be a language for the sort of mathematical reasoning from mathematical concepts that was then becoming standard. And although Frege at times seems to suggest that the language is equally applicable to reasoning in other contexts or in regard to other sorts of concepts, no such assumption will be made here. The task is to understand *Begriffsschrift* as a *mathematical* language, that is, as similar in essential respects both to the language of Euclidean diagrams and to the language of early modern algebra.

We saw that Descartes came to read the symbolism of algebra, which for Viète was only a convenient formalism, as a fully meaningful language within which to reason through a metamorphosis in the way he regarded drawn Euclidean figures. Frege, we will see, similarly comes to read equations in Descartes' language in a new way and discovers thereby a new kind of language within which to exhibit the contents of concepts. But although both effect in this way a radical transformation in how some inscription or collection of marks is to be regarded, there is also a very significant difference between the two cases. Whereas Descartes learned to do a new kind of mathematics with a new sort of subject matter *through* learning to see in a new way, Frege had *already* the new form of mathematical practice of the nineteenth century. And of course he also had already a mathematician's familiarity with the symbolic language of arithmetic and algebra due ultimately to Descartes. His thought was to achieve something similar to that language for the *Denken in Begriffen* tradition in nineteenth-century German mathematics, to devise a formula language of pure thought modeled on the formula language of arithmetic.

In natural language,

> there is only an imperfect correspondence between the way words are concatenated and the structure of the concepts. The words 'lifeboat' and 'deathbed' are similarly constructed though the logical relations of the constituents are different. So the latter isn't expressed at all, but is left to guesswork. Speech often only indicates by inessential marks or by imagery what a concept-script should spell out in full. . . . A lingua characterica ought, as Leibniz says, *peindre non pas les paroles, mais les pensées*. (Frege 1880, 12–13)

And, again, already devised languages of mathematics provide for Frege a model of what is wanted in such a Leibnizian language: "The formula languages of mathematics come much closer to this goal, indeed in part they arrive at it". But, as Frege goes on,

> at precisely the most important points, when new concepts are to be introduced, new foundations laid, it has to abandon the field to verbal language, since it only forms numbers out of numbers and can only express those judgments which treat of the equality of numbers which have been generated in different ways. But arithmetic in the broadest sense also forms concepts—and concepts of such richness and fineness in their internal structure that in perhaps no other science are they to be found combined with the same logical perfection. And there are other judgments which arithmetic makes, besides equalities and inequalities. (Frege 1880, 13)

Introducing new concepts, laying new foundations, making mathematical judgments that do not have the form of equalities or inequalities, in short, doing mathematics in the new style championed by Riemann instead of in the old style of Euler and Gauss, had meant abandoning the formula languages of mathematics, and with them all the virtues they possess. The task was to devise a new mathematical language, more exactly, to extend the existing language in a way that would enable it to deal with the new style of mathematical practice that had emerged in nineteenth-century Germany.

The task of a *Begriffsschrift*, a concept-script, is to form expressions for complex concepts out of signs for primitive concepts, and to do so in a way that facilitates rigorous reasoning. In order to achieve this the formula language of arithmetic must be supplemented with signs for the logical element that will serve as the "logical cement" binding together the primitive signs available already in arithmetic. It is just this that Frege's own concept-script attempts: "to supplement the formula language of arithmetic with symbols for the logical relations in order to produce—at first just for arithmetic—a conceptual notation of the kind I have presented as desirable" (Frege, 1882b, 89), a notation that has "simple modes of expression for the logical relations" that are "suitable for combining most intimately with a content" (Frege 1882b, 88).[6] "I wish to blend together the few symbols which I introduce and the symbols already available in mathematics to form a single formula language" (Frege 1883, 93). As one builds numbers out of numbers in the formula language of arithmetic so Frege will build concepts out of concepts in his formula language of pure thought. In *Begriffsschrift* "we use old concepts to construct new ones . . . by means of the signs for generality, negation, and the conditional" (Frege 1880, 34).

We saw in Chapter 2 that in Euclidean diagrammatic reasoning it is not the definitions expressed in natural language but instead diagrammatic constructions

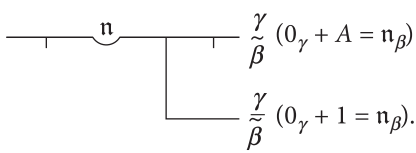
[6] The invocation here of something like logical form as contrasted with content is not significant. Frege's language aims to express content, including purely logical content. See my (2005) section 3.4 for some historical context for Frege's remark here.

of figures that formulate content in a mathematically tractable way. Assuming only a few primitive constructions as set out in Euclid's postulates—of a line between two given points, of a circle given a point and line length, and of an extended line length given some (shorter) line length—Euclid shows how to construct on that basis other more complex sorts of figures, not only, say, equilateral triangles and squares but also, for instance, cuts of lines in extreme and mean ratio and solid angles out of plane angles. A drawn figure in Euclid serves in this way not as a picture or instance of some geometrical entity but instead to formulate what it is to be this or that geometrical entity, and it does so, we saw, in a way enabling reasoning in the system of signs as scripted by the apodeixis. The system of signs is, in its own unique way, at once a *characteristica* setting out the contents of concepts in drawn figures and a *calculus ratiocinator* within which to reason from those concepts.

The language of early modern algebra is also, in its way, a Leibnizian *characteristica* and a *calculus ratiocinator*, though (as one might expect) it is one that is cognitively much more sophisticated than that we find in Euclid. It takes time and practice in the language to learn to see the contents of concepts displayed in collections of signs in this language, even the contents of such simple concepts as that of a sum of a number and its reciprocal: $x + 1/x$, or that of a product of two sums of (two) integer squares: $(a^2 + b^2)(c^2 + d^2)$. In Euclid there are both (basic and derived) rules of construction and (basic and derived) rules of inference, both of which are involved in any diagrammatic demonstration. In elementary algebra there are, on the one hand, the axioms that provide the basic rewrite rules of the system, and on the other, the theorems one can prove and the problems one can solve given those rules. Whereas in Euclidean diagrammatic reasoning one perceptually reconfigures parts of wholes, in early modern algebra one reconfigures by rewriting, putting equals for equals.

We have seen that one can show algebraically that if two numbers are each of them a sum of integer squares then their product is also a sum of integer squares, and that one begins the demonstration by formulating the starting point, two numbers that are each of them a sum of integer squares, in the language thus: $a^2 + b^2$ and $c^2 + d^2$. What we must show is that the product of two such sums is also a sum of integer squares. So we write down the product of our two sums: $(a^2 + b^2)(c^2 + d^2)$. As we have also seen, one can similarly formulate the contents of important mathematical functions in a tractable way in this language. We saw this in our demonstration of Euler's theorem when it was argued, for instance, that $e^x = 1 + x + x^2/2! + x^3/3! + \ldots + x^n/n! + \ldots$ Because the complex sign on the right of the identity sign is formed out of primitive signs in the language, the rules of the language can be applied to that complex sign in a way that is simply impossible in the case of the simple sign on the left of the identity sign. As Frege would put it, although the simple sign on the left and complex sign on the right in our identity have the same designation or *Bedeutung*, namely, some mathematical function (conceived following Riemann as a mapping, or law of correlation), they express different senses. And we know that they express different senses because we had to *argue* (based on the way the sign on the right is built up out of parts) that the identity holds. But in fact we can see already in the notation that the expressions on the two sides of the equal sign express different senses. The mere fact that the sign on the right has a great deal of internal, arithmetical articulation on the basis of which the rules of algebra can be applied, while the sign on the left lacks any such articulation, shows that the two signs express different senses.

In our examples from algebra, the content that is formulated is articulated arithmetically, in sums, products, powers, and so on. The content of concern to Frege is instead articulated inferentially; it is content as it matters to inference, not as it matters to algebraic computations, that needs to be expressed in the cases of concern to Frege. It may seem surprising that concepts can have contents that admit of such different sorts of articulation—though having in mind the rich, long history of mathematics will make this seem less surprising. Consider, for instance, the concept *rational number*. The ancient Greeks had no such concept. Because a number is, for them, a collection of units, what we think of as a rational number is instead conceived as an entirely different sort of entity, namely, as a ratio of two numbers, as two numbers in a certain relation. The early modern notion of a rational number is a notion of a sort of number that can be thought of either relative to whole numbers as a fraction, that is, as something of the form m/n, where m and n are whole numbers, $n \neq 0$, or relative to irrational numbers as decimals that are either finite or repeating. Such expressions formulate rational numbers in a way that is arithmetically articulated, suitable for the calculations that are characteristic of early modern mathematical practice. Frege thinks of that same content as logically (that is, inferentially) articulated. In his presentation of the content of this concept something, A, is a rational number just in case there is a positive whole number that is a multiple of it, in *Begriffsschrift*:



What we need to understand is how exactly this works.

Frege's *Begriffsschrift* was to be a Leibnizian *characteristica* in which signs for complex concepts are constructed out of signs for simple concepts, and constructed in such a way that reasoning on the basis of such concepts could be conducted according to determinate rules. Because the form of mathematical reasoning he is concerned with is deduction, that is, the relation of ground and consequent, Frege chooses the conditional as his fundamental logical connective (Frege 1880, 37). Frege's three primitive signs for expressing logical content as it figures in mathematical concepts, such as that of a rational number above, are the negation and conditional strokes, and the concavity for the expression of higher-level concepts and relations involving generality.

Isolating the primitive logical notions that are needed to serve as the "logical cement" binding old concepts into new, and devising written signs for them, is straightforward. Much less so is to figure out how it is possible to exhibit the contents of concepts at all. Frege needs not merely to *say* what a particular content amounts to but to *exhibit* the inferentially articulated contents of concepts themselves. We know already that a drawn circle in a Euclidean diagram iconically displays the content of the concept *circle*, that all points on the circumference are equidistant from a center, and hence that one can infer given two (or more) radii of that circle that they are equal in length. Similarly, a complex of signs in the formula language of arithmetic displays, for instance, the content of the concept *sum of two integer squares*, shows what it is to be that in a way enabling calculations, namely, $a^2 + b^2$. Frege needs, analogously, a way of writing that will display the contents of concepts in a way that supports inferences. The task is not merely to *record* necessary and sufficient conditions for the application of a concept, what is the case if the concept applies; it is to *show*, to set out in written marks, the contents of concepts as those contents matter to inference. And to do that Frege needs to invent a new sort of written language, not merely a new system of written marks but a fundamentally new *kind* of system of written marks.

We saw in Chapter 2 that a Euclidean diagram can be variously regarded, that the various points and lines it comprises can be seen now this way and now that, and that this feature of the diagram is critical to an adequate understanding of the fruitfulness of a Euclidean demonstration. A line that is first seen as, say, a radius of a circle is later in the demonstration to be seen as a side of a triangle. In itself the drawn line is neither the one nor the other but in the context of the appropriate diagram it has the potential to be taken either way; one arrives at a particular geometrical figure by analyzing the diagram. Independent of such an analysis the drawn points and lines that make up the diagram are not icons of geometrical entities; drawn points are not, for instance, centers of circles or vertices of triangles, and drawn lines are not radii of circles or sides of a triangle. A particular point is a center of a circle or a vertex of a triangle, and a particular line, a radius or side of a triangle, only relative to a way of regarding the collection of lines and points in the drawn diagram, only relative to an analysis. Frege's concept-script, we will see, functions in an essentially similar way. The primitive signs, or collections of them, serve to designate concepts only given a context of use and relative to an analysis into function and argument.

Frege's conception of his language, although (as just indicated) it is fundamentally similar in certain respects to the conception of a diagram that is needed in Euclidean geometry, is essentially late, possible at all only as a very late fruit of our intellectual development and maturation. It furthermore involves a transformation that is as profound and consequential as that Descartes enacted in founding early modern mathematics. And much as Descartes came to his understanding of the symbolic language of arithmetic and algebra through a kind of metamorphosis in the way he "reads" a drawn Euclidean figure, so we can achieve a Fregean understanding of the language of Frege's logic through a kind of metamorphosis in the way we read Descartes' language. It will help to distinguish two stages in the process: first, the transformation itself through which one learns to read the language in a new way, and then the full realization of the power of this new sort of language.

In the case of Descartes, the first step is to see a drawing, say, of a square, not as exhibiting (the content of the concept of) a kind of *object* but instead as exhibiting a certain arithmetical *relation* among arbitrary quantities, one that can equally well be expressed symbolically: x^2. The first step for us is to learn to read the formula language of arithmetic in a new way, not as exhibiting arithmetical relations but as expressing a Fregean thought and hence as variously analyzable into function and argument.

We begin with a mathematical language, that is, a system of written marks within which to do mathematics, specifically, the formula language of arithmetic and algebra. In this system the various signs—the numerals, the signs for arithmetical operations, and so on—all have their usual meanings, and in virtue of those meanings, equations in the language serve to display various arithmetical relations that obtain among numbers (or magnitudes more generally). Much as the equation '$a^2 + b^2 = c^2$' exhibits a certain relation among arbitrary quantities (namely, that exhibited also in a right triangle), so we can read the equation '$2^4 = 16$', for instance, as displaying an arithmetical relation that obtains among the numbers two, four, and sixteen.[7] In this equation so conceived, the Arabic numeral '2' stands for, or designates, the number two, the numeral '4' stands for (designates) four, the numeral '16' stands for (designates) sixteen, and the manner of their combination shows the arithmetical relation they stand in. Now we learn to read the language differently, as a fundamentally different kind of language from that it was developed to be. Instead of taking the primitive signs of the language to designate prior to and independent of any context of use, as we needed to do to devise the language in the first place, now we take those same signs *only* to express a sense prior to and independent of any context of use.[8] Only in the context of a whole judgment and relative to some one function/argument analysis will we arrive at sub-sentential expressions, whether simple or complex, that designate something.

Suppose, for instance, that we take the numeral '2' to mark the argument place. In that case the remaining expression, '$\xi^4 = 16$', is to be read as expressing a certain arithmetically articulated sense and through that sense as designating the concept *fourth root of sixteen*. A concept, in this context, is furthermore to be understood on

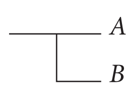
[7] The example is Frege's in (1880, 16–17).

[8] Already in algebra, we find something approaching this idea insofar as a whole collection of signs can be taken to formulate the content of a single concept or function.

the model of a function as it is understood by nineteenth-century mathematicians such as Riemann; a (Fregean) concept is a mapping, or as Frege sometimes puts it, a law of correlation, objects to truth-values in the case of first-level concepts and lower-level concepts to truth-values in the case of higher-level concepts. If now we instead regard the numeral '4' in '$2^4 = 16$' as marking the argument place, the remainder designates the concept *logarithm of sixteen to the base two*. And other analyses are possible as well. The language is, then, symbolic much as the language of elementary algebra is, but its primitive signs nonetheless function much as the written marks for points, lines, angles, and areas function in Euclid. In the language as Frege conceives it, *the primitive signs only express a sense independent of a context of use*.[9] Only in the context of a proposition and relative to an analysis into function and argument do the sub-sentential expressions of the language, whether simple or complex, serve to designate anything.[10] This is the crucial first step in coming to see how the contents of concepts might be displayed in *Begriffsschrift*.

The first step for Descartes was to learn to see drawn geometrical figures in a new way, as exhibiting arithmetical relations. The second step was fully to realize the potential of this new way of seeing in equations in two or more unknowns, for instance, those that trace the paths of moving points through Cartesian space. In order fully to realize the potential of Frege's new way of reading we need similarly to go on to consider *logically* articulated thoughts. We need, that is, to "[devise] signs for logical relations that are suitable for incorporation into the formula language of mathematics, and in this way forming—at least for a certain domain—a complete concept-script" (1880, 14).

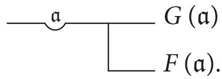
As already noted Frege has three primitive signs to serve as the "logical cement" binding concept words together in the formation of new concept words, namely, the conditional and negation strokes and the concavity (with its German letters). The content stroke is also needed as that to which these signs attach. (Later we will see that the content stroke has a further, logical role to play as well.) The conditional stroke is iconic in Peirce's sense; it exhibits (the truth of) one thought as depending, or resting, on (the truth of) another:



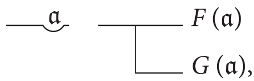
[9] Of course, Frege could put the point this way, that is, in terms of the distinction of sense (*Sinn*) and meaning (*Bedeutung*), only after 1890. Nevertheless, in his practice, the distinction is there from the beginning; although made explicit only in 1892, it is implicit already in the 1879 logic that formulae of Frege's language only express Fregean thoughts (and designate truth-values) independent of an analysis into function and argument, that sub-sentential expressions, whether simple or complex, only designate relative to a function/argument analysis of the whole.

[10] This feature of Frege's notation is explored at length in my (2005).

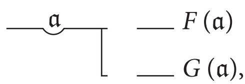
exhibits the thought that A is true on condition that B is true, that is, as we will often write it, A-on-condition-that-B. The whole formula is true if and only if it is not the case that B is true and A is false. Consider now a simple case involving both the conditional stroke and the concavity:



Here we have a complex expression comprising two (first-level) concept words 'Fξ' and 'Gξ', the concavity, and the conditional stroke, each of which only expresses a sense independent of the context of a proposition and relative to an analysis. Two analyses are most immediately relevant, namely, this:



according to which the formula ascribes the second-level concept *universally applicable* (designated by the concavity) to the first-level conditional, monadic concept *F-on-condition-that-G* (designated by the complex expression on the right), and this:



according to which the formula ascribes the second-level relation *subordination* (designated by the concavity *together with* the conditional stroke) to the concepts *F* and *G*. On the first reading what this formula of *Begriffsschrift* says is that the concept *F-on-condition-that-G* has the higher-level property of giving the value True no matter what the argument, that that conditional concept would be correctly applied no matter what object were to be considered. So read, this *Begriffsschrift* formula does *not* say that all objects have the property F-if-G, as '$(\forall x)(Gx \supset Fx)$' does; rather it ascribes a second-level property—the property of yielding the value True no matter what object is given as argument (that is, of being universally applicable)—to a first-level concept, from which, of course, the quantified claim about objects follows. On the second reading the formula says instead that the concept *G* is subordinate to the concept *F*, that those two first-level concepts stand in a certain second-level relation. And neither reading is in any way privileged. The one is just as good as the other as a reading of the sentence much as in the context of an appropriate diagram seeing a line as a side of a triangle is just as good as seeing it as a radius of a circle. Thus, already in this simple example we see how Frege's radically new conception of language enables the formation of concept words. Given the appropriate analysis, we obtain a concept word (formed from the concavity and conditional stroke) for the second-level relation of subordination, which takes two first-level concepts as arguments to yield a truth-value as value.

The fundamental purpose of Frege's concept-script is the same as that of earlier mathematical languages, to formulate content in a written system of signs in a way that enables reasoning in the system of signs. More specifically, it is to formulate such content for the mathematical practice of reasoning deductively from defined concepts. Again, the contents Frege aims to display are inferentially rather than arithmetically or spatially articulated. So he introduces primitive signs for primitive logical properties and relations. But once he has done that he must radically reconceive how those signs function in the context of the language as a whole. Because the task is not merely to record what is the case if a concept applies but to display its content, Frege needs to begin not with concepts but with the contents of whole judgments, that is, Fregean thoughts that are a function of the senses expressed by the primitive signs, which can then be analyzed into function and argument in various different ways to give different concept words. A function sign, or concept word, so derived can be highly inferentially articulated, that is, express a very logically complex sense. It nonetheless designates something simple, namely, a concept conceived as a mapping or law of correlation.

7.2 The Basics

Anyone who has taught logic, or indeed any rule-governed but non-algorithmic system, knows that it is one thing to know the rules, how to apply them, and another to be able to use them intelligently. In such cases, although one must begin by learning the rules—what they do and do not allow one to do—one has facility in the system only when one has acquired a sense of strategy, of what is likely to work or not work given what it is one is trying to do. As strategies remind us, reasoning is not merely a matter of applying rules. It is the capacity to apply them well in a particular context, and this is a skill one learns by practicing, and can improve with further practice. But even strategies do not exhaust the mastery of the mathematician. In mathematics, interesting and significant proofs invariably have a twist, some move or moves that are surprising, in some way unique, and often altogether wonderful. Frege's proof of theorem 133 involves such moves, and until we can recognize them as such, we cannot be said fully to understand the proof, or by extension how the language works as a language to enable proof.[11] In this

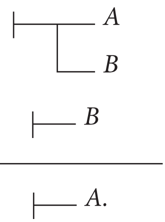
[11] Chemla (2009) emphasizes the crucial importance of acquiring literacy in a mathematical practice that is the object of one's philosophical investigation, and also the difficulties there can be in determining whether in fact one has achieved the required literacy. Both points are especially important in the present context. Readers of Frege think that they understand Frege's notation, that they are literate in it provided they can translate it into standard notation, but until and unless one can see the *ideas* at work in a proof such as that of theorem 133 of *Begriffsschrift*, in fact one is not. It takes hard work, and a great deal of

section we focus primarily on the rules. In the next we turn to strategy, that is, to a general-purpose strategy that goes a long way in accounting for how reasoning from definitions of concepts goes in *Begriffsschrift*. In the penultimate section we turn finally to distinctive features of Frege's proof that are not explained by that general strategy, features that alone fully exemplify the power of reasoning from concepts in *Begriffsschrift*.

In Part II of the 1879 logic Frege introduces nine axioms and derives on that basis the various theorems that will be needed in the proof of theorem 133 in Part III. Much as in early modern algebra, for example, in the demonstration of Euler's Theorem, one rewrites formulae according to the rules given in the axioms and derived theorems of that algebra, so in Frege's system one rewrites according to rules given in axioms and derived theorems. And in both cases there is a kind of über-rule that cannot be stated in the system but must instead be presupposed in any application of a rule that can be stated in the system. In algebra the rule is that equals can be put for equals wherever they occur. In Frege's system the rule is that if you have a conditional judgment and know that the condition is met then you may infer the conditioned judgment unconditionally, that is, detachment.

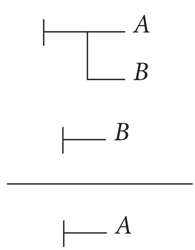
We know that any inference, whether purely logical or not, is an instance of something more general, of a rule that can be applied in other cases. That is, any inference draws a conclusion from some premise or premises according to what Peirce calls a leading principle, where a leading principle is essentially general: "*whenever we draw a conclusion*, we have an idea, more or less definite, that the inference we are drawing is only an example of a whole class of possible inferences, in each of which from a premise more or less similar to the actual premise there would be a sound inference of a conclusion analogous to the actual conclusion" (Peirce 1992, 131). And as Lewis Carroll (1895) famously argues, such a rule cannot be expressed as a premise of the inference; more exactly, if it is so expressed, then another rule is needed to govern the passage from the newly augmented set of premises. In Ryle's words, "conclusions are drawn from premises in accordance with principles, not from premises that embody those principles" (Ryle 1950, 328). The rule governing a particular inferential step, that is, the passage from a given judgment or judgments to a conclusion in actual practice, is not expressed in the system, and it cannot be so expressed insofar as it is functioning as a rule. Because the rule governing an inference cannot be expressed in the system (without presupposing another, unexpressed rule), Frege restricts himself in the 1879 logic to only one such rule, namely, this:

practice, to learn to see all that is expressed in Frege's formula language, especially in cases involving reasoning from concepts, as in Part III of the 1879 logic.



If one has a judgment of the form *A*-on-condition-that-*B* and also the judgment that *B* then it may be inferred that *A*. And these must be judgments, that is, acknowledged truths, in Frege's system; one can infer, properly speaking, only from what is true and is acknowledged to be so. What is inferred, the conclusion, is similarly an acknowledged truth. In Frege's system as presented in the 1879 logic, all other rules of inference are expressed as judgments, either as axioms or as theorems derived from those axioms according to Frege's one rule of inference.

With one exception, every inferential step in Parts II and III of the 1879 logic involve two premises, namely, a conditional of the form *A*-on-condition-that-*B* and the judgment that *B*, and the conclusion that *A*. (The exception is a multi-premise inference in Part III that will be considered in due course.) We will refer to the judgment that *B* as the *ground* of the inference and the conditional premise, the one that has the form *A*-on-condition-that-*B*, as the *bridge*. As we can think of it, the conditional premise provides, much as a bridge does for the case of (say) crossing a river, the resources to take one from one judgment, the ground, to another, the conclusion. At least in some cases, we will see, the bridge functions as a kind of a rule licensing the inferential passage from the ground to the conclusion.



Bridge

Ground

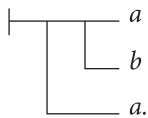
Conclusion

But although all inferential steps in *Begriffsschrift* have this same basic form, conceptually, two very different cases can be distinguished.[12] In the predominant sort of case, the conditional really does function as a rule; it is something essentially

[12] And here we come to what will be for some a stumbling block. If one is more mathematical than metaphysical in temperament then the fact that the form is shared is liable to dominate, and then it will be hard to discern the conceptual differences. These conceptual differences are nonetheless critical if we are to come to an adequate philosophical understanding.

general that applies also to other instances. In other cases the inferential step is merely that of detachment: one has a conditional and also the judgment that the condition of the conditional is satisfied, and so can conclude the consequent unconditionally.[13] Frege's derivation of theorem 5 on the basis of axioms 1 and 2 in Part II of the 1879 logic, illustrates the difference between these two cases.

Frege's first axiom is this:



If we think of this formula as a ground, that is, as expressing a judgment or acknowledged truth on the basis of which to draw an inference, there are two different ways we can regard it. First, we can read it as saying that the content *a* (to which the main, topmost content stroke takes us) is true on the two conditions that are listed beneath that content and connected to it by conditional strokes, namely, the condition that *b* is true and the condition that *a* is true. What we have, that is to say, is the content that *a*-on-condition-that-[*b*-and-*a*]. But we can also read this formula differently, as saying instead that the content *a*-on-condition-that-*b* (that is, treating the condition *b* now as part of the conditioned judgment) is true on the single condition that *a*, that is, as the content that [*a*-on-condition-that-*b*]-on-condition-that-*a*. On the first reading, the formula is trivially true: if both *a* and *b* are true, then *a* is true. On the second, it presents instead an important truth about truth, that what is true is true unconditionally, that is, on any condition you like.[14]

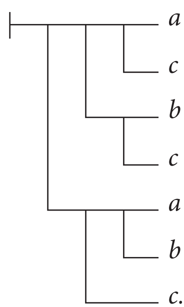
But we can also take such a judgment to function not as a truth but as a rule of passage or inference license, that is, as a bridge that can take us from one truth to another. Conceived as such a rule, axiom 1 licenses a one-premise inference from the judgment that *a* (the left-most, bottom condition) to the judgment that *a*-on-condition-that-*b*, for any *b* you like. (Again, it is important to keep in mind

[13] The essential difference here is illustrated in the difference between the following two inferences: 'Being a cat entails being a mammal; Felix is a cat; therefore Felix is a mammal' and 'If Felix is a cat then Felix is a mammal; Felix is a cat; therefore Felix is a mammal'. In the first inference one has a rule, that being a cat entails being a mammal, that is here applied in particular to Felix but can be applied in other cases as well. In the second inference there is no general rule among the premises but only a truth-functional compound.

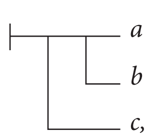
[14] The truth expressed in this formula is often held to be paradoxical insofar as it is taken to mean that a true sentence is implied by anything, however irrelevant, even by something false. On our reading of it, which includes also the idea that one can infer only on the basis of something that is acknowledged to be true, there is no paradox. Just as one can add and subtract something (that is, one and the same thing) to an equation, and may need to do this in the course of solving a problem, though taken by itself such a move seems utterly pointless, so one can add a condition, any condition you like, to an already acknowledged judgment, and may need to do this in the course of a proof, even though taken by itself such a move seems utterly pointless. We will soon see examples of this.

that it is possible to infer only from an acknowledged truth, and that what one infers is likewise an acknowledged truth.) Read as a rule licensing inferences, axiom 1 functions in this way as a rewrite rule much as an axiom or derived theorem of algebra does. Or as we can also think of it, it functions as a postulate does in Euclid. Much as a Euclidean postulate licenses, say, the construction of a line between two given points, or the construction of a circle given a point and a length as radius, so Frege's first axiom licenses the construction of the (acknowledged) formula *a*-on-condition-that-*b* given that one has the (acknowledged) formula that *a*.

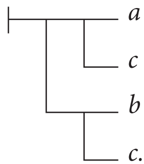
Frege's second axiom is this:



To convince ourselves that this formula is true, that it expresses a thought that ought to be acknowledged as true, we can read it thus: if it is true that *a*-on-condition-that-*b*-and-*c* (the left-most, lowest condition), and also true that *b*-on-condition-that-*c* (the middle condition), then it is true that *a*-on-condition-that-*c*. That is, we read it as about the judgment that *a*-on-condition-that-*c*, that it is true on two conditions. This is less obvious than is the truth of the first axiom, but nonetheless fairly obviously true. Suppose that we have convinced ourselves that it is true. If we want now to read this *Begriffsschrift* sentence as an inference license or bridge, we read it instead as the principle that if you have some conditional, here *a*-on-condition-that-*b*, that is itself on a condition, here *c* (that is, the whole of the left-most, lowest condition read not now as a conditional with two conditions but instead as a conditional on a condition), then you can infer that conditional, *a*-on-condition-that-*b*, with the condition *c* instead attached as a condition both on the condition *b* of the original conditional and on the conditioned judgment *a*. (In general, to read a formula of *Begriffsschrift* as a one-premise inference license, one takes the left-most, bottom condition to supply the form of the premise and the remainder as giving the form of the conclusion.) Read as bridge, axiom 2 licenses an inference from a judgment (ground) that looks like this:

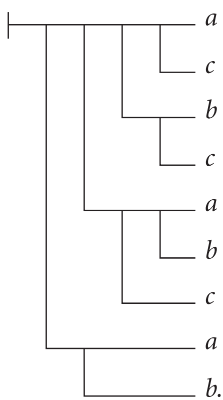


to a judgment (conclusion) that looks like this:



Conceived as a rule of inference axiom 2 allows one to move (that is, rewrite) in this way the condition *c* so that it becomes a condition on both the conditioned content *a* and the condition *b*.

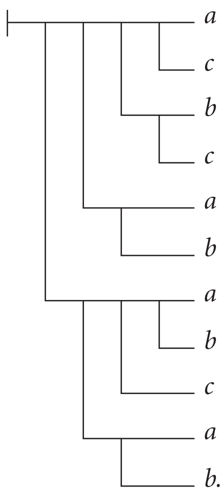
Much as familiar rules of algebra such as that $a(b + c) = ab + ac$ license rewritings in elementary algebra, so Frege's first two axioms license rewriting a formula in a new way given that one has a formula of the appropriate shape. For example, taking axiom 2 as the ground or premise of an inference, with axiom 1 serving as the bridge or rule of passage, we can, according to that rule, rewrite axiom 2 with an added condition, say, the condition that *a*-on-condition-that-*b* :



This is theorem 3, the first derived theorem of the 1879 logic.

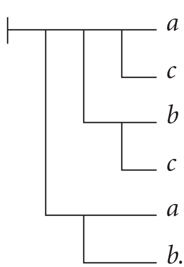
Theorem 3 is derived from axiom 2 according to the rule, formulated in axiom 1, that if you have a judgment then you may infer that judgment on any condition you like. That is, although officially the inference is a case of Frege's one rule of inference insofar as Frege substitutes the content of axiom 2 for '*a*' in axiom 1 and '*a*-on-condition-that-*b*' for '*b*', it is clear that we can also think of this step as an inference from axiom 2 to theorem 3 according to the general rule expressed in axiom 1 that given that one has an acknowledged truth one may infer that truth on any condition one likes.

Now we use axiom 2 as the bridge, or inference license, with theorem 3 as ground, to infer theorem 4:



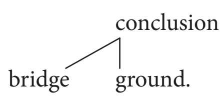
That is, we move what is in theorem 3 the lowest condition, *a*-on-condition-that-*b*, onto both arms of the conditional of which it is the condition in theorem 3. Notice that in this case we read the two right-most conditions on the content *a* (that is, the top-most '*a*' to which the main content stroke takes one), in theorem 3, as part of the conditioned judgment in the conditional of which *a*-on-condition-that *b* is the condition. To apply the rewrite rule in axiom 2 to theorem 3 as premise, *a*-on-condition-that-*b* takes the place of *c* in axiom 2, *a*-on-condition-that-[*b*-and-*c*] takes the place of *b*, and [*a*-on-condition-that-*c*]-on-condition-that-[*b*-on-condition-that-*c*] takes the place of *a*. And this is generally true in reasoning in Frege's *Begriffsschrift*: to see that and how a given rule applies to some *Begriffsschrift* formula one must regard that formula in some particular way, regard certain conditions as conditions *on* a conditioned judgment and others as *parts of* the conditioned judgment; and different rules will, often, require different ways of regarding the formula. Much as one does in Euclid's system of signs, in *Begriffsschrift* one must perceptually configure things now this way and now that, depending on what rule one aims to apply.

Theorem 4 serves as the bridge from axiom 1—with *a*-on-condition-that-*b* for *a*, and *c* for *b*—to theorem 5:



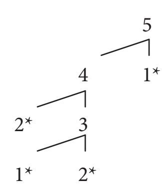
Notice that here theorem 4 is *not* functioning as a rule; it is not something more general than that to which it is applied, namely, axiom 1, but something less general. We need a special case of axiom 1 here, and so must think of the move from theorem 4 to theorem 5 not as one in which the bridge is functioning as a rule of inference but merely as a case of detachment. We know that the condition on theorem 4 is satisfied because it is nothing more than a special case of axiom 1; so we can detach that condition to give theorem 5.

We have just seen that theorem 5 can be derived from axioms 1 and 2: beginning with axiom 2 as ground and axiom 1 as bridge, we derived theorem 3, which then served as the ground, with axiom 2 as bridge, to yield theorem 4. Theorem 4 was the bridge from axiom 1 as ground to theorem 5. In this little derivation, then, both axiom 1 and axiom 2 serve once as ground and once as bridge. Because it will help to have a way of visualizing the whole course of reasoning, we introduce the following convention. We write the number of the formula that serves as the ground in an inferential step directly beneath the number of the conclusion of that step and connect the two numerals using a small vertical line. The number of the formula that serves as the bridge for that step in the reasoning is written off to the side, on a level with the number for the ground, and connected to the conclusion using a diagonal line:



Using this convention, and marking the axioms with asterisks, the course of the derivation of theorem 5 is as shown in Figure 7.1.

Figure 7.1 The course of the derivation of theorem 5.

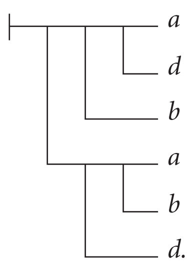


That is, reading upwards, the chain of reasoning begins with axiom 2 as ground or premise. The first step is to infer theorem 3 from axiom 2 according to the rule expressed in axiom 1. One then uses the rule expressed in axiom 2 to infer theorem 4 from theorem 3. Theorem 4 serves in turn as the bridge from axiom 1 to theorem 5 insofar as it plays the role of the conditional premise. But again, this inferential step, although formally identical to the first two, is conceptually different. Theorem 4 is not functioning as a rule (though there could conceivably be cases in which it did so function) but instead as a simple conditional the condition of which is satisfied, as axiom 1 shows. Whereas the first two inferential steps have the form 'being B entails being A, this is B; hence it is A', the third is a simple detachment: A is true on condition that B is true, but B is true; hence A is true.

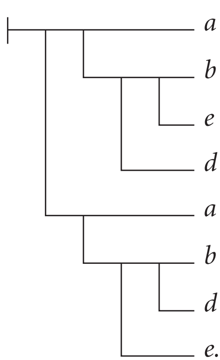
Our little derivation of theorem 5 is fully rigorous, every step derived from a ground and a bridge according to Frege's one principle of inference. But if that were all one saw in the derivation one would be missing something. We have already noted an important difference between the first two inferential steps and the third. And there is something else to be seen as well. Think again of the demonstration that the product of two sums of integer squares is also a sum of integer squares. On the way to that result we first derived $a^2c^2 + b^2c^2 + a^2d^2 + b^2d^2$ according to the rules of algebra. Then we reordered the terms and both added and subtracted $2abcd$ (with the four letters ordered in two different ways) on the way to the desired result. As far as mere rigor is concerned each step and each result of a step is as good as any other. But to see only that each step is allowed by the rules is to fail to grasp the *point* of the demonstration. The point is to reveal a connection between two concepts, that of a product of sums of integer squares and that of a sum of integer squares; the intermediate steps are merely a means to that end and are of no significance in themselves. Just the same is true in the case of the derivation of theorem 5. Although the derivation of theorem 3 is a legitimate move in the system, there is no point to deriving it except in light of what it allows one to go on to do, namely, to derive theorem 4 and on that basis to derive theorem 5. It is theorem 5 that is of interest, and that theorem is of interest at least in part because it expresses an important truth about the relation encoded in the conditional stroke. Theorem 5, appropriately regarded, exhibits the transitivity of the relation designated by the conditional stroke: if a-on-condition-that-b and b-on-condition-that-c, then a-on-condition-that-c. Neither theorem 3 nor theorem 4 has any such intrinsic interest.

We know, because Frege tells us in the subtitle of the 1879 logic, that Frege's concept-script *Begriffsschrift* was modeled on the formula language of arithmetic. In the Preface of that work Frege further tells us that "the most immediate point of contact between my formula language and that of arithmetic is in the way the letters are used" (1879, 104). In the formula language of arithmetic, letters are used in the expression of a kind of generality; they enable the formulation of laws such as that $a(b + c) = ab + ac$, where such a law relates not any particular numbers but instead kinds or forms of numbers. That is, letters in algebra enable the expression of concepts, for instance, the concept of a product of a number and a sum, '$a(b + c)$', or the concept of a sum of integer squares, '$(a^2 + b^2)$', and on that basis enable one to prove that various sorts (forms) of numbers are constitutively related. Similarly, in Frege's axioms in *Begriffsschrift* the use of letters enables the expression of laws about kinds or forms of sentences, that is, concepts such as that of a conditional sentence, and the derivation of relations of entailment between various sorts (forms or concepts) of sentences. Formulae that express such relations of entailment can, furthermore, often be read also as ascriptions of higher-level properties to lower-level ones. Much as '$a + b = b + a$' can be read as ascribing the higher-level property of commutativity to the operation of addition, so, for example, Frege's theorem 5 can be read as an ascription of a higher-level property to the lower-level relation designated by the conditional stroke, namely, the property of transitivity. As one gains facility with the language one begins to see in this way not only that given steps are valid but also that lower and higher level concepts are expressed in at least some of the formulae of the language, and what they are.

The axioms and derived theorems of Part II express inference licenses in the form of generalized conditionals: if one has a judgment of a certain form then one can derive another judgment of a certain form. One can, for instance, reorder the conditions in a conditional involving two or more conditions. Axiom 8 sets out the basic case in which one has a judgment on two conditions that are then switched:

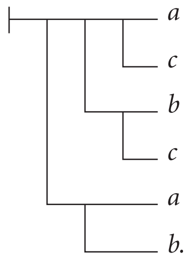


Read as a rule licensing inferences, this axiom says that if one has the judgment that *a*-on-condition-that-[*b*-and-*d*] then one may infer, rewrite it as, *a*-on-condition-that-[*d*-and-*b*]. On the basis of axiom 8 Frege also derives various other cases governing the reordering of conditions, cases that involve three, four, and five conditions (in theorems 12 to 17, and 22). And he shows that one can reorder conditions even in the case in which those conditions are conditions on something that is itself a condition:

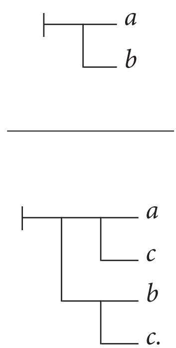


This rule licenses a switch in the order of conditions (here, *d* and *e*) in the case in which they are conditions not on a judgment but instead on the condition (here, *b*) of a judgment (here, *a*).

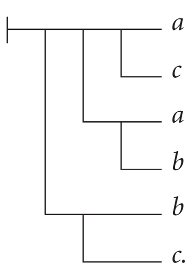
To see how Frege derives this theorem, consider again theorem 5:



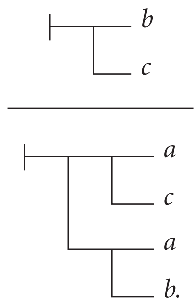
We have already seen that we can read this formula as expressing the thought that the relation designated by the conditional stroke is transitive. But we can also read it differently, in particular, as an inference license. Read as a rule licensing a one-premise inference, this formula says that if you have a conditional *a*-on-condition-that-*b*, that is, the left-most, bottom condition, then you can attach a condition, any condition you like (here, *c*) to both arms of the original conditional. That is, it licenses inferences of this form:



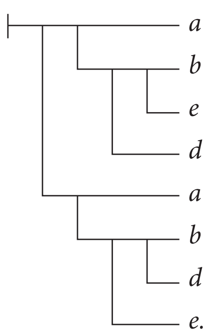
Another way to regard theorem 5 is as the judgment that *a*-on-condition-that-*c* on two conditions, and hence as a judgment the two conditions of which can be reordered, licensed by the rule in axiom 8, thus:



This is theorem 9. What it allows, read as a rule governing a one-premise inference, is an inference from a conditional, here, *b*-on-condition-that-*c*, to a new conditional both arms of which are also conditionals: [*a*-on-condition-that-*c*]-on-condition-that-[*a*-on-condition-that-*b*]. Relative to the result of applying theorem 5 as a one-premise inference rule, here everything is switched. The new content, *a*, is not added as a condition but is instead the conditioned judgment, and the order of *b* and *c* is switched: *c*, which had been the condition, is now the condition on the conditioned judgment, and *b*, which had been the conditioned judgment, is now the condition on the condition of that judgment. That is, theorem 9 governs inferences of this form:



Applying this rule to axiom 8 governing the reordering of two conditions as ground directly yields theorem 10 governing the reordering of conditions that occur in something that is itself a condition on a judgment:

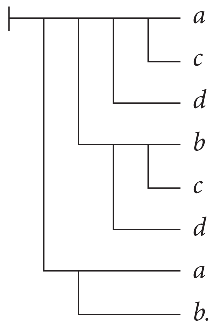


Because axiom 8 is—that is, has the form of—a conditional (both arms of which are also conditionals), theorem 9 licenses this move from axiom 8 to theorem 10.

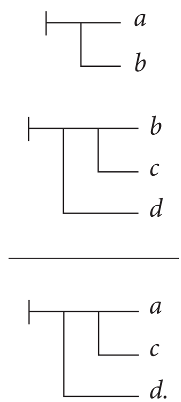
So far we have considered only one-premise inferences, and officially every inferential step in Frege's system is a one-premise inference (with the other, conditional premise serving as the rule).[15] But at least some of the theorems Frege derives

[15] We have already seen that strictly this is not correct; detachment is a two-premise inference governed by Frege's one mode of inference. In most cases, however, and in particular in the cases that are most important for understanding reasoning in *Begriffsschrift*, the conditional premise, the bridge, functions as a

in Part II of *Begriffsschrift* are more naturally read instead as rules governing two-premise inferences, for instance, theorem 5. Read as a rule governing a two-premise inference, theorem 5 is simply the rule we call hypothetical syllogism: given that *a*-on-condition-that-*b* and that *b*-on-condition-that-*c*, it may be inferred that *a*-on-condition-that-*c*. We have already seen that theorem 9 is just like theorem 5 except for the order of its two conditions; so the same is true of 9 read as a rule governing a two-premise inference. Theorem 7 is just like theorem 5 except that in place of the condition *c* there are two conditions *c* and *d*:



Theorem 7 is thus a variant of the rule of hypothetical syllogism for the slightly more complicated case in which one of the conditionals has two conditions:



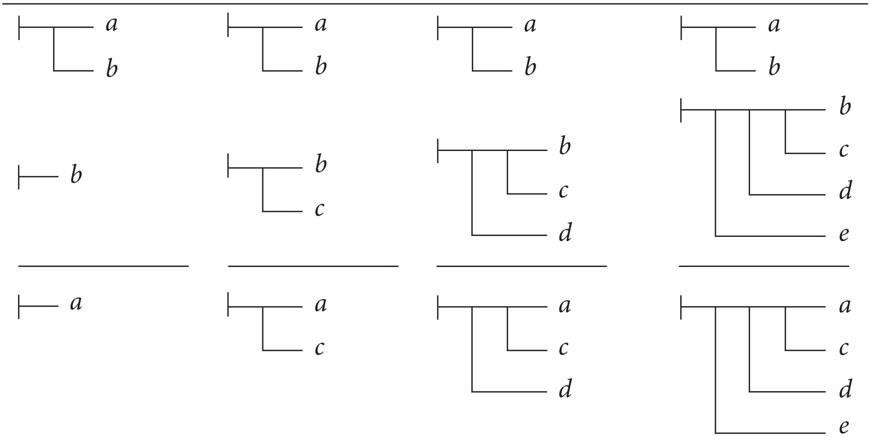
Theorem 19 stands to theorem 7 as theorem 9 does to theorem 5; the only difference between theorems 7 and 19 is the order of the two conditions. Theorem 20 governs the case in which one has three conditions (rather than two as in theorems 7 and 19,

rule licensing the inference from the ground to the conclusion. It is something essentially general that can or only one as in the original cases of 5 and 9) that must be carried over into the conclusion.

All of theorems 5, 7, 9, 19, and 20 can function both as rules governing one-premise inferences and as rules governing two-premise inferences. Seen as rules licensing two-premise inferences they all govern a form of hypothetical syllogism and are distinguished (leaving aside differences in the ordering of conditions) only by the number of conditions they involve. There are three cases, and in principle could be more, that together reveal a pattern (see Table 7.1) that in turn shows something fundamental about the relationship between the rule of detachment and the rule of hypothetical syllogism.

Table 7.1 The pattern exhibited by Detachment and various forms of Hypothetical Syllogism.

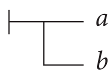
Detachment	Hypothetical Syllogism	Hypothetical Syllogism with Two Conditions	Hypothetical Syllogism with Three Conditions

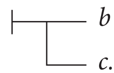


As the progression in Table 7.1 shows, although detachment and hypothetical syllogism can seem quite different as inference forms, we can also see them as deeply related insofar as any conditions on the judgment that *b*, if there are any, carry over to the conclusion. It is in every case the conditional *a*-on-condition-that-*b* that is supplying the inference rule or bridge. And because it is, one could not in the same way add conditions to that conditional and still have a rule of the same sort. The premise that *a*-on-condition-that-*b* is functioning as the major in Aristotle's sense. It provides the rule, the bridge from the other premise as ground to the conclusion. The other premise in each case is what Aristotle calls the minor; it provides a case to which the rule in the major can be applied.

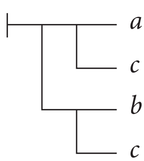
We have seen that theorem 9 enables the derivation of theorem 10, which licenses the reordering of conditions that occur in the condition of a conditional. Theorem 9 is used in that case as a one-premise rule to transform theorem 8 into theorem 10. But Frege also uses theorem 9, as he uses theorem 5 (and theorems 7, 19, and 20), to license two-premise inferences. Nevertheless, because he has, officially, only one rule of inference, what is in effect a two-premise (one-step) inference is actually conducted in two (one-premise) inferential steps, using either theorem 5 or theorem 9.[16] For reasons that will become clear, it is vital that we understand just what is going on here.

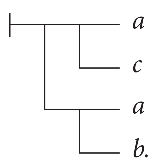
We begin with the two premises, *a*-on-condition-that-*b* and *b*-on-condition-that-*c*:





What we want to conclude, of course, is that *a*-on-condition-that-*c*. We cannot, however, do this directly given Frege's one rule of inference. Instead we must, according to Frege's official practice, construct from one of these premises, using either theorem 5 or theorem 9, a bridge that will take us from the other premise as ground to the desired conclusion that *a*-on-condition-that-*c*. We can either use theorem 5 on the premise that *a*-on-condition-that-*b*, or use theorem 9 on the premise that *b*-on-condition-that-*c*, the results of which would be these bridges:





From the first of these as bridge and the other premise, *b*-on-condition-that-*c*, as ground, we can now derive the desired result, that a-on-condition-that-c; and from the second as bridge and the premise that *a*-on-condition-that-*b*, similarly, the conclusion that *a*-on-condition-that-*c* follows. It is in just this way that one derives in two steps—by forming a bridge, from one of the premises, that will take one from the other premise as ground to the conclusion—what might otherwise be achieved in one step according to the rule of hypothetical syllogism. That is, one could also derive the conclusion directly from the two original premises, using either theorem 5 or theorem 9 read now as rules governing two-premise inferences.

[16] Frege deviates from this official position only once in the 1879 logic. In the proof of theorem 133 in Part III, in the derivation of theorem 102, with theorem 101 serving as bridge, both 96 and 92 serve as the grounds of the inference. The inference is from two judgments as premises. Indeed, as will become clear, this is actually a three-premise inference, from 92, 96, and 100, as governed by theorem 48, derived in Part II.

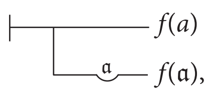
Although in some cases theorems 5, 7, and 9 really are used as one-premise rules of inference, in other cases it is clear that Frege uses these rules to derive in two steps what is in fact a (one-step) two-premise inference.[17] We see this, for example, in the move from theorems 52 and 55 to theorem 57. Although theorem 9 could have been used to license a two-premise inference directly from theorems 52 and 55 to theorem 57, what Frege actually does is first to construct a bridge using theorem 9 as a one-premise rule, namely theorem 56, and only then does he derive theorem 57 (with 52 as ground). Theorem 7, similarly, could have been used as a two-premise inference rule from axioms 28 and 31 as grounds directly to theorem 33 as conclusion but Frege instead uses theorem 7 (read as a one-premise rule) to first construct a bridge, theorem 32, and only then derives theorem 33 from 28 as ground.

Although in outward appearance all inferential steps in the derivations of Parts II and III of the 1879 logic have one and the same form (with the exception of the two-premise inferential step that gives theorem 102), in fact, we have seen, important differences can be discerned, both between cases that involve the application of a rule in the form of a bridge and those that are instead instances of the rule of detachment, and between, on the one hand, cases that are presented as two one-premise inferences but can equally well be thought of as single two-premise inferences, and on the other, those that just are one-premise inferences. Putting these two ideas together reveals another possibility as well. We saw that there is a nice pattern connecting the rule of detachment to the rule of hypothetical syllogism that becomes apparent when we consider cases of hypothetical syllogism involving more conditions. In each case there is a bridge of the form *a*-on-condition-that-*b*. The ground for detachment is simply *b*. For the most basic case of hypothetical syllogism the ground is instead *b*-on-condition-that-*c*, and hence, whereas in inference modus ponens the conclusion is simply *a*, for hypothetical syllogism the condition *c* must be carried over to the conclusion: *a*-on-condition-that-*c*. But the ground can also be, say, *b*-on-condition-that-[*c*-and-*d*], in which case there are two conditions that need to be carried over into the conclusion; in this case, the conclusion is of the form '*a*-on-condition-that-[*c*-and-*d*]'. It follows from these two features of inference in Frege's concept-script taken together that there can be both cases of hypothetical syllogism that are like detachment, which is the way one might most naturally think of hypothetical syllogism, as involving two mere conditionals that are on equal footing, and cases that in fact involve a rule—that is, something essentially general that can be applied in other cases—in the bridge.

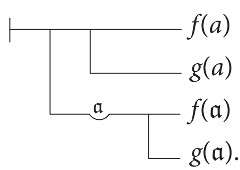
[17] Frege clearly recognizes that there are in logic both one and two-premise inferences. See his discussion of inference in Frege (1879, sec. 6), and his remark in Frege (1914, 204) that "we may distinguish two kinds of inferences: inferences from two premises and inferences from one premise." This distinction is, we will see, surprisingly important in the case of reasoning from defined concepts.

Where *a*-on-condition-that-*b* is functioning as a rule in what superficially appears to be a straightforward case of hypothetical syllogism, there is an important asymmetry between the two premises. In such cases, one is applying a rule to a formula in order to transform it in some way, but that formula happens to have a condition and this condition must then be carried over to the conclusion. Later we will see cases of just such an asymmetry between the two premises in what superficially appears as a straightforward case of hypothetical syllogism.

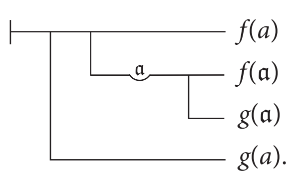
We have seen that *Begriffsschrift* formulae (like collections of lines in a Euclidean diagram) can often be regarded in various different ways, and need to be so regarded depending on what rule of inference one aims to apply. One consequence of this feature of Frege's notation is that something very like the phenomenon of pop-up objects that we observed in Euclidean diagrammatic reasoning and again in the proof of Euler's Theorem can also occur in steps of reasoning in Frege. (Again, this is something that will turn out to be very important in any fully adequate understanding of reasoning from concepts in *Begriffsschrift*.) Here is a simple example. We start with Frege's axiom concerning the concavity:



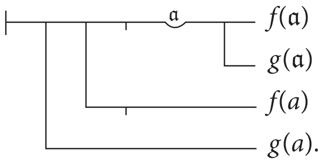
which expresses the thought that if some concept *f* has the (higher-level) property of being universally applicable then it can be inferred, for any object you like, that that object falls under the concept *f*. Now we consider a special case of this axiom with '*f*-on-condition-that-*g*' in place of '*f*':



Viewed in one way this formula is nothing more than a substitution instance of Frege's axiom. But we can also view it differently, as is made manifest when we reorder the conditions:



This rule of syllogistic reasoning, that if some particular thing is *g* and all *g* is *f*, then that particular thing is also *f*, just pops up when one forms the relevant substitution instance of Frege's axiom. Suppose now that we apply the rule of contraposition:



Up pops another rule of syllogistic reasoning, that if a particular object is *g* but not *f*, then it follows that some *g* is not *f*. Later we will see a case of an expression for a concept popping up in the course of a proof; one draws a certain inference and the resultant formula, suitably regarded, now contains a complex sign for a concept that is integral to the proof.

We know that Frege's aim in his little monograph *Begriffsschrift* was to take the first step in his logicist program of showing that arithmetic is merely derived logic. Ultimately, what Frege wanted to show is that all the concepts of arithmetic can be defined by appeal only to logical concepts and that all laws of arithmetic can be derived on the basis of purely logical laws. What mattered to him, then, was that his proofs be maximally rigorous, that *everything* on which a proof depends be set out in advance so that it is manifest what it depends on, whether logic alone or also some concepts and/or truths of a special science. "Because modes of inference must be expressed verbally" (Frege 1880, 37), that is, in natural language rather than in *Begriffsschrift*, Frege employs only one mode of inference in the 1879 logic. All other modes of inference are instead expressed as judgments, either as axioms or as derived theorems. Thus, although one can read various judgments derived in Part II as rules governing one- and two-premise inferences, they, or more exactly, substitution instances of them, are (with one exception) invariably used as premises in inferences by detachment, that is, they are read as conditionals with only one condition that the ground shows to be satisfied. All other conditions are to be conceived as conditions on the conditioned judgment, that is, on the judgment that is true given the satisfaction of that one condition. And one consequence of this practice is that it can encourage, especially in those antecedently disposed, the idea that reasoning in *Begriffsschrift* is merely mechanical, that content, ideas, are irrelevant to it. This, however, is false. As Frege repeatedly emphasizes, his concept-script is an expressive language—just as the formula language of arithmetic is. It formulates content in a way enabling rigorous reasoning. So far we have seen only the barest indication of this. To see more we need to consider the derivations in Part III of *Begriffsschrift*, derivations that begin not with axioms but with definitions of concepts.

7.3 A Second Pass Through

Because Frege, for the purposes of logicism, allows himself only one rule of inference, every step in any chain of reasoning can appear, and does appear to the unpracticed eye, to be the same as any other. It involves a ground and a bridge both of which either are or ultimately derive from either an axiom (or axioms) or a definition (or definitions). Using our earlier convention, we can, for instance, chart the course of reasoning that ultimately yields, say, theorems 48 and 51 in Part II, both of which are used in Part III, and also chart the course of reasoning that ultimately yields theorem 133 in Part III. The results are shown in Figures 7.2 and 7.3, respectively, and seem essentially the same. The only difference, aside from the greater length of the derivation of theorem 133, seems to be that the Part II derivations begin with axioms, using them now as grounds and now as bridges, and the Part III derivation begins with definitions as grounds. In the Part III derivation, as in Part II derivations, various theorems appear now as grounds and now as bridges. In particular, although Part II axioms and theorems (those numbered 1 through 68) are mostly used as bridges in Part III, there are also steps in which they appear as grounds. As inspection

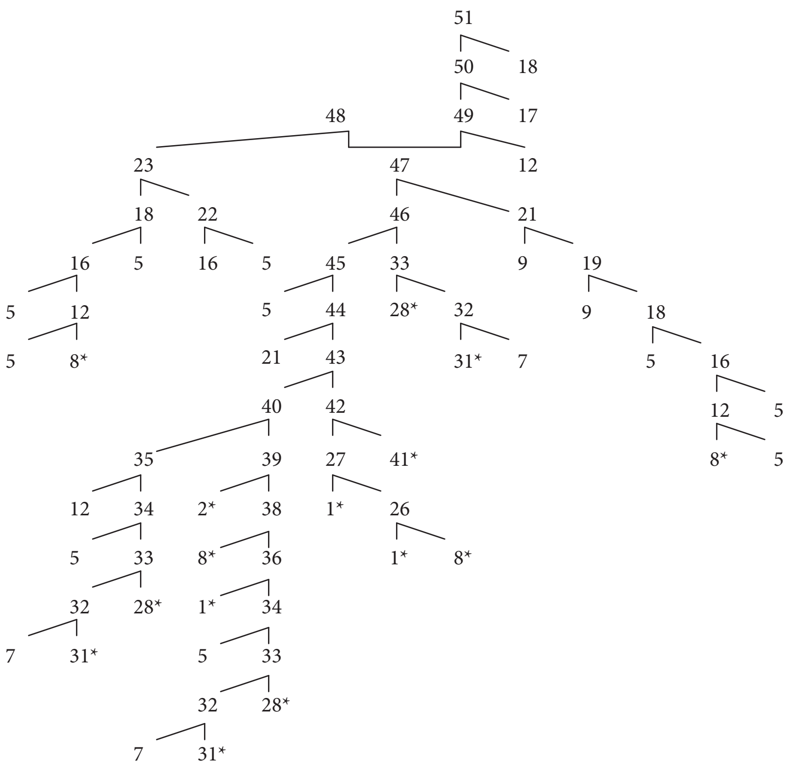


Figure 7.2 The course of the derivations of theorems 48 and 51 in Part II of *Begriffsschrift*.

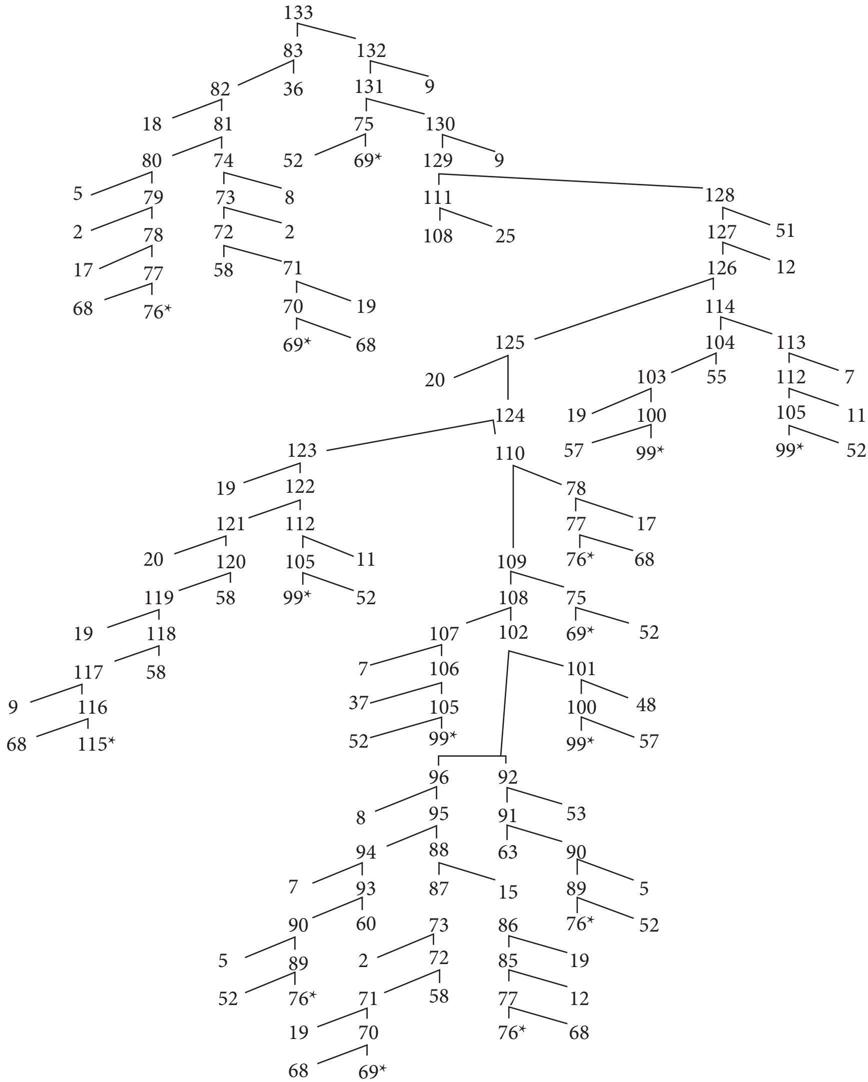
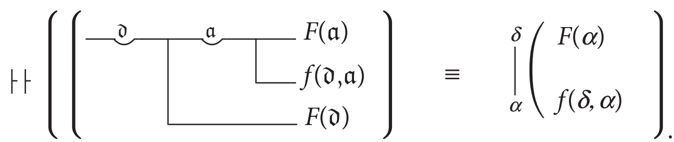


Figure 7.3 The course of the derivation of theorem 133 in Part III of *Begriffsschrift*.

of Figure 7.3 reveals, in Frege's derivation of theorem 133, theorems 36, 55, 58, 60, and 63 all occur as grounds. Nevertheless, as we will eventually see, there are crucially important differences to be discerned between any Part II derivation and the derivation of theorem 133 in Part III. As Frege himself says, the proof of theorem 133 is "*from* the definitions of the concepts of following in a series and of many-oneness *by means of* my primitive laws" (1880, 38; emphasis added). It is, we will see, Frege's definitions that provide the grounds of the proof of theorem 133; axioms and theorems from Part II function in that proof solely as inference licenses, as rules governing the passage from one judgment to another, beginning with the definitions. Once we have proved them they can be treated in Part III, where our concern is reasoning on the basis of definitions, as rules of inference of the sort one finds in a natural deduction system.

In Frege's system, a definition of a concept introduces a simple sign with the stipulation, marked by the double judgment stroke, that that simple sign is to have the same meaning, *Bedeutung*, as the complex of signs in the definiens. Because the signs in the definiens are already understood, and because the sense of the whole complex sign is a function of the senses of the parts, the sense of the definiens is displayed in the complex of signs that is the definiens. The definiens lays out the inferentially articulated content, the sense, of the relevant concept. Consider, for example, Frege's definition of being hereditary in a sequence:



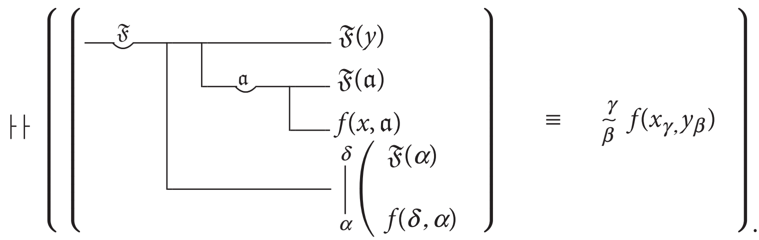
On the left is the definiens, a collection of primitive signs of the language that displays what it is for a property *F* to be hereditary in the *f*-sequence. On the right is a newly introduced simple sign; although this sign visually has parts, logically it is a simple two-place (second-level) predicate of the form '$\Theta(F,f)$' designating the property (of *F*) of being hereditary (in the *f*-sequence). Being hereditary in a sequence is thus a higher-level property of lower-level properties and relations. Being a positive whole number (which is a first-level concept that takes objects as arguments to give truth-values as values), for example, is hereditary in the successor sequence: it follows from being a successor of a positive whole number that that successor is a positive whole number. This is just what the definiens expresses using the concavity and conditional stroke: given that some object *d* is *F* and that *a* is the result of an application of *f* to *d*, that is, that $f(d,a)$, it can be inferred that *a* is *F*; or alternatively, that it can be inferred from the fact that *d* is *F* that if $f(d,a)$ then *a* is *F*. Thus, if one knows of some concept that it is hereditary in some particular sequence then one can infer of any object appropriately related to an object falling under the concept that it too falls under that concept. To apply the (higher-level) concept *hereditary in a given sequence* to some (lower-level) concept is in this way to issue an inference license regarding that latter (lower-level) concept, what may be inferred given that some object falls under the lower-level concept.[18]

[18] Some non-mathematical concepts manifestly contain inference licenses, for example, concepts of dispositional properties such as that of being soluble. To say that a stuff is soluble is to say that a certain inference with respect to it is good, namely, that if some of that stuff were put in water it could be inferred that the stuff would dissolve. Frege would express the content of such a concept using his conditional stroke and his Latin italic letters lending generality of content. For an extended discussion of the idea that Frege's letters lending generality of content serve, together with the conditional stroke, in the expression of rules licensing inferences see Chapter 1 of my (2005).

We saw already in section 6.3, that to trace or track truth conditions, what is the case if something is true, is not thereby to trace or track inferential consequences, what follows if something is true. The two notions are different and need to be distinguished. The distinction between them is furthermore crucial to an adequate understanding of what a definition such as that of being hereditary in a sequence expresses in *Begriffsschrift*, and in particular, the role of the concavity in such a definition as it contrasts with the role of a universal quantifier in a definition in standard mathematical logic. Suppose we write the definiens of Frege's definition in standard notation, namely, thus: $(\forall x)[Fx \supset (\forall y)(f(x,y) \supset Fy)]$. This formula states something that is the case if, and only if, F is hereditary in the f-sequence, namely, that all objects y that are the result of an application of f to any object that is F also have the property F. The definition so understood sets out necessary and sufficient conditions for something having the property of being hereditary in some sequence; it sets out what is true about objects if and only if some property is hereditary. It does not express an inference license, a rule according to which to reason, but instead a claim, something that is the case, something from which to reason.

Consider now a different case, that of being a prime number. Here again we can think of the content of this concept, what it is to be prime, in either of two ways, either in terms of marks, necessary and sufficient conditions, or in terms of the notion of an inference license, what one may infer given that some condition is satisfied. According to the first way of thinking about the content of the concept *prime*, to say that a number is prime is to say that no other number (greater than one) divides it without remainder. It is, in other words, to make a claim about numbers not equal to our number (and greater than one), all of them, that none divides our number without remainder. The alternative is to think of the concept *prime* as containing an inference license to the effect that *if* one is provided another number (greater than one) *then* it can be inferred that that number does not divide the given number without remainder. To ascribe the property prime to some number is not, on this second, inferential way of thinking, to say something about all numbers but instead to issue an inference license, one that can actually be employed only if some number is given that satisfies the condition in that license. To know, on this way of thinking, that some number is prime is to know that certain inferences involving that number are good. If the condition on the inference is met, then the inference may be drawn. And now we can see as well why a judgment such as that there is no largest prime number need not be taken to show that there are mathematical objects such as numbers. Read quantificationally, that judgment *does* commit one to there being objects that are prime numbers and indeed infinitely many of them. But if we follow Frege, we will think of that claim as instead licensing an inference, as an expression of the rule that if something is a prime number then it can be inferred that there is another larger than it. As we can also think of it, the judgment is about the concept *prime*, and ascribes a certain higher-level property to that concept. Objects do not come into it at all.[19]

We have seen that, as Frege defines it, the concept *hereditary in a sequence* contains, as its content, an inference license, that such and such can be inferred if some condition is met.[20] Frege's definition of following in a sequence is essentially similar, though more complex. This latter definition is also, Frege tells us (1880, 34), a paradigm of what he thinks of as a fruitful definition, one that enables a significant proof that extends our knowledge. The definition is this:



The question is how to read it.

The basic idea of following in a sequence is that one can get to the (following) value y from the (followed) value x by repeated applications of the function f. There is, however, no assumption that the function is single-valued, or that it forms a linear chain, as, for example, the + 2 sequence does. The chain could branch at various points, that is, have more than one result for some value as argument, and different branches could also merge. The chain, or part of it, could also close in on itself to form a ring. All that is required for y to follow x in the f-sequence is that from x by repeated applications of the procedure f one eventually gets y as value. The task of the definition is to spell out this intuitive idea in a form that is usable in inference. And here again the definition does not merely provide marks, what is the case if the defined concept applies, any more than the earlier definition of being hereditary in a sequence does. It displays not what *is* the case if the concept applies but instead what *follows* (inferentially) if it applies. To ascribe this relation of following in a sequence to some pair of objects is to issue an inference license, what can be inferred provided that some condition is satisfied, as Frege's own formulation of the definition in words makes explicit.

If from the two propositions, that every result of an application of the procedure f to x has the property F, and that the property F is hereditary in the f-sequence, it can be inferred, whatever

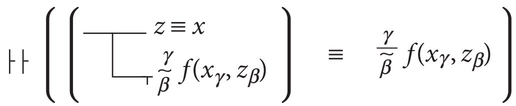
[19] And just the same is true of the so-called δ-ϵ definition of continuity. Although one can, with difficulty, think of it quantificationally, in terms of how things are with objects, one can also, and more easily, think of it as containing an inference license, what may be inferred if some condition is met.

[20] And Frege is quite explicit about this. As is evident from his formulation of the content of the definition in words, the definition concerns what "can always be inferred" given that some condition is met (Frege 1879, sec. 24).

F may be, that *y* has the property *F*; then I say: '*y* follows *x* in the *f*-sequence' or '*x* precedes *y* in the *f*-sequence'. (Frege 1879, sec. 26)

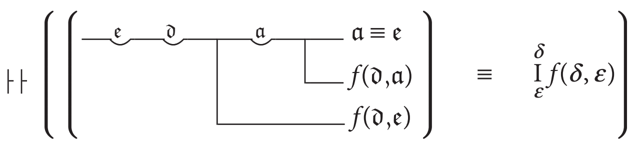
Here again, the definiens contains an inference license, something to the effect that something *can be inferred* given that appropriate conditions are fulfilled. If two objects *m* and *n* are such that *n* follows *m* in some *f*-sequence, then if one knows of some heritable property *F* that it belongs to any result of an application of *f* to *m*, one may infer that *n* is *F*.

Frege's third definition, of belonging to a sequence, is different. Here the definiens does not contain an inference license but is merely truth functional, as is marked by the fact that there are no concavities in the definiens. To say that *z* belongs to the *f*-sequence beginning with *x* is merely to say that *z* either follows *x* in the *f*-sequence or is identical to *x*:[21]



The definiens of this definition does not contain an inference license but only states what is the case—here, a disjunction, either this or that—if (and only if) the defined concept applies. One might well wonder why Frege would bother to define such a concept.

Frege's fourth definition, like the first two, contains an inference license, what may be inferred, given the satisfaction of certain conditions, if, and only if, a function is single valued:



Frege renders it in words thus: "If it can be inferred from the circumstance that *e* is a result of an application of the procedure *f* to *d*, whatever *d* may be, that every result of an application of the procedure *f* to *d* is identical to *e*, then I say: '*f* is a many-one procedure'" (Frege 1879, sec. 31). To say, on this account, that a function *f* is many-one (or as we will also say, single valued) is *not* to say something about the objects it relates one to another but instead to say something about the function, that the *function* has a certain higher-level property and hence that certain inferences regarding it are valid.

[21] As Frege's newly introduced sign for this relation indicates, the relation of belonging stands to the relation of following as the relation less-than-or-equal-to, $\leq$, stands to the relation less-than, $<$.

A definition is not a judgment but a stipulation: it "does not say, 'The right side of the equation has the same content as the left side.'; but, 'They are to have the same content.'" (Frege 1879, sec. 24). And because definitions are stipulations rather than judgments, rules of inference do not apply to them. One needs to make the identity judgment that corresponds to the stipulation. And this, though not a step of inference, is obviously correct: once one has stipulated that some newly introduced sign is to have the same meaning (*Bedeutung*) as some collection of signs that are already in the language then it is obviously and trivially true that they do have the same meaning, that the relevant identity is true. The reasoning begins with those identity judgments, and the first step is always to transform such identity judgments into conditionals, either with the definiens as the condition or with the definiendum as the condition.

Although the derivation of theorem 133 in Part III of *Begriffsschrift* is from definitions, more precisely, from the identity judgments that are trivially true given the definitions, while the derivations of theorems in Part II are from axioms, once we have formed conditionals from those identity judgments, all subsequent steps of both derivations can appear to be essentially similar. Superficially regarded, they are all (with one exception) instances of Frege's one rule of inference. Save for the derivation of theorem 102 from theorems 92 and 96 as grounds, with theorem 101 serving as the bridge, every step in every derivation, whether in Part II or in Part III, involves a ground and a bridge. In Part II, all bridges and grounds are, of course, either axioms or theorems derived from axioms. In Part III, theorems from Part II can serve either as bridges or as grounds, as can theorems derived in Part III. Nevertheless, as already indicated, Frege's axioms and the theorems that are derived from them in Part II in fact play an essentially different role in Frege's derivations in Part III from his definitions and the theorems derived from them. There is a pattern, or general strategy, discernible in the derivations of Part III that is wholly absent from any Part II derivation, even very involved ones such as the derivations of theorems 48 and 51. We need to learn to see this pattern.

Think again of the demonstration of Euler's Theorem on the basis of three identities regarding the exponential and two trigonometric functions that was rehearsed in section 6.5. That demonstration has three principal elements: (1) the identities with which the reasoning begins, identities that give the functions e^x, sin x, and cos x also as various infinite sums, (2) the successive preparatory rewritings of the identity involving e^x, with 'ix' for 'x', and (3) the stage at which content from different formulae are combined into one formula by putting equals for equals to give Euler's famous theorem. Reasoning from identities in *Begriffsschrift*, we will see, involves three analogous elements: (1) the definitions, or corresponding identities, with which the reasoning begins, (2) the successive preparatory rewritings governed by various one-premise rules of inference, and (3) a stage at which content from two different formulae is combined into one formula by means of some version of hypothetical syllogism.

We begin with two definitions, which in Frege's notation take the form

$$\Vdash\!\!\text{—}\ \mathit{definition} - \mathit{of} - \alpha \equiv \alpha$$

and

$$\Vdash\!\!\text{—}\ \mathit{definition} - \mathit{of} - \beta \equiv \beta,$$

where what is to the left, the definiens, is a complex expression formed from primitive signs, and perhaps also previously defined signs, and what is on the right, the definiendum, is a simple sign newly introduced that is stipulated to have the same meaning (*Bedeutung*) as the definiens. Immediately we judge that the relevant identities are true. The first step in the preparation is then to transform both judgments of identity into conditionals, into, for instance, α-on-condition-that-[*definition-of-*α] and [*definition-of-β*]-on-condition-that-*β*.

$$\begin{array}{ll} \vdash\!\!\top\!\!\text{——} & \alpha \\ \quad\llcorner\!\!\text{——} & \mathit{definition} - \mathit{of} - \alpha \end{array} \qquad\qquad \begin{array}{ll} \vdash\!\!\top\!\!\text{——} & \mathit{definition} - \mathit{of} - \beta \\ \quad\llcorner\!\!\text{——} & \beta \end{array}$$

That is, in one the definiens is made the condition and in the other it is the definiendum that is made the condition. (In practice, other combinations can also occur.) Then we transform those two conditionals in various ways according to rewrite rules until they share content. By two independent chains of inferences (one for each definition), we derive, in other words, something like α-on-condition-that-[*definition-of-*α]*, ultimately from the definition of α, and also something of the form [*definition-of-β*]*-on-condition-that-*β*, ultimately from the definition of *β*, where [*definition-of-*α]* is identical to [*definition-of-β*]*.

$$\begin{array}{ll} \vdash\!\!\top\!\!\text{——} & \alpha \\ \quad\llcorner\!\!\text{——} & [\mathit{definition} - \mathit{of} - \alpha]* \end{array} \qquad\qquad \begin{array}{ll} \vdash\!\!\top\!\!\text{——} & [\mathit{definition} - \mathit{of} - \beta]* \\ \quad\llcorner\!\!\text{——} & \beta \end{array}$$

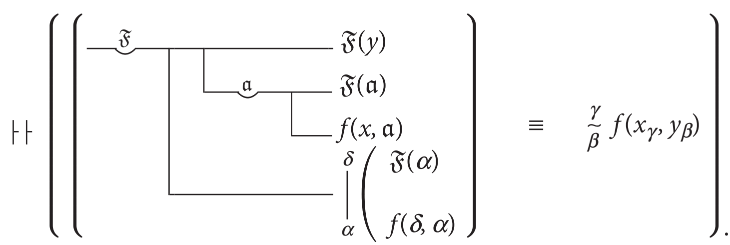
(In actual practice, things are of course not this simple.) Now because [*definition-of-*α]* is identical to [*definition-of-β*]* we can use hypothetical syllogism to join the defined signs *α* and *β* in a single conditional judgment, as mediated by the content that is common to the two conditionals, α-on-condition-that-*β*:

$$\begin{array}{ll} \vdash\!\!\top\!\!\text{——} & \alpha \\ \quad\llcorner\!\!\text{——} & \beta. \end{array}$$

This basic process is then repeated until all the signs occurring in the theorem that one aims to prove are appropriately joined.

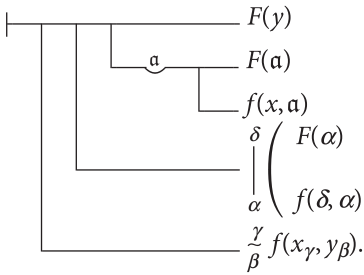
In a derivation from definitions according to the strategy just outlined, two essentially different sorts of moves need to be distinguished. The first sort of move, that made in the stage of preparation, is a one-premise inference, which we can think of as a *linear* inference because it functions merely to transform one formula, derived ultimately from the identity corresponding to a definition, into another formula. Rules governing such single-premise inferences are rewrite rules to the effect that if you have something of such-and-such a form then you may infer, rewrite it as, a formula that is thus-and-so. Such rules are analogous to rewrite rules in algebra such as that $a(b + c) = ab + ac$. The second sort of move, that in which content ultimately from two definitions is combined by hypothetical syllogism, is a two-premise inference. Such inferences are *joining* inferences that combine (by way of hypothetical syllogism) content from two different formulae ultimately derived from two definitions. Much as in a demonstration in algebra such as that of Euler's Theorem we needed *both* the axioms and theorems of elementary algebra governing rewritings *and* the general rule that equals can be put for equals, which allowed us ultimately to join the exponential function e^{ix} to the trigonometric functions sine and cosine in a single identity, so in the proofs of concern to Frege we need *both* rewrite rules governing one-premise inferences that serve to transform individual formulae *and* rules governing two-premise inferences, which allow us to join content from two formulae, derived ultimately from definitions by rewrite rules, into one.

Here is an example: the chain of linear inferences that effects various transformations on definition 76 (technically the identity judgment that corresponds to it) preparatory to the join with formula 74, which is derived from definition 69 by a series of linear inferences, that yields theorem 81.[22] The definition, again, is this:



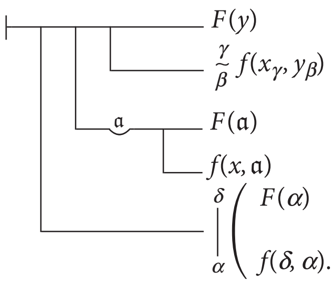
[22] See the top left part of the layout of the derivation of theorem 133 in Figure 7.3.

The first step is to convert this definition, more exactly the identity judgment that corresponds to it, into a conditional judgment with the outer-most concavity removed, theorem 77:



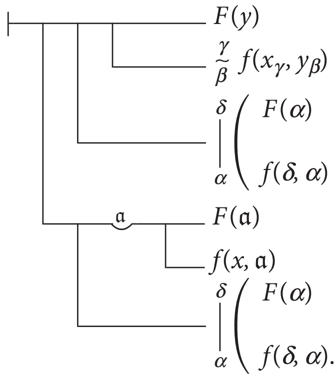
That is, we make the defined sign, the definiendum, a condition on the content that is the definiens of the original definition.

Now we make various modifications to this formula in a series of linear inferences preparatory to our join. First, we switch around the three conditions, licensed by one of Frege's many reordering theorems, to derive theorem 78:



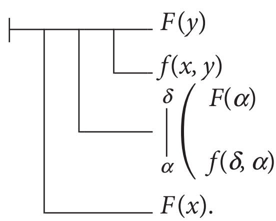
Notice that in order to do this we had to regard theorem 77, derived from the definition, in a new way. In order to see that theorem 77 is a conditional formed from definition 76, we had to regard the lowest condition on it as the condition on the rest of the formula conceived as the conditioned content. But in order to see that we can reorder the conditions as we just did to get theorem 78, we needed to treat everything except $F(y)$ as a condition on $F(y)$ as the conditioned content. And, we have seen, this point applies generally to reasoning in *Begriffsschrift*. Conditions that are at one point in the reasoning regarded as parts of the conditioned judgment *in* a conditional are at a different stage in one's reasoning regarded instead as conditions *on* a conditioned judgment.

Now we reorganize the content as licensed by Frege's second axiom to yield theorem 79:



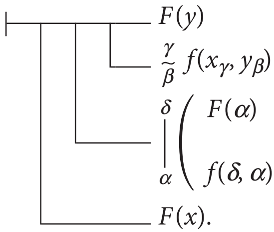
That is, we remove the lowest condition in theorem 78 and reattach it to both the condition and the conditioned content of the remainder suitably regarded. The preparation is complete.

We assume a similar preparation for the second formula needed in the join, namely, theorem 74. It is easy to see that theorem 74 is derived by a series of linear inferences from the definition of being hereditary in a sequence. We merely make the definiendum a condition on the definiens, remove the concavities, and reorder the conditions. This gives us theorem 74:



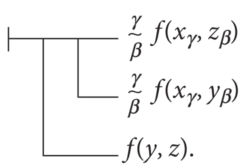
Notice now that the lowest condition (itself a conditional) in theorem 79 is identical—save for the presence of the concavity in theorem 79—to the conditioned judgment in theorem 74, provided that we regard theorem 74 as having $F(x)$ as the sole condition on that conditioned content. But we know (from *Begriffsschrift*, section 11) that we can insert a concavity in the relevant place in theorem 74, and hence, together, these two formulae yield, by hypothetical syllogism, theorem 81:[23]

[23] Again, in his actual practice, Frege does not directly join content from the two formulae as licensed by hypothetical syllogism. Instead he forms, from one of the two original formulae, a new formula that together with the other of the two original formulae yields the desired conclusion by his one rule of inference. In this particular case he uses theorem 5 to form a bridge (theorem 80) from theorem 79. That bridge takes us from theorem 74 to theorem 81 by Frege's one rule of inference.



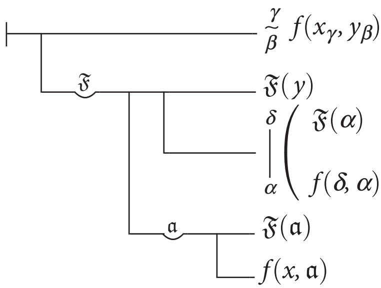
In this formula the lowest condition, $F(x)$, derives from theorem 74, ultimately from the definition of being hereditary in a sequence, and the rest of the formula derives from theorem 79, ultimately from the definition of following in a sequence. Notice further that theorem 81 differs from theorem 74 (derived by linear inferences from the definition of being hereditary in a sequence) in only one small—but crucially important as it will turn out—respect: where theorem 74 has as a condition that $f(x,y)$, that is, that y is a result of an application of the function f to x as argument, theorem 81 has instead the *weaker* condition that y follows x in the f-sequence. Theorem 81 shows that heritable properties are inherited not only by the results of applying the function f to things that have the property but by *anything* that follows, in Frege's technical sense, in the f-sequence. (This is most obvious if one switches the order of the two bottom conditions in theorem 81; then it is manifest that if F is hereditary then it is hereditary where y only follows x.) We will eventually see that this fact, that heritable properties are heritable also under weakening—that is, not only if $f(x,y)$ but also if y only follows x in the f-sequence—will turn out to be surprisingly important to any really adequate understanding of the proof of theorem 133 overall.

We have just seen that the general strategy of proof from definitions in Frege's concept-script is to find a way based on definitions to make joins that combine defined and other signs in conditionals. And we have seen one small example of this general strategy. Now we need to see the strategy working in reverse, that is, how one might *discover* a proof of some theorem using the strategy. We need to begin, then, with something that seems intuitively to be true given what our signs mean. It seems, for example, obvious given what it means to follow in a sequence that if y follows x in the f sequence and z is the result of an application of f to y, that is, $f(y,z)$, then z follows x in the f sequence. If you can get to y from x by repeated applications of f, and z is the result of an application of f to y, then obviously you can get to z from x by repeated applications of f:

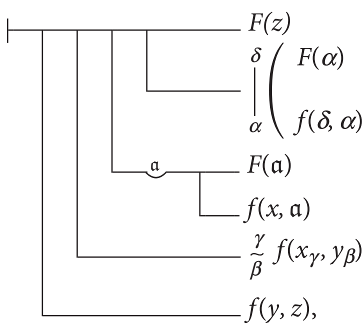


The task of the proof of this theorem is, in effect, to construct this formula and the structure of the formula immediately gives us a strategy. We have to get our signs to be joined from somewhere, and the only place is the definitions. So the structure of this formula—in particular, the fact that it involves two occurrences of the defined sign for following in a sequence, once as the conditioned judgment and once as a condition, and also, as a condition, a formula, $f(x,y)$, that occurs only in the definition of being hereditary—suggests that we will use the definition of following in a sequence twice, once for the conditioned judgment and once to get the defined sign as a condition, and will use the definition of being hereditary in a sequence once to get the condition that $f(y,z)$. Because three (occurrences of) definitions are involved, we will need at least two joins.[24]

Given our general strategy of making preparatory linear inferences followed by joins using (some version of) hypothetical syllogism, the construction of our theorem to be proved will obviously need the conditional that has the definiendum as the conditioned judgment that we can derive from the definition of following in a sequence:



Call this theorem I. We need it because we need to get the defined sign for following in a sequence to the right of the main content stroke, and given our strategy, forming this conditional (with z for y) is the first step on the way to doing that. If now we can prove this, call it theorem II:

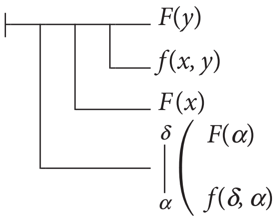


[24] Recall that, as we saw in section 5.4, Peirce held that deductive reasoning from concepts in mathematics can be constructive and hence ampliative. What we see here is that proofs that follow the general strategy for reasoning from definitions in Frege's *Begriffsschrift* can be conceived as construction problems, the task being to build the desired theorem given the relevant definitions. In section 8.3 we will consider in what sense such proofs ought to be conceived as ampliative.

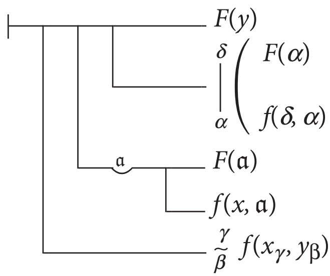
then we can use hypothetical syllogism on theorems I and II to derive the desired theorem. For what we have in this formula just is the condition of theorem I, but on two conditions, namely, that *y* follows *x* and that *z* is the result of an application of *f* to *y*, which are precisely the conditions in the formula we are trying to prove.

In order to derive theorem II we will obviously need both the definition of following in a sequence (because its defined sign occurs in this formula) and that of being hereditary in a sequence (because that is the only way to get the condition *f* (*x*,*y*)). And because both defined signs occur as conditions in Theorem II, we will need in particular these formulae, which derive directly from the definitions of being hereditary in a sequence and following in a sequence, respectively.

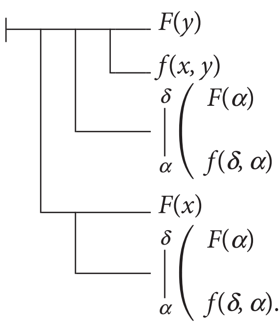
Theorem III



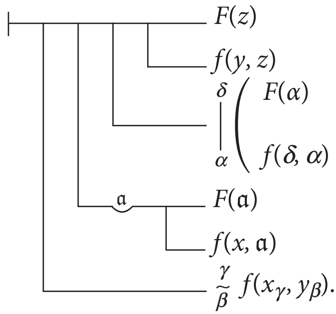
Theorem IV



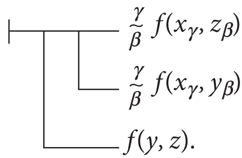
The structure of theorem IV immediately suggests that we use axiom 2 to transform theorem III so that it has a condition identical to the conditioned (conditional) judgment in theorem IV. That is, from theorem III, with axiom 2 as the bridge or inference license, we can easily get this, theorem V:



And now we can use hypothetical syllogism on theorems IV and V, though we need to re-letter in theorem V, putting *y* for *x* and *z* for *y*, to give theorem VI:



Now we just need to reorder the conditions in theorem VI to give theorem II, and add a concavity, and that theorem can be joined with theorem I by a version of hypothetical syllogism to give the theorem we aimed to prove:



From the definition of following in a sequence used twice (once to get a condition and once to get the conditioned judgment) and the definition of being hereditary in a sequence used once (to get the other condition) we proved our little theorem in just two joins. The essentials of the proof are set out in Figure 7.4. And all that was needed in order to discover this little proof was attention to the particular structure of the theorem we aimed to prove (what was a condition and what the conditioned content), together with our general strategy for proving theorems on the basis of definitions in Frege's concept-script.

There are a total of fourteen joins in Frege's derivation of theorem 133, one of which joins three (linear) chains and the rest of which join two. Figure 7.5, in which most of the linear inferences involved in the derivation of 133 are not shown, highlights the series of joins that take us from Frege's four definitions to theorem 133.

Ten of these joins are (or at least appear at first sight to be) straightforward joins using some version of hypothetical syllogism; that is, they are licensed by one or other of theorems 5, 7, 9, 19, and 20. This can be seen in Figure 7.3 by the fact that just before the join Frege uses one of these theorems to form a bridge out of one of the theorems in the join, a bridge that will take one from the other theorem in the join to the conclusion. Table 7.2 lists these ten joins, both as Frege presents them in two-step inferences and as two-premise (one-step) inferences by some version of hypothetical syllogism. (The content in this table can also be seen if one compares the relevant joins as presented in Figures 7.3 and 7.5.)

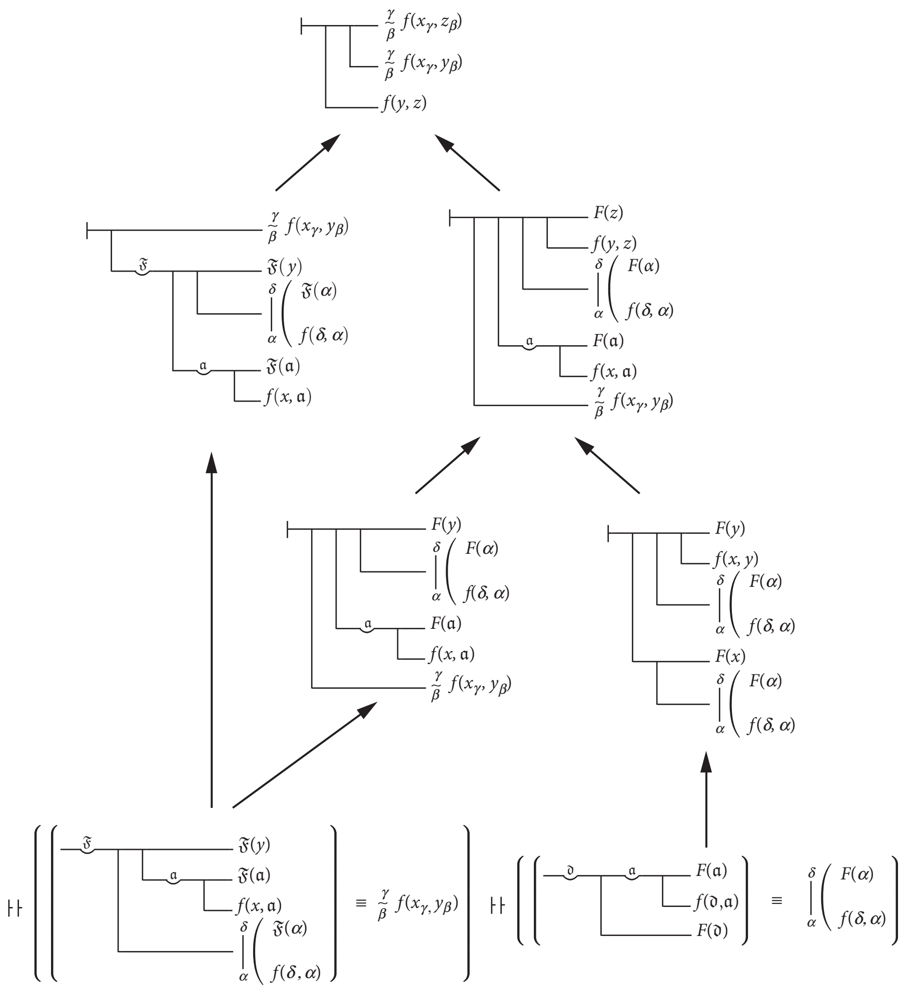


Figure 7.4 An example of the general proof strategy when reasoning from definitions.

The join of 92, 96, and 100 by theorem 48 is essentially similar, different only in involving three rather than two premises; although Frege uses theorem 48 to construct a bridge (theorem 101) out of theorem 100, one that takes one from theorems 92 and 96 to theorem 102, theorem 48 can be used directly to license the inference from 92, 96, and 100 to theorem 102.

The three remaining joins are different. Although in these cases two chains are joined, they are not joined by any form of hypothetical syllogism. Consider first the join of 108 and 75 to give 109. That this join is different is indicated already by the fact that both in Figure 7.3 of the derivation as given in the 1879 logic and in Figure 7.5 of the fourteen joins, just the same theorems appear in the inferential

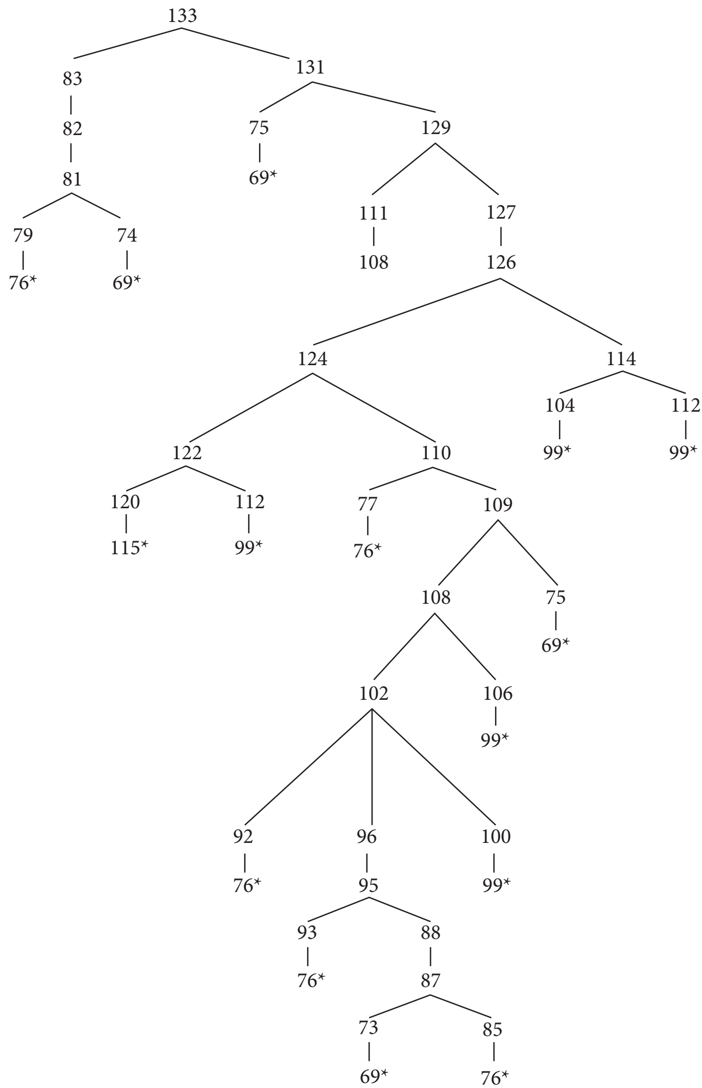


Figure 7.5 The fourteen joins in the derivation of theorem 133.

step. Here Frege is not, as he does in the cases just discussed, doing in two steps what might be done in one. What is happening here is something different. Theorem 75, derived directly from the definition of being hereditary in a sequence, states that if the condition for being hereditary in a sequence is satisfied by some concept, then one may infer that the concept *hereditary in a sequence* applies to that concept. Theorem 108 shows that belonging to a sequence satisfies the condition for being hereditary. Hence it is inferred (theorem 109) that belonging to a sequence is hereditary in that

Table 7.2 The ten (seemingly standard) joins in the proof of theorem 133.

Frege's Two-step Joins	The Corresponding One-step Inferences
1. derive 80 from 79 via 5; 80 is the bridge from 74 to 81	1. 79 and 74 yield 81 via 5 as two-premise rule
2. derive 86 from 85 via 19; 86 is the bridge from 73 to 87	2. 85 and 73 yield 87 via 19 as two-premise rule
3. derive 94 from 93 via 7; 94 is the bridge from 88 to 95	3. 93 and 88 yield 95 via 7 as two-premise rule
4. derive 107 from 106 via 7; 107 is the bridge from 102 to 108	4. 106 and 102 yield 108 via 7 as two-premise rule
5. derive 113 from 112 via 7; 113 is the bridge from 104 to 114	5. 112 and 104 yield 114 via 7 as two-premise rule
6. derive 121 from 120 via 20; 121 is the bridge from 112 to 122	6. 120 and 112 yield 122 via 20 as two-premise rule
7. derive 123 from 122 via 19; 123 is the bridge from 110 to 124	7. 122 and 110 yield 124 via 19 as two-premise rule
8. derive 125 from 124 via 20; 125 is the bridge from 114 to 126	8. 124 and 114 yield 126 via 20 as two-premise rule
9. derive 130 from 129 via 9; 130 is the bridge from 75 to 131	9. 129 and 75 yield 131 via 9 as two-premise rule
10. derive 132 from 131 via 9; 132 is the bridge from 83 to 133	10. 83 and 131 yield 133 via 9 as two-premise rule

sequence. The inference is essentially linear, a matter of rewriting a formula (108) that *shows* that belonging is hereditary, by showing that it satisfies the relevant condition, instead as one that explicitly *says* this (109), by using the defined sign for being hereditary, according to the rewrite rule derived from the definition of being hereditary.

The move from theorem 109 to 110 is analogous. Frege derives from the definition of following in a sequence—which, it may be recalled, includes the defined sign for being hereditary—what may be inferred given that some concept has the property of being hereditary in a sequence. This is theorem 78. Because theorem 109 says that belonging is hereditary, theorem 78 licenses the move to 110. The move is (conceptually) linear, a matter of rewriting 109 as 110 according to the rewrite rule (theorem 78) that is derived from the definition of following in a sequence.

The last join is that from 111 as ground, with 128 as bridge, to 129. Here Frege uses theorem 51 to construct the bridge out of 127. But it is easy to see that theorem 51, if construed as a two-premise rule, can take us directly from theorems 111 and 127 to 129. As in our earlier cases, Frege constructs a bridge in order to do in two steps according to his one rule something that can also be done in one multi-premise inferential step. Nevertheless, as we will see in more detail later, this is not a case of hypothetical syllogism because although content is joined, nothing is lost in the process; all the original conditions in 111 and 127 reappear in theorem 129, though differently combined.

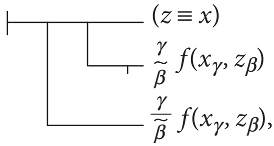
We have just seen that of the total of fourteen joins in the derivation of theorem 133 (all of which are displayed in Figure 7.5) eleven can be accounted for by appeal to the general strategy outlined above of modifying definitions in various ways until two defined, or in some cases other, signs can be joined using some version of hypothetical syllogism. The remaining three joins, resulting in theorems 109, 110, and 129, cannot be so accounted for. Why does Frege proceed as he does in these cases? Of course the steps are valid, and they do get Frege where he wants to go, but relative to our general proof strategy, they seem quite mysterious and unmotivated. And insofar as they do they suggest that there is something we have not yet grasped about this proof. Before we can see what it is, we need, however, to resolve another issue, that of the Part II theorems that appear in the derivation of theorem 133 as grounds rather than as bridges. We need to see that had Frege derived the appropriate theorems in Part II, all axioms and theorems from Part II would function in Part III as bridges, that is, as inference licenses, never as grounds. And this is just as we should expect. Those axioms and theorems of logic ought to be functioning as principles according to which to reason rather than as premises from which to reason, at least they ought to do so when one is reasoning about some particular subject matter such as the general theory of sequences as is the case in Part III of *Begriffsschrift*.

We have seen that in Frege's derivation of theorem 133 on the basis of his four definitions, theorems from Part II generally function as inference licenses governing the linear and joining inferences that are involved in the general proof strategy we have identified. They function either as rewrite rules, much as the axioms and derived theorems of early modern algebra do, or as some version of hypothetical syllogism, to join content from two different formulae (as the general rule that equals can be put for equals allows such content to be joined, for instance, in the demonstration of Euler's Theorem). But, again, in some parts of the derivation of theorem 133 theorems from Part II appear not as bridges, that is as rules governing rewritings or joins but instead as grounds, and hence, roughly speaking, as that to which a rule applies. As can be seen in Figure 7.3, theorems 36, 55, 58, 60, and 63 all appear in the proof of theorem 133 as grounds. What needs to be shown is that in all these cases Frege could have derived theorems in Part II that would provide the needed rewrite rules.[25]

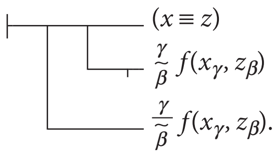
Consider first theorem 55, which is a simple theorem about equivalence: *d* is equivalent to *c* on condition that *c* is equivalent to *d*. This theorem serves as the

[25] Frege's logicist program is well served so long as it is shown in a fully rigorous way that theorem 133 is derived by appeal only to strictly logical concepts and strictly logical rules of inference. Relative to that program his way of proceeding is perfectly acceptable. If, on the other hand, one's concern is to understand *how* the reasoning works *as reasoning* then, as we will eventually see, one needs clearly to distinguish between the axioms and theorems derived from them, on the one hand, and the definitions and what may be inferred from those definitions, on the other. Frege's logicist program must not be confused with our philosophical one.

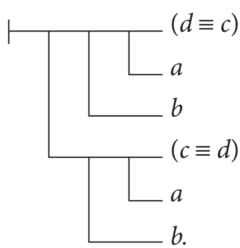
ground in the inference from theorem 103 to theorem 104. The fact that 103 is derived in turn from 100 using theorem 19 as bridge indicates that our interest should be focused on the move from 100 to 104, that is, from this:



to this:

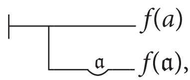


Clearly, the only difference between these two formulae is that whereas theorem 100 has z equivalent to x on two conditions, theorem 104 has x equivalent to z on those same two conditions. Theorem 55 cannot, then, be used directly to license the move from 100 to 104. What we need instead is a theorem that says that if you have an equivalence on two conditions then you may infer that equivalence with the terms reversed on those same two conditions. Because we know from theorem 5, read as a rule governing a one-premise inference, that if you have a conditional you can add any condition you like to both arms of that conditional, applying theorem 5 (read as a rule governing a one-premise inference) to theorem 55 and then again to the result yields just what is wanted:

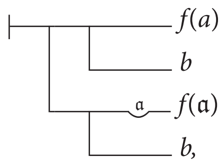


Had Frege derived this theorem in Part II, it could have served as the bridge in a linear inference from theorem 100 to theorem 104.

Axiom 58 serves as ground in three different inferential steps: in the derivation of 72 from 70 (which occurs twice in our display in Figure 7.3, once on the way to 81 and once on the way to 87), in the derivation of 118 from 116, and in the derivation of 120 from 118. The effect of this step in the first and last cases is to remove a concavity from the main content stroke when it is on two conditions; in the derivation of 118 from 116 the effect is to remove a concavity from the main content stroke when it is on one condition. Here again, then, it would have been easy for Frege to derive on the basis of his axioms the needed rule. Given axiom 58,

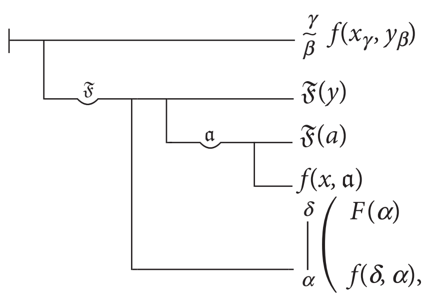


and theorem 5, treated as a one-premise rule, gives this:

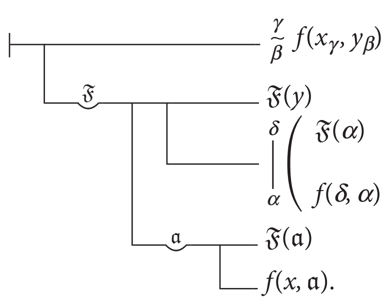


which provides the rule for an inference from 116 to 118. Applying the (one-premise) rule in theorem 5 again provides the rule for the cases in which there are two conditions.

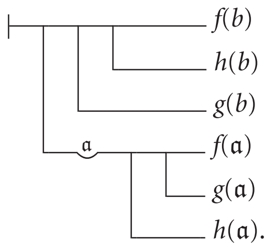
Frege uses theorem 60 in reordering the conditions in a conditional that has a concavity on its main content stroke and is itself a condition, that is, in the move from theorem 89:



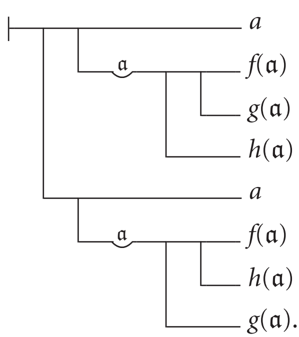
to theorem 93:



Theorem 60 is this:



To derive the rule that we need, we first insert a concavity into the main content stroke between the two left-most conditional strokes the scope of which is $f(b)$-on-condition-that-[$h(b)$-and-$g(b)$], and then use theorem 9 as a one-premise rule to derive our rule:



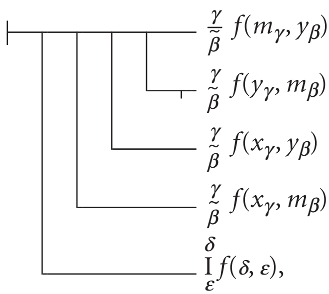
And just the same holds in the last two cases. Suitably modified theorem 63 provides a rule governing the inference from 89 to 91, and theorem 36 similarly provides the rule licensing the inference from 81 (with '$g(y)$-or-$h(y)$' for '$F(y)$' and the conditions suitably ordered) to 83 (after again reordering the conditions). Had Frege derived these theorems in Part II, it would have been *evident* that Part II theorems serve in Part III only as rules of inference, that is, as bridges, never as grounds.

Although, superficially regarded, every inferential step in a *Begriffsschrift* derivation of some theorem from definitions is the same as any other—and indeed the same as any inferential step in a derivation from axioms—closer inspection has revealed a range of very distinctive features for the case in which the derivation takes its starting point from definitions. First, although in a derivation of a theorem from axioms the axioms serve now as grounds and now as bridges, in a derivation of a theorem from definitions, the axioms and theorems derived from them (that is, formulae from Part II of the 1879 logic, those numbered from 1 to 68) serve as bridges. That is, they function as inference licenses, rules that allow one to move from the identity judgment corresponding to a definition, in a series of steps, ultimately to the theorem that is wanted. And as we have seen, there are two very different sorts of rules, rules governing linear inferences that merely change a formula in some way (for instance, by reordering its conditions or removing a concavity), and rules governing joining inferences that combine, by some form of hypothetical syllogism, content from two different judgments. The task of Frege's proof of theorem 133 is to find the sequence of joins that will eventually yield that theorem. And as we have furthermore seen, the structure of a theorem to be proved, in particular the relative locations of the various signs in that theorem, can provide important clues for finding the sequence of joins that is needed. In some cases that is *all* one needs to consider. In our derivation of theorem 95, for example, the structure of that theorem—the fact that the defined sign for following in a sequence occurs once as a condition and also as the conditioned judgment, with the only other condition being $f(x,y)$—told us everything we needed to know in order to derive that theorem from the relevant definitions.

But that general strategy is not sufficient for finding the proof of theorem 133. Although much of the proof is explained by appeal to the notion of a join that, by some form of hypothetical syllogism, combines content from two different judgments, we have already seen that some steps are not of this form and depend in a quite distinctive way on the definitions. We need to understand what is going on at these points in the derivation.

7.4 Seeing How It *Really* Goes[26]

The theorem proven in *Begriffsschrift* Part III is theorem 133:



which says that if f is a single-valued function and y and m both follow x in the f-sequence, then either m follows y or y belongs to the f-sequence beginning with m. Given what it means to follow in a sequence and to belong to a sequence, this seems

[26] I owe here a debt of gratitude to Julie Singer, Haverford class of 2012, whose work on reasoning in mathematics and in *Begriffsschrift* while a summer research assistant for me first catalyzed the work of this section.

intuitively to be true—on the condition that the function *f* is single-valued. (If *f* were not single-valued then it could happen that although both *y* and *m* follow *x*, they follow down different chains after a branching, in which case it would be false both that *m* follows *y* in the *f*-sequence and that *y* belongs to the *f*-sequence beginning with *m*.) The problem is to prove this theorem, and our general strategy suggests a task of construction. Given the definitions and the rules governing linear and joining inferences, the task is to find a series of joins that will connect together in the required ways all the defined signs that occur in theorem 133.

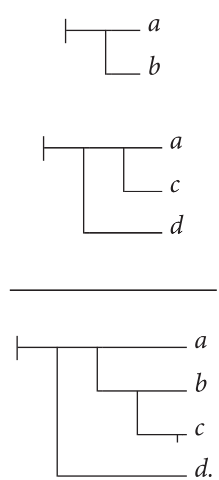
The general strategy of finding the joins that will relate in the required ways the various defined signs appearing in theorem 133 is clearly in play in Frege's proof of theorem 133; at least some parts of the derivation are fully explained in terms of this strategy. But, we have seen, not all are, suggesting that there is also the very particular strategy of *this* proof, a strategy that seems to depend in some way on the *particular* concepts, such as being hereditary and following in a sequence, with which it begins. What we are after are the key ideas of this proof in particular, the ideas that enabled its discovery in the first place.

We have seen already that of the fourteen joins in Frege's proof, ten of them are—or at least appear to be—straightforward applications of some form of hypothetical syllogism, and one more, namely, that using 48 to combine content from theorems 92, 96, and 100 to yield 102, although more complex, also conforms to essentially the same pattern. The remaining three joins are different. In the inference from theorem 108 to theorem 109, we have seen, one makes the transition from a theorem that *shows* that belonging to a sequence is hereditary in that sequence (108) to one that *says* that belonging to a sequence is hereditary in that sequence, using the defined sign for being hereditary to predicate of belonging to a sequence that it is hereditary in that sequence (109). The inference does not have the form of a hypothetical syllogism but is instead by Frege's one rule with theorem 75, derived from the definition of being hereditary, as the conditional or bridge. The move from 109 to 110 is again according to Frege's one rule: given that belonging to a sequence is hereditary in that sequence (109) it follows, with 78 (derived from the definition of following in a sequence) as the bridge, that belonging to a sequence has a certain other (complex and unobvious) property as well. Having shown that belonging to a sequence is itself hereditary in that sequence, we infer, based on a judgment derived from the definition of following in a sequence, that belonging to a sequence has some other property as well. That is what the two joins give us.

But there is a puzzle here insofar as it is not at all clear why the proof proceeds as it does at this point, what the guiding idea is. How did Frege come by the thought to use the property of being hereditary in this way? The question is all the more pressing once we realize that although superficially a sequence of two standard joins, the inferences from 129 and 75 to 131, and then from 131 and 83 to 133, are conceptually more like the sequence of two joins just outlined. As in the move from 108 to 109, Frege first shows (theorem 129) that a certain property is hereditary in the relevant sequence, and then (as in the move from 109 to 110) he shows, using a rule derived from the definition of following in a sequence, that something follows given that that property is hereditary, namely, theorem 133. The move *appears* to be a join by hypothetical syllogism, and hence just like the other standard joins involved in the general strategy, only because the property that is hereditary is on a condition that must be carried along at each step. Thus at two key points in the proof Frege uses essentially the same strategy: prove that some property is hereditary then infer something from that fact using a conditional derived from the definition of following in a sequence. We need to understand this strategy.

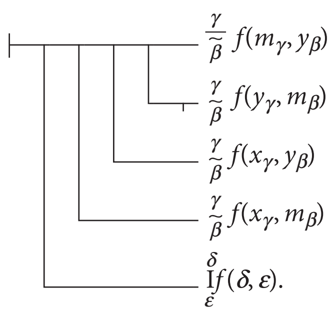
And there is a further puzzle as well. Frege suggests shortly after the publication of *Begriffsschrift* in 1879 that "I now regard it as superfluous to introduce the combination of signs [that form the definiendum of the definition of being hereditary in a sequence]" (1880, 28). According to Frege, theorem 133 is proved from only *two* of his four defined concepts, namely, following in a sequence and being single-valued (1880, 38). Given that belonging is merely disjunctive it is not surprising that the definition of this concept plays no essential role in the proof. Did Frege regard the defined sign as superfluous in this case as well? We have no evidence either that he did or that he did not. The concept *hereditary in a sequence*, by contrast, is clearly important to the derivation, in particular, at the two points just noted, though Frege says that the defined sign for that concept is superfluous. The concept *belonging to a sequence* does not seem to be important in the same way insofar as it does not play any such role in the derivation overall, and yet Frege defines it even though it is a very simple disjunctive property. Clearly there are things at work in the proof that we do not yet understand.

The final join only adds to the puzzle. Using theorem 51, which it is easy to see is functioning in this context as a rule licensing a two-premise inference, Frege joins theorems 127 and 111 to give theorem 129. The form of the inference, governed by theorem 51, is this:



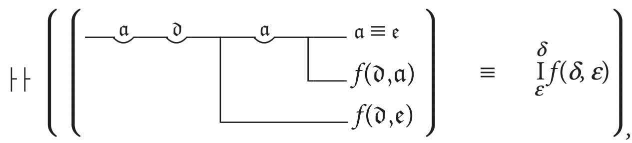
If a is true on condition that c and d, and a is true on condition that b, then it can be inferred that a is true on condition, first, that b or c, and also that d. As already noted, this is clearly not any form of hypothetical syllogism because although content is clearly joined, nothing is lost in the inference. All the conditions that occur in the two premises reappear also in the conclusion, though differently arranged. This is also clearly a valid rule of inference, as Frege shows in his derivation of it in Part II. The problem is to understand how Frege got the idea of making just this move at this point, why it is not merely effective but an intelligent move to make here. Obviously there is a sense in which it is a good move not only in itself but also for the purposes of the proof insofar as it enables Frege to get where he wants to go. And once one has seen how the derivation goes at this point, one will be able to reproduce it. Nevertheless, it seems unmotivated, something Frege just pulled out of the air. And it is of course possible that Frege himself merely stumbled upon the move, and seeing that it worked, used it. But if so, then the proof as a whole is essentially arbitrary at least at some points, and for that reason not fully satisfying as a piece of reasoning. The steps of the proof, each and every one of them, need to be *motivated*; otherwise, although we have a proof that shows *that* theorem 133 is true, we cannot adequately understand *why* it is true. We need to determine whether there is something that we are missing, some pattern to the proof that will enable *all* the steps to fall into place.

Consider again what we are trying to prove, theorem 133:

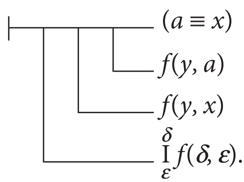


Visual inspection of this formula already suggests that our general proof strategy will not be sufficient insofar as we do not have only conditionals here but also a negation stroke the effect of which (given where it occurs) is to generate a disjunction. Now we do have a disjunction in the definition of belonging to a sequence—y belongs to the f-sequence beginning with x iff either y follows x or y is identical to x—but it should be clear that that is not going to be of any help here given that the disjuncts in 133 are belonging to a sequence and following in a sequence. The general proof strategy will not give us what we want. We need another.

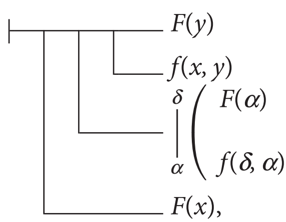
Theorem 133 says that if y and m follow x in the f-sequence (f single valued) then either m follows y or y belongs to the f-sequence beginning with m. That is, given the definition of belonging to a sequence, this formula says that if y and m follow x in the *f*-sequence (*f* single valued), then either *m* follows *y* or *y* follows *m* or *m* and *y* are identical. It will be helpful to have a name for this (doubly) disjunctive relation. We will say that *x* and *y* are *connected* in the *f*-sequence just if either *x* follows *y* in the *f*-sequence or *y* follows *x* in the *f*-sequence or *x* and *y* are identical. What we are to show, then, is that if *y* and *m* follow *x* in the *f*-sequence (*f* single-valued), then *y* and *m* are connected. Notice, now, that if a function *f* is single-valued, and *y* and *m* are the result of an application of *f* to *x*, that is, *f*(*x*,*y*) and *f*(*x*,*m*), then trivially *y* and *m* are connected because if *f* is single-valued and *f*(*x*,*y*) and *f*(*x*,*m*), then by the definition of being single valued, *y* and *m* must be identical. And if they are identical then obviously they are connected. That is, we start with the identity corresponding to the definition of being single valued:



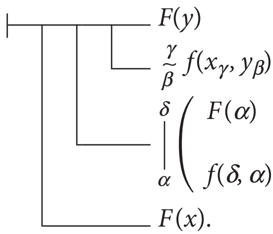
and derive what in Part III is theorem 120:



This formula suggests the beginnings of a strategy involving two related tasks. We need to show that something like this holds not only where *x* and *a* are the result of applying *f* to *y*, that is, *f*(*y*,*x*) and *f*(*y*,*a*), but also where *x* and *a* only *follow y* in the *f*-sequence; and we need to weaken the consequent property, show that where *x* and *a* follow *y* in the *f*-sequence not that they are identical but that they are connected in our technical sense. Given that connectedness is a disjunctive property one of whose disjuncts is an equivalence, the latter task should be relatively straightforward. The former seems more daunting. But we do have a precedent for it given that it is easy (as we have seen) to derive from theorem 74:

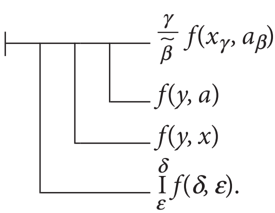


which follows directly (by a series of linear inferences) from the definition of being hereditary, theorem 81:



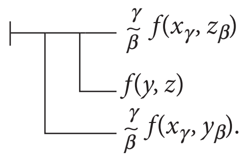
Theorem 81, we saw, is the result of a standard join by hypothetical syllogism of theorem 74 with 79, which derives (by a series of linear inferences) from the definition of following in a sequence. Furthermore, as already noted, theorem 81 differs from theorem 74 only in the topmost condition: the condition that y is the result of an application of f to x, that is, $f(x,y)$, has been weakened, replaced by the condition that y only follows x in the f-sequence. But that is just what we need to do for the two conditions $f(y,a)$ and $f(y,x)$ in theorem 120, weaken them to the conditions that a and x only follow y in the f-sequence. Perhaps, then, the definition of following in a sequence can also be used in these cases as well.

Given the definition of belonging to a sequence it is obvious that if x and y are identical then y belongs to the f-sequence beginning with x. So we can, by hypothetical syllogism, replace the equivalence in 120 with belonging, theorem 122:

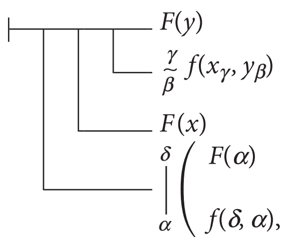


And again, because a disjunctive property is weaker than the property that is one of the disjuncts, adding the negated condition in order to form our disjunctive property connectedness should not be much of a problem. The real problem, again, is to find a way to replace the conditions $f(x,y)$ and $f(x,m)$ with weaker conditions that involve only the notion of following in a sequence. We have already seen that if a property is hereditary then we can show, using the definition of following, that it is hereditary under weakening. Our thought, then, is to use the property of being hereditary together with the definition of following in a sequence to weaken our two conditions as needed in theorem 133.

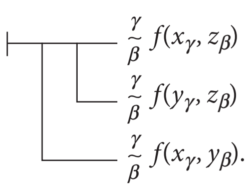
We know already that it is easy to show that following in a sequence is hereditary in that sequence using the general proof strategy of making joins by (some version of) hypothetical syllogism, that is, theorem 96:



(This formula differs from that we already derived using our general strategy only in the order of the two conditions.) And we saw in the derivation of 81 how to use the definition of following in a sequence to replace the condition $f(x,y)$, which appears in a condition derived from being hereditary, with the weaker condition of following. Theorem 84:



is just a reordering of theorem 81. What this formula says is that if some property F is hereditary in the f-sequence then that property is heritable even if y only follows x. Because we have shown that following in a sequence is hereditary in the f-sequence (theorem 96 above), it follows that following in a sequence is heritable even where y only follows x in the f-sequence (whether or not it is the result of applying f to x). That is, we can use 84 to license the move from 96 to 98:[27]



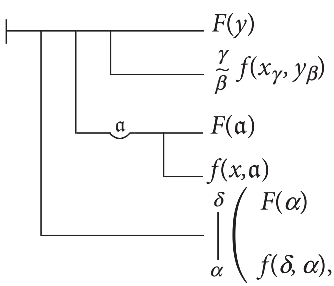
As this formula shows, following in a sequence has the property of being transitive. But as we can also think of it, we have, in this formula, *weakened* the condition,

[27] Technically, we would have first to derive from theorem 96, which shows that following is hereditary, a theorem that uses the defined sign for hereditary to say that following is hereditary. This is, however, trivial. As Frege notes shortly after *Begriffsschrift* was published, having a defined sign for being hereditary is superfluous.

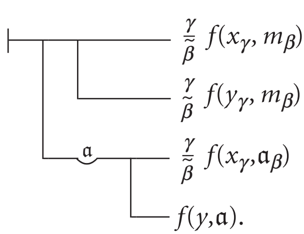
in theorem 96, that $f(y,z)$, to the condition, in theorem 98, that z only follows y in the f-sequence. Now this theorem (98) in fact has no role to play in the proof of 133; as far as that latter proof is concerned, theorem 98 is a dead-end. Nevertheless, Frege includes it and it gives us insight into a possible strategy for the proof of theorem 133 insofar as it shows—using the property of being hereditary in a sequence and, by way of the definition of following in a sequence, what follows from something having that property—one way to weaken a condition from something's being an application of f to its merely following in the f-sequence. We have a strategy to try.

To use the suggested strategy one needs first to show of some property that it is hereditary (that is, that it has the higher-level property of being hereditary) as we did for following in a sequence. An obvious thing to try to show is that belonging to a sequence is also hereditary in that sequence. Frege shows this in theorem 108, which, given that he has already shown that following is hereditary (theorem 96) and what it means to belong to a sequence, is relatively straightforward to prove. Now at this point Frege could have used the strategy above to show that belonging is heritable even where y only follows x. This, however, would not advance the proof. What we need to do is to alter the conditions in theorem 122, not the conditions in theorem 108. But we will still use the fact that belonging is hereditary.

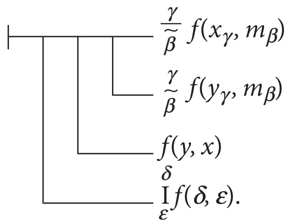
Given that belonging is hereditary in the f-sequence (theorem 108), and given this theorem, theorem 78:



which follows directly from the definition of following in a sequence, we can derive theorem 110:

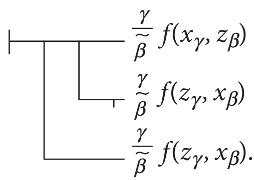


In this theorem, belonging to a sequence is the property F in theorem 78. Notice now that it gives us *another* way of replacing the condition $f(x,y)$ with the weaker condition that *y* follows *x*, namely, when it is a condition on something's belonging to a sequence. That is just what theorem 110, appropriately regarded, says. Theorem 110 together with theorem 122 yields, by hypothetical syllogism, theorem 124:

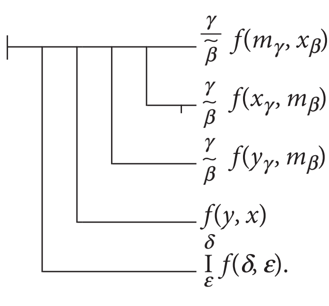


We have managed to replace one of our two conditions of the form *f*(*x*,*y*) with the weaker notion of following. The problem of the remainder of the proof is to weaken the other condition. And here Frege is especially brilliant. We know what the strategy is: show that some property is hereditary then somehow use the definition of following in a sequence to weaken the condition that *f*(*x*,*y*) to that of following. But what property should we do this for? We have already shown that both following in a sequence and belonging to a sequence are hereditary. What further properties are there? The answer is at once simple and elegant: the property of connectedness.

If we are going to prove that connectedness is hereditary, we need it in theorem 124 in place of belonging in the conditioned judgment to which the main content stroke takes one. This is easily done. First, we introduce the notion of connectedness in theorem 114:

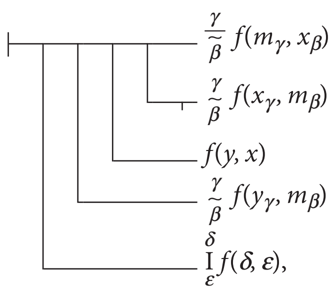


This theorem follows from a standard join of two judgments both of which derive from the definition of belonging to a sequence. Clearly it is true: if *x* belongs to the *f*-sequence beginning with *z* then *x* and *z* are connected. Now we can join 124 and 114 by hypothetical syllogism to give theorem 126:

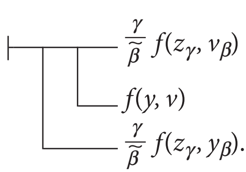


Notice that theorem 126 is just what we want to prove except for the fact that we have *f*(*y*,*x*); what we want is only that *x* follows *y* in the *f*-sequence. Our strategy for effecting such a change is, again, that of showing first that some property is hereditary then on that basis, using (somehow) the definition of following in a sequence, to weaken the condition involving an application of *f* to one involving only the notion of following. Because this is our strategy, we need to focus *not* directly on *f*(*x*,*y*) in theorem 126 (which is what the general proof strategy for reasoning from definitions would suggest) but *instead* on the condition that *m* follows *y* in the *f*-sequence. If we can somehow transform that condition into the condition that *m* and *y* are connected, that will show (with a reordering of conditions) that being connected is hereditary in the *f*-sequence (on condition that *f* is single valued). Now obviously we cannot do this directly, simply by adding a condition to that condition. But we *can* do it indirectly using theorem 51: if *a*-on-condition-that-[*c*-and-*d*] and *a*-on-condition-that-*b*, then *a*-on-condition-that-[[*b*-or-*c*]-and-*d*]. That is, Frege *constructs inferentially* the second occurrence of connectedness that is needed to show that connectedness is hereditary. The needed occurrence of the concept of connectedness just pops up when the inference is made. The move, which we will soon walk through step-by-step, is a delight.

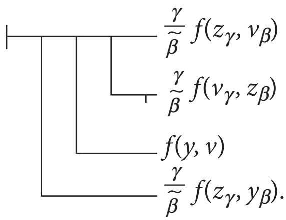
We already have the premise that *a*-on-condition-that-[*c*-and-*d*], namely, theorem 127 derived from 126 by reordering:



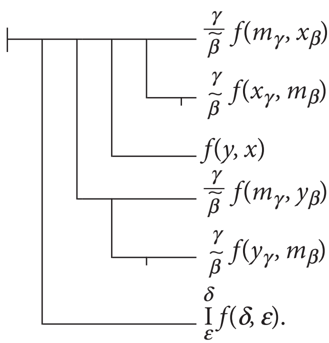
where *a* is the content that *m* and *x* are connected on condition that *f*(*y*,*x*), *c* is the condition that *m* follows *y* in the *f*-sequence, and *d* is the condition that *f* is single valued. What we need to show is that that same content that we put for '*a*'—namely, that *m* and *x* are connected on condition that *f*(*y*,*x*)—is also true on condition that *y* belongs to the sequence beginning with *m*. This is trivial to show. We already know that belonging to a sequence is hereditary in that sequence:



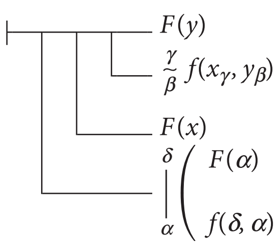
This is theorem 108. And we know that we can always add a condition to a judgment, any condition we like, and reorder. So given 108, we know that theorem 111 is true:



From this judgment, and theorem 127 we derive, by theorem 51, what is wanted, a theorem showing that connectedness is hereditary, on the condition that *f* is single valued, theorem 129:

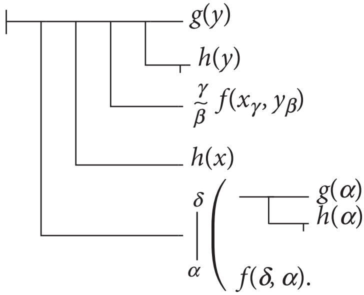


We further know that if a property is hereditary in the *f*-sequence then it can be inferred that that property is heritable even where it is not the case that $f(x,y)$ but only that y follows x, that is, theorem 84:



But this is not quite what is wanted. There is one more wrinkle.

It is a simple fact of logic that a disjunction is logically weaker than one disjunct taken alone. Hence it follows from theorem 84 that where the property we have shown to be hereditary is disjunctive, as connectedness is, then the following is also true, theorem 83:[28]



This is, of course, a rather peculiar case. One would not think of it unless one was working with our strategy of using the fact that some property is hereditary to infer something where the condition $f(x,y)$ has been weakened to y's merely following x. But having that general strategy in mind, we know that we will need something like 84 above, and it is not hard to see that what we need in particular is theorem 83. Having shown that connectedness is hereditary in the f-sequence (on condition that f is single valued), and using the rule in 83 that shows that if a disjunctive property is hereditary then it is heritable also given weakening to following and only one disjunct, we infer theorem 133. Figure 7.6 sets out the key moves in the proof.

And now it is manifest that although officially 129 and 83 are joined by hypothetical syllogism—that is, the inference looks to be an instance of our general strategy of proving things by hypothetical syllogism—conceptually it is instead a case of Frege's one rule of inference (as the move from 108 to 110 using 78 is), only with B on a condition that needs to be carried over into the conclusion. Conceptually, what is going on is that connectedness is hereditary (on the condition that f is single valued); hence by 83 connectedness is heritable on condition that one of its disjuncts applies even in the weaker case in which y only follows x (all on condition that f is single valued).

It is also now clear why the concept of belonging is defined and why the property of connectedness is not. Connectedness *is* a crucial property in any fully adequate understanding of the proof of theorem 133, but it is also critical to the proof strategy that connectedness is a disjunctive property, and hence constructible inferentially. It is not in the same way critical to the proof strategy that belonging is disjunctive; and because it is not, it makes things a little easier, though it is hardly critical, to introduce a defined sign for the concept of belonging. The definition of being a single-valued function gave us our template for the proof. Having that definition is, then, not merely useful or convenient; it is suggestive of how to approach the problem of finding a path from Frege's definitions to the desired theorem. The definition of following in a sequence gave us various conditionals that are needed at crucial points to show something that is entailed by some property's being hereditary in a sequence. Clearly that definition is central to the proof. On the other hand, we did not really

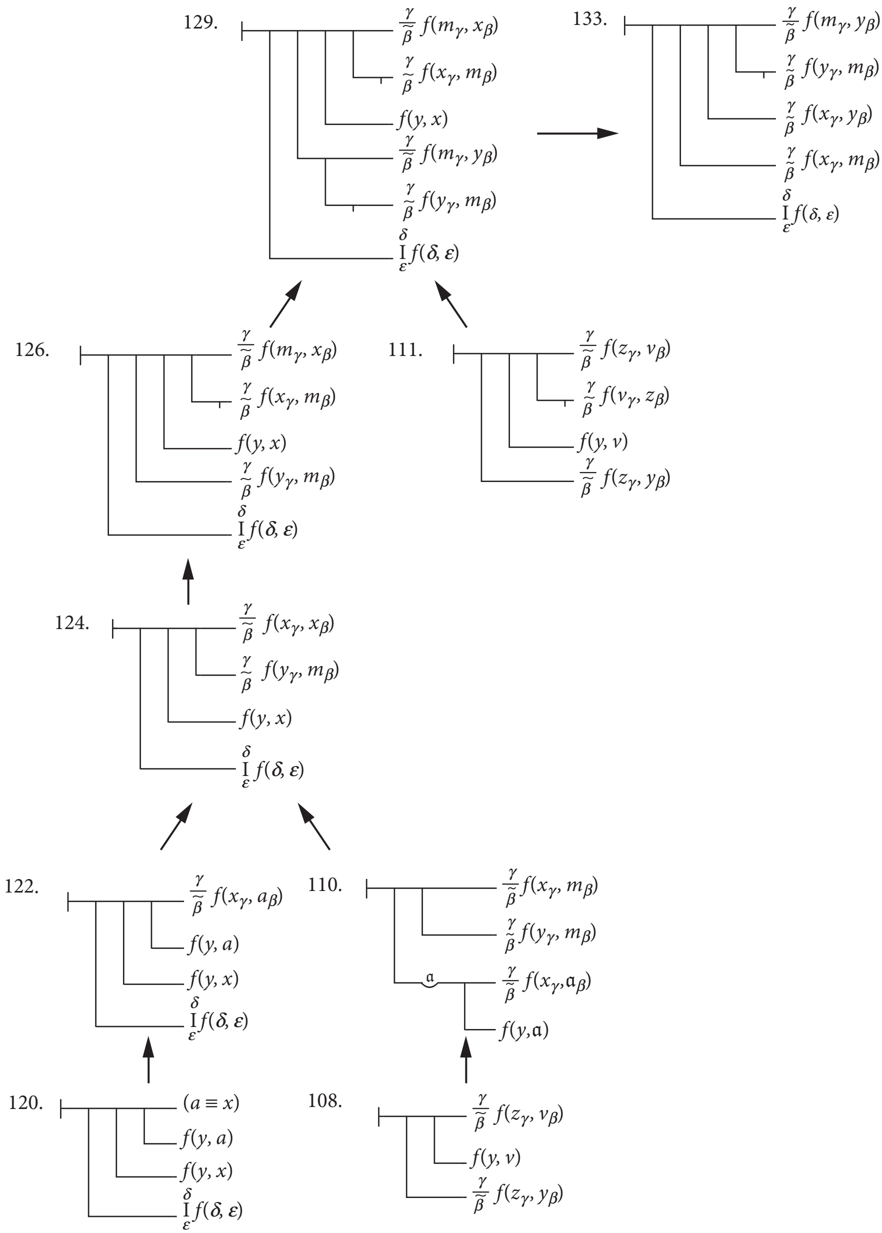


Figure 7.6 The key moves in Frege's proof strategy for theorem 133.

need a simple sign for being hereditary any more than we needed one for belonging to a sequence (though it does help to make the notion of being hereditary, which is central to the proof, more salient). In the end, then, the definitions that are crucial are, just as Frege says, the definitions of following in a sequence and of being a single-valued function. It is a marvelous and elegant little proof.

7.5 Conclusion

I have suggested, following Jourdain, that a good notation of mathematics is one that puts the reasoning before one's eyes, that displays it. One can do mathematics, and in particular the sort of mathematics that involves deductive inference from concepts, without such a notation, but in that case one's reasoning is and must be a private mental act rather than a public, observable one. We also saw in our discussion in section 6.5 of the Leibnizian notion of a universal language, a language that is at once a *characteristica* and a *calculus ratiocinator*, what is required of such a notation. Following the model of the formula language of arithmetic and algebra we determined, first and most obviously, that the notation must display the contents of concepts in a mathematically tractable way, in a way enabling rigorous reasoning *in* the system of signs. Because the mathematical practice of concern to Frege involves deductive reasoning from defined concepts, as contrasted both with the sort of algebraic calculating characteristic of mathematics after Descartes and through the eighteenth century, and with the diagrammatic reasoning of the ancient Greeks, the concepts of concern to Frege must be understood to be inferentially articulated, and their contents must be displayed in a way that is amenable to moves that are licensed by recognized rules of logical inference. Not only must primitive signs be introduced to enable the display of logical relations, axioms expressing basic truths about those primitive notions need to be formulated and theorems derived from those axioms. Those axioms and theorems can then function in the system as basic and derived rules of inference. Because the definitions are formulated using those same signs, the rules of inference can then be applied to them just as the axioms and theorems of basic algebra are applied in reasoning from identities in algebra, for instance, in the demonstration of Euler's Theorem. That, we saw, is what is needed in a Leibnizian universal language, and that, we have just seen, is what Frege's language *Begriffsschrift* provides.[29] *Begriffsschrift* is a language within which to reason deductively from defined concepts. A proof such as that of theorem 133 in Part III of Frege's 1879 logic, written in Frege's *Begriffsschrift*, *displays* the reasoning. It does not merely report it but instead puts it before our eyes in a way enabling us to *see* how it goes—or at least it does for one who has achieved adequate mastery of the system of signs.

We saw in section 6.4 that both Rav and Thurston object to formalized proofs written in standard systems of logical notation because they involve the merely mechanical application of rules rather than ideas. And Poincaré, as briefly discussed

[29] It is, however, not a universal language in the sense of being applicable in all cases. For reasons that will become clear in Chapter Eight, Frege's concept-script is suitable for deductive reasoning from concepts in mathematics in particular. It is not a general purpose language for all deductive reasoning.

in section 5.4, suggests something similar. He holds that when you break a mathematical proof all the way down to strictly logical inferences you lose sight of the mathematics, much as one focused on the cells of which the body of an animal is made up loses sight of the animal. We are now in a position to begin to understand why that is. In thinking about reasoning in *Begriffsschrift* we at first focused on the individual steps in Frege's derivations in the 1879 logic, all of which (except one) are essentially the same insofar as they conform to Frege's one rule of inference. But then we began to discern a pattern in the proof of theorem 133 that was absent from the derivations of Part II, even from the most complex of those derivations. There is, we saw, a general proof strategy at work in reasoning from definitions (at least those that have the form of generalized conditionals, that contain inference licenses), one that bears a strong structural analogy to the strategy discernible in our little demonstration of Euler's Theorem. And although it could not be shown here, it is not hard to verify that the course of much of Frege's derivation of theorem 133 is motivated and explained by just this strategy. But not all of it can be so motivated and explained. Indeed some steps that seemed at first sight to conform to the pattern of the general strategy, in fact, once we see how the proof really goes, ought to be regarded differently, as cases of the strategy that is peculiar to this proof in particular. To find that latter strategy we had to attend to the ideas of the proof, and in particular, to the concepts of connectedness, being a single-valued function, and following in a sequence. Only by working at the level of ideas, in Poincaré's imagery, at the level of the living, breathing animal, could we come to see what was really going on. And, surprisingly enough, this is true even in the case of pure logic with which Frege is concerned in *Begriffsschrift*.[30]

Furthermore, as should by now be evident, it takes real work and devoted practice with the language to come to be in a position readily to see *in* the system of signs the ideas that are at play in the proof. The point is, in other domains, a very familiar one. Whether one is learning a new language such as German or Greek, a new game such as chess, or a new area of mathematics such as Fourier analysis, one needs to go through a process of training and must practice (by reading *and* writing, or in the case of chess, reading *and* playing), and at a given point will be more or less capable. Much the same, we need to recognize, is true in the case of learning to reason in Frege's *Begriffsschrift*. Being able to see that each step is valid is not sufficient. One has full mastery of the language only when one is able to work at the level of the ideas of the proof as they are displayed in the notation; only then can one be said fully to understand the proof and the notation it is written in. And only then can one see for oneself how the proof actually goes, and so share in the pleasure of its discovery.

[30] Although Frege's definitions of concepts in the general theory of sequences are, at least some of them, crucial to the proof, those definitions involve only purely logical notions and hence are definitions of purely logical concepts.

We have learned the language, or at least enough to understand what it is capable of for the purposes of reasoning from concepts. Now we need to reflect on some of the philosophical ramifications of this language and the form of mathematical practice it puts before our eyes, what this language and this practice have to teach us about the nature and possibility of truth and knowledge in mathematics.

8

Truth and Knowledge in Mathematics

Our concern is to understand the striving for truth in the practice of mathematics, how this form of inquiry works, and Frege, I have suggested, holds the key, not Frege as he has been understood but Frege as we have learned here to understand him, within the particular historical context of nineteenth-century German mathematics. What Frege alone saw is that the developments in the mathematics of his day reveal not only that Kant's account of truth and knowledge in mathematics in terms of constructions in pure intuition must be jettisoned but that the most fundamental features of the Kantian system were in need of radical revision. Nowhere is this more manifest than in Frege's claim that despite being strictly deductive the proof of theorem 133 that Frege provides in Part III of the 1879 logic is ampliative, a real extension of our knowledge. Kant's division of judgments into those that are analytic, known by logic alone, and those that are instead synthetic, or ampliative, his conception of the synthetic a priori, his account of the distinction between concepts and intuitions, even the very idea of transcendental philosophy, all would need to be wholly rethought in the wake of the newly emerged practice of *Denken in Begriffen*.

It is characteristic of early modern thought, and of contemporary philosophical understanding insofar as it remains essentially Kantian, to be profoundly ahistorical, to turn its back on the past and to build, so it thinks, everything anew from the ground up. The search for foundations, an adequate axiomatic basis on which to build the edifice of mathematical knowledge, that occupied philosophers of mathematics for most of the twentieth century, and even some still today, is a paradigmatic expression of this form of consciousness. Supremely confident of its own powers, it fails altogether to realize that the revolutionary changes in mathematics in the nineteenth century *show* that its self-understanding is only a moment in a larger process of intellectual maturation and growth. The ahistorical understanding that was born of the revolutionary developments of the seventeenth century cannot withstand a second revolution. And nineteenth-century mathematicians such as Bolzano and Riemann, we have seen, were well aware of this. They knew that mathematical practice is fallible, that it is not grounded in any firm foundation but involves instead an essentially historical process of growth, maturation, and self-correction.

Although a foundationalist conception of knowing is very natural in the context of ancient thought, with everyday experience providing the needed foundation, developments in mathematics in the seventeenth century raised profound problems for foundationalism. A foundation cannot by its nature be founded in anything else, but how then is it known? The dogmatist holds that it simply *is* known; the skeptic rightly denies this but can offer nothing in its stead. The right response to the stalemate, Kant saw, was to reject the whole foundationalist picture, to focus instead on the processes of inquiry and the constitutive role played by our capacity for self-correction in those processes. But the point applies, Kant thinks, only to reason in its discursive use. It does not apply to the case of mathematics, to reason in its intuitive use. In the case of mathematics there *is* a given foundation, as secured by the pure forms of sensibility, on which to erect the edifice of mathematical knowledge. And given the nature of mathematical practice in the seventeenth and eighteenth centuries, in particular the role that Cartesian space plays in it, this is not an unreasonable supposition. It is developments in mathematics in the nineteenth century that show that it is untenable. What nineteenth-century mathematics suggests is that something like Kant's account of reason in its discursive use can, even must, be applied also to the case of mathematics.

In Kant's thinking the capacity for self-correction is closely linked to the theoretical, postulational character of early modern physics; and we hear an echo of this in Riemann and others. Riemann describes, for example, the foundations he proposes as having the status of hypotheses, and his work on the clarification of concepts as philosophical rather than mathematical. But Riemann's mathematical practice, though it is constitutively self-correcting, is not best thought of on the model of early modern physics and Kantian philosophy. It is not reductive and mechanistic, and although there is a sense in which it constructs theories aimed at explaining the appearances or data, the appearances or data in this case are earlier mathematical results. We shall need, then, a somewhat different understanding of the capacity for self-correction that is exercised in the mathematical practice of concern to us now from that we find in Kant.

We also need a different understanding of the role a written system of signs plays in mathematics given that, unlike earlier practices, contemporary mathematics is not essentially written. A language such as that Frege developed within which to do this new sort of mathematics is clearly not a necessary condition of engaging in this practice. Indeed, given that one of the principal aims of mathematicians such as Riemann was to free mathematics of the constraints of any and all symbolization, Frege's attempt to devise a system of signs within which to do this work can seem fundamentally misguided, even perverse. Worse, reasoning in Frege's *Begriffsschrift* seems, as we saw in Chapter 7, to be *very* like the paper-and-pencil practices that Kant held to involve constructions in pure intuition and so to involve reason in its intuitive use rather than in its discursive use. How can a self-correcting practice of deductive reasoning from concepts involve an intuitive use of reason? The answer, of course, is that it does not. Again, mathematicians reasoning deductively from concepts do not need a system of written signs within which to work. The practice, as a practice, is *purely* rational and in this regard essentially different from earlier mathematical practices. It is not *qua* mathematicians that we need Frege's language, or something analogous, but instead *qua* philosophers concerned to understand the nature of the reasoning involved. In order to grasp how the reasoning works in contemporary mathematical practice, in order to achieve an adequate philosophical understanding of this practice as a mode of intellectual inquiry, it is essential that it be publicly manifested. The purely internal, mental goings-on of the individual thinker are of no interest to the philosopher; our concern is instead with *the* reasoning and it is this that Frege's language enables, a rational reconstruction in a specially devised system of signs of the course of reasoning that takes one from one's starting points to the desired conclusion. The language, as a system of written marks, is obviously shaped by contingencies of our biological and social make-up, and by our historically conditioned writing practices. The reasoning it displays is not. The reasoning is wholly and purely rational, essentially the same for all rational beings, whatever their biological and social circumstances.[1]

We saw in Chapter 5 that in becoming purely conceptual, mathematics was to be freed of all accidental and arbitrarily imposed constraints, allowed to develop according to its own inner needs and requirements. Cartesian space within which we first come to understand the notion of a function is revealed to be merely one conceptual possibility among many, and functions first given through their analytic expression in equations are now to be characterized intrinsically through concepts. Those equations, at first taken to be the thing itself, as giving the subject matter of mathematics, are now revealed to be merely facets of what is really real in mathematics; they are the way that mathematical functions first become present to us, available for study, but not what they are in themselves. It is not by way of intuitions but through concepts of reason, Kantian ideas, that we at last come to know mathematics as it is. This, at any rate, is the way Riemann, Dedekind, and others following them thought of their new form of mathematical practice. Our task is to provide an account of how this is to work. Already we have had intimations of how the practice of deductive reasoning from concepts in mathematics, as that practice contrasts with earlier constructive mathematical practices, is the essential first step in dismantling the architectonic of Kant's transcendental idealism insofar as this new practice is not essentially written and does not presuppose space as a given infinite totality of possible positions. The task is to think through what this means, and

[1] Nevertheless, the fact that it can be displayed in a specially devised system of written marks is significant. In particular, it seems to show that there is something right about Kant's division of the uses of reason into one that is intuitive and one that is instead discursive. Although both mathematics and philosophy are a priori sciences, it appears that only mathematical reasoning can be displayed in a system of written signs. Philosophical reasoning seems instead to require the much richer expressive resources that are provided by natural language.

thereby to clarify how it is that by reason alone one might achieve mathematical knowledge of that which is its subject matter.

8.1 What We Have Seen

We saw in section 5.4 that Rav (1999, 11) distinguishes between a mathematician's proof and a strictly deductive, logical proof, where the former is "a conceptual proof of customary mathematical discourse, having irreducible semantic content," and the latter, which Rav calls a derivation, "is a syntactic object of some formal system." What Rav calls Hilbert's thesis is the logicist's claim that "every conceptual proof can be converted into a formal derivation in a suitable formal system: proofs on one side, derivations on the other, with Hilbert's thesis as a *bridge* between the two" (Rav 1999, 11–12). This bridge, Rav claims, is a one-way bridge.

> From a formalized version of a given proof, there is no way to restore the original proof with all its semantic elements, contextual relations and technical meanings. Once we have crossed the *Hilbert bridge* into the land of meaningless symbols, we find ourselves on the shuffleboard of symbol manipulations, and as these symbols do not encode meanings, we cannot return via the Hilbert bridge and *restore* meanings on the basis of a sequence of symbols representing formal derivations. (Rav 1999, 12)

Just as Poincaré had argued, to formalize a mathematical proof is to destroy it, at least as a piece of mathematics; it is to cross over a bridge from which there is no return. And as is becoming more and more widely appreciated, this is true in the case of standard mathematical logic: to formalize a mathematician's proof in mathematical logic is to destroy it as a piece of mathematical reasoning. What we saw in the last chapter, and will be explained in more detail below, is that this is not true in the case of Frege's logic. Hilbert's bridge conceived as a bridge to a derivation in Frege's concept-script is a two-way bridge.

Standard mathematical logic is, we have seen, deeply Kantian in its overall orientation. First, this logic is formal in just the same sense that Kant's general logic is formal: because it abstracts from all relation to any object, it is without content or truth. It furthermore conceives the activity of reasoning as the rule-governed manipulation of signs, something even a mere machine could be (and has been) built to do. To reason is to follow rules that require no insight or understanding to be applied, a conception that is reinforced by the fact that Descartes' new mathematical practice, unlike diagrammatic practice, has a method that is largely algorithmic. With these developments came also, we saw, a new model of understanding that was resolutely reductive and mechanistic. To understand is to reduce, for example, all of mathematics to set theory. A derivation in this logic is, as Rav says, a syntactic object of a formal system. Frege's proof of theorem 133, we have seen, is not.

Frege's language *Begriffsschrift* is not a formal system in our sense; it is, and is intended to be read as, a fully meaningful language within which to reason from defined concepts in mathematics. Frege is actually drawing inferences in the 1879 logic; he is reasoning *in* the language. Much as one reasons in the diagram in Euclid and similarly reasons in the symbolic language of arithmetic and algebra, so one reasons in *Begriffsschrift*. In all three systems of written signs content is formulated in a mathematically tractable way, in a way that enables rigorous reasoning (whether or not deductive) on the basis of that content. And this works, at least in the case of Frege's language, because by contrast with the primitive signs of mathematical logic, which have their own designation or meaning independent of any context of use, the primitive signs of Frege's logical language only express sense (*Sinn*) independent of their involvement in a formula and a way of regarding that formula. As a certain collection of lines can be seen to form a particular geometrical figure in Euclid but can also be seen differently, as part of a different figure, so a certain collection of primitive and defined signs in Frege can be seen to form a sign for a particular concept, though it can also be seen differently. As a triangle just pops up when certain lines are drawn in proposition I.1 of the *Elements*, so a sign for connectedness just pops up when a certain inference is made in Part III of *Begriffsschrift*.

As a system of written signs, *Begriffsschrift* functions in certain respects very like the system of signs that is employed in Euclid's *Elements*. In other respects, Frege's system of signs functions more like the formula language of arithmetic and algebra. First, and most obviously, the reasoning is not intra-configurational as it is in Euclid (the reasoning staying within the diagram) but instead trans-configurational. Steps of reasoning are steps of rewriting just as they are in elementary algebra. And as in that latter practice we can distinguish between two different sorts of steps of rewriting, both what I have called linear inferences, in which some formula is rewritten in some way according to a rule, and (what I called) joining inferences that combine content from different formulae, by putting equals for equals in algebra and by some form of hypothetical syllogism in Frege's system. And as in algebra identities involving both a simple sign for some concept or function and a complex sign for that same concept or function play a crucial role in the reasoning.

A definition in Frege's system is not a mere abbreviation.[2] It is a stipulation but only as regards the *Bedeutung* or designation of the newly introduced sign. The newly introduced sign is to have the same designation as the complex of signs (taken altogether as designating some one concept) that forms the definiens. The newly introduced sign, the definiendum, does not also express the same sense as that complex of signs—though it would were it functioning merely to abbreviate the complex of signs. Indeed, one can just see that the sense is different insofar as the definiendum is a

[2] Frege does occasionally describe a definition as introducing an abbreviation, but this does not seem to be what he, in fact, thought insofar as it is inconsistent with other things he thinks, and indeed ought to think given the way his language functions.

simple sign and the definiens is a *complex* sign, one that is highly articulated in ways that can be utilized in one's reasoning. Because the definiens is complex in a way that the definiendum is not, the rewrite rules of the system can be applied to it just as they can be applied to, say, an infinite series as contrasted with a simple sign such as 'sin x' or 'e^x'. What such identities involving both a simple sign and a complex sign reveal is that we are working in these cases with what we have called (following Grosholz) *intelligible* unities, wholes of parts that are nonetheless not reducible to their parts. The simple sign marks the unity of the relevant concept or function, the fact that it is one. The complex sign with which it is identified reveals that that one thing nonetheless has parts that are independently intelligible. Much as a line in Euclid is intelligible independent of, say, triangles but can nonetheless *be* a side of a triangle, so a primitive sign in Frege, say, the concavity or conditional stroke, is intelligible independent of, say, the concept *hereditary in a sequence* but can nonetheless *be* a constituent of that concept. As Frege famously remarks, at least some of his definitions are fruitful (as he puts it); they draw new lines. And that means, as we will see in more detail later, that they are wholes that despite having parts are not *reducible* to their parts (in relation).

It has been assumed for over a century that one understands Frege's system of signs when one knows the rules and can see how they are applied in individual steps of reasoning. This we have seen is wrong. Although one *begins* with such an understanding, to achieve *full* literacy and competence in the system is to come to see the *ideas* at work in the proof, *how* the defined concepts are used to guide the reasoning from the starting point to the desired theorem. It is in precisely this way, by learning to understand the proof at the level of ideas, that we *cross back* over what Rav calls the Hilbert bridge to the conceptual proof "with all its semantic elements, contextual relations and technical meanings" (Rav 1999, 12).

Poincaré objects to logicism on the grounds that replacing all mathematical modes of reasoning with a series of purely logical steps destroys the (mathematical) unity of a proof that is essential to mathematical understanding. Following Avigad (2008) we can think of the sort of mathematical understanding Poincaré has in mind in terms of certain sorts of capacities and abilities. A person who *understands* a proof, as contrasted with merely being able to see that each step is valid, possesses the following (Avigad 2008, 327–8):[3]

1. The ability to respond to challenges as to the correctness of the proof, and fill in details and justify inferences at the skeptic's request;
2. The ability to give a high-level outline, or overview of the proof;
3. The ability to indicate "key" or novel points in the argument, and separate them from steps that are "straightforward";

[3] These are only a subset of the marks of understanding a proof that Avigad sets out, but enough, I think, to establish what needs to be established for purposes here, namely, that one understands why the theorem is true, that is, the ideas of the proof.

4. The ability to "motivate" the proof, that is, to explain why certain steps are natural or to be expected;
5. The ability to indicate where in the proof certain of the theorem's hypotheses are needed, and, perhaps, to provide counterexamples that show what goes wrong when various hypotheses are omitted.

If a person can display the sort of understanding that is exemplified in these various ways then it seems fair to say that they understand the proof. And contrariwise if they can display such understanding, this shows that there is in the proof the relevant meanings and ideas to be understood. That is, we can use Avigad's criteria to establish that we really have crossed back over Hilbert's bridge.

We need to establish that formalizing a mathematical proof in Frege's language, that is, making it fully rigorous and gap-free, does not *destroy* the mathematics but *preserves* it—though it does take work to acquire the eyes to see the meanings that are still there after the formalization. And we can do that by showing that Avigad's criteria for understanding are satisfied; to show that the criteria are satisfied will be to show that meanings, the grounds of understanding, are still there in the formalized proof. The first capacity manifesting mathematical understanding is displayed in one's ability to respond to challenges to correctness, fill in details, and justify inferences. Obviously we have that insofar as the first thing one learns in learning to use Frege's system of signs is what the basic rules of inference are as given in the axioms and how to use those rules to derive various other non-basic rules of inference. All those axioms and derived theorems (set out in Part II of the 1879 logic) function in the system as the axioms and derived theorems of elementary algebra function; they are rewrite rules for the cases we really care about, namely, cases of reasoning from defined concepts. Even someone with only the most rudimentary literacy in the system can verify that all the steps in Frege's proof of theorem 133 are correct.

At a more advanced stage of literacy in Frege's language, one is able to see larger patterns, and in particular the general proof strategy for proving generalized conditionals on the basis of certain sorts of definitions, namely, those that contain inference licenses. Already at this stage of understanding one can indicate novel points in the argument, points at which content is joined by hypothetical syllogism, and can separate them from the straightforward steps in which identities corresponding to definitions are merely transformed on the way to a join. Because one has internalized the rules one no longer needs to write out each step, for instance, steps in which the order of conditions is switched or in which a concavity is added or removed. To be able to apply the general strategy in finding a proof, leaving out the obvious steps, is to satisfy the third desideratum. But we have seen that someone with only this level of understanding will not understand *all* the steps of Frege's proof; relative to that general strategy some steps seem merely ad hoc. Only at the final stage of full literacy can one motivate the central ideas of the proof and explain how in outline it goes. Only at this stage can one recognize that in a fundamental sense the proof begins with a conditional derived from the definition of being a single-valued function and proceeds by transforming that conditional into the theorem that is wanted, that *this* is the proper overview of the proof as a whole, of its unity as a proof. Someone who has achieved this level of understanding has satisfied the second and fourth criteria on understanding a proof.

The final test of understanding is the ability to indicate where in the proof conditions on the truth of the theorem are playing an essential role and to provide counterexamples showing what goes wrong when they are omitted. This we showed for the theorem to be proved: only if f is single valued is it true that m and y are connected on condition that both follow x in the f-sequence. It is also easy to show for the property of being hereditary in the case of connectedness: connectedness has the property of being hereditary only on condition that the relevant function is single valued. Suppose that x follows y, that is, that we can get to x by repeated applications of f to y. If at y there were a branching, with x following down one branch and z, the result of an application of f to y, starting another branch, then we would have x connected to y and $f(y,z)$ but not that x is connected to z. So the last of Avigad's criteria is satisfied as well. We have satisfied all the criteria of adequacy for understanding a proof and have shown thereby that the proof has content, meaning that is there to be understood. The bridge from the mathematician's proof, which is at the level of the ideas of the proof (the last level to be achieved in one's coming to understand the proof as Frege presents it), to a fully rigorous proof in Frege's concept-script is a two-way bridge. The proof can be fully formalized but even so one *can* recover the ideas of the proof, how it really goes and how it would have been discovered in the first place. A fully formalized proof in *Begriffsschrift* adequately understood is not reductive and does not destroy the mathematical meaning of the proof.[4]

The conception of reasoning and understanding that is the legacy of early modernity and generally accepted still today is, we have seen, reductive and mechanistic. The conception of reasoning and understanding that is required for any fully adequate understanding of Frege's formula language of pure thought is neither. The contents of concepts are not reduced to collections of primitive notions but are instead displayed in definitions as the conceptual contents they are, and those displays are mathematically tractable. One can reason rigorously in the system given those displays. Nor is the reasoning merely mechanical. Indeed, although it is deductive throughout, some steps of the reasoning are not merely by logic; they are

[4] We saw in section 5.4 that Descartes distinguishes an analytic proof, which focuses on meanings and does not *compel* assent, from a synthetic proof in which everything is made explicit and formally valid, and which thus compels assent. Analysis, he suggests, is the means by which a proof is discovered. What we have seen here is that Frege has given us a synthetic proof in Descartes' sense from which we have recovered the analytic proof that Frege must have first discovered.

licensed by a rule that is neither an axiom of logic nor a theorem derived from such axioms. Instead these steps are warranted by inference licenses that are derived from a definition. In order to discover a proof in such cases requires insight and understanding, the capacity to see in a definition its potential to provide such rules. The task now is to understand more clearly what all this means and also what it entails for our understanding of truth and knowledge in mathematics beginning with some reflections on mathematics as a domain of scientific inquiry.

8.2 The Science of Mathematics

We saw in Chapter 5 that in the work of Galois, Riemann, and other nineteenth-century mathematicians the practice of mathematics was, for the second time in its long history, radically transformed. As was noted in particular in section 5.3, this second transformation furthermore seemed (unlike the first) to constitute a second birth of the subject. The whole of mathematics was to be reborn as a purely conceptual enterprise purified of all the contingencies that had hitherto attached to it. Because the world of the mathematician came in this way to be an autonomous realm of meaning subject only to its own internal demands, traditional abstractionist conceptions of mathematical concepts were clearly inadequate to explain the nature of our cognitive access to those concepts. Instead, it was suggested, mathematicians adopted a "top-down" approach according to which the fundamental notions of the science are to be understood by appeal to higher-order properties. Although the concepts of contemporary mathematics may first be discovered by reflecting on instances, they are to be understood independent of any instances, defined by appeal to their internal constitutive contents and properties. Such concepts, we saw, have the character of Kantian ideas, concepts of reason, insofar as they are wholes of parts that are not reducible to their parts. The task of the mathematician is to analyze these concepts, clarify what exactly their mathematician meaning is, which can then be set out in a definition, and on that basis to prove theorems concerning such concepts. And for this one needs as well, though it is not yet completely clear why, to formulate axioms, basic propositions regarding one's primitive notions. As we noted, both Bolzano and Riemann were explicitly anti-foundationalist and fallibilist about both aspects of this practice. And Riemann was furthermore resolutely historicist about the concepts of mathematics; only over time, in light of the problems that emerge, do we achieve the conceptions we need in order fully to understand the mathematics we are concerned to understand.

We also saw, in Chapter 7, something of how this new form of mathematical practice works insofar as we saw displayed in Frege's concept-script just the sort of deductive reasoning from concepts that has been characteristic of mathematical practice since the nineteenth century. We looked both at examples of proofs of theorems from axioms, for instance, the little proof of theorem 5 from the first two of Frege's axioms of logic, and at examples of proofs of theorems from definitions, both the proof of theorem 133 and proofs of some of the subsidiary theorems that are needed along the way. In Frege's system, we saw, there are, first, the primitive signs, which are elucidated in Part I, and the axioms, provided in Part II, setting out the fundamental inferential significance of those signs as well as theorems derived from those axioms. As we furthermore indicated, Part III of the 1879 logic, which introduces definitions and theorems derived on the basis of those definitions, is essentially different insofar as the axioms and derived theorems from Part II are now, in Part III, to be used instead as rules of inference. Again, as Frege himself emphasizes, his proof of theorem 133 is *from* his definitions *by means of* the rules of inference set out in Part II. What we need now to understand is how this purely conceptual enterprise works as a science, in particular, how it can involve an autonomous realm of meaning to which one's judgments are nonetheless answerable. And for this, it will be argued, we need, as Frege already saw, something very close to the classical model of science first formulated by Aristotle.

According to the Aristotelian model, a system of concepts and judgments is a *science* just if it satisfies the following six desiderata (de Jong and Betti 2010, 186):

- All those *concepts* and *judgments* concern a certain *domain of being(s).*
- Among the *concepts*, some are *primitive* and the rest are *defined* by appeal to those primitive concepts.
- Among the *judgments*, some are *primitive* and the remainder are *proven* as theorems from those primitive judgments.
- The *judgments* of the science are *true*, *necessary*, and *universal.*
- The *judgments* are *known to be true*, either directly or through proof.
- The *concepts* are *adequately known*, either directly or through definitions.

This model, de Jong and Betti suggest, was in fact abandoned over the course of the twentieth century. Among the factors that led to its demise, they list the following:

> the improved standard of rigor that logic enjoyed after Frege; the restriction to deductive sciences; the discovery of non-Euclidean geometry and the ensuing debate of what geometry, if any, was 'the true one'; pluralism in logic; Hilbert's formalistic turn in mathematics together with debates on the notion of interpretation and meaningfulness of symbolism and the emergence of model-theoretical tools in semantics. (de Jong and Betti 2010, 197)

In short, if one follows the lead of mainstream twentieth-century analytic philosophy then one will be led to reject the model. But, we have seen, we need not follow that lead but can instead take the more radical path, opened up by Frege's work, that jettisons not only Kant's forms of sensibility and understanding but even Kant's conception of logic as purely formal and merely explicative. From the perspective of this latter path, the prospects for the Aristotelian model look very different.

Euclid's system as presented in the *Elements* has been, throughout history, the exemplar of a science in the Aristotelian sense. And so it is for Frege. He writes, for instance, in the introduction to *Grundgesetze* (1893):

> The ideal of a strictly scientific method in mathematics, which I have here attempted to realize, and which might indeed be named after Euclid, I should like to describe as follows. It cannot be demanded that everything be proved, because that is impossible; but we can require that all propositions used without proof be expressly declared as such, so that we can see distinctly what the whole structure rests upon. After that we must try to diminish the number of these primitive laws as far as possible, by proving everything that can be proved. Furthermore, I demand—and in this I go beyond Euclid—that all methods of inference employed be specified in advance; otherwise we cannot be certain of satisfying the first requirement. This ideal I believe I have now essentially attained. (Frege 1893, 2)[5]

But although Frege adopts this venerable conception of scientific method, he also subtly transforms it.

Frege was a logicist, and to be committed to logicism seems clearly to entail a commitment to something like the Aristotelian model of science insofar as the way to show that logicism is true is by systematically deriving all the basic truths of arithmetic from purely logical laws (by means of purely logical forms of inference), and defining all the basic and defined concepts of arithmetic using only purely logical concepts. But one can be committed to the model without being committed to logicism. If logicism is false then arithmetic is—as, a century on from Frege, it clearly seems to be—a science with its own primitive notions and its own primitive laws. But the way to show this, and thereby settle the question of the fundamental nature of arithmetic, is, again, by settling the question of its most basic laws and therefore also its most basic concepts. Frege explains in "Logic in Mathematics":

> Science demands that we prove whatever is susceptible of proof and that we do not rest until we come up against something unprovable. It must endeavour to make the circle of unprovable *primitive truths* as small as possible, for the whole of mathematics is contained in these primitive truths as in a kernel. Our only concern is to generate the whole of mathematics from this kernel. The essence of mathematics has to be defined by this kernel of truths, and until we have learnt what these primitive truths are, we cannot be clear about the nature of mathematics. (Frege 1914, 204–5)

The mathematical demand for the utmost rigor, which includes the demand for proof where proof is possible, and the philosophical question of the true character of mathematical knowledge, whether purely logical or grounded in distinctively mathematical truths, results in one and the same requirement:

> that the fundamental propositions of arithmetic should be proved, if in any way possible, with the utmost rigor; for only if every gap in the chain of deductions is eliminated with the greatest care can we say with certainty upon what primitive truths the proof depends, and only when these are known shall we be able to answer our original questions. (Frege 1884, sec. 4)

[5] In "Logic in Mathematics" (1914, 205), Frege is less charitable, claiming that "Euclid had an inkling of this idea of a *system*; but he failed to realize it."

Whatever the status of logicism, Frege clearly thinks that the demand for the utmost rigor, and hence proof where proof is possible, which requires in turn the analysis of concepts and the formulation of definitions of them, is a requirement of mathematics as a science: "in mathematics we must never rest content with the fact that something is obvious or that we are convinced of something, but we must strive to obtain a clear insight into the network of inferences that support our conviction" (Frege 1914, 205).[6] But it can seem that the model inevitably commits us to something more, not only to rigor in mathematics but also to a false epistemology, in particular, that it commits us to a demand for certainty or infallibility in mathematics. And this seems to have been the way it was understood, for instance, by Descartes and by Kant. Such an understanding is not, as Frege shows, constitutive of adherence to the model. It is perfectly coherent *both* to take the model to be the standard of scientific rationality in mathematics *and* to be fallibilist about mathematical knowledge, that is, to take it to be an "experimental" science, one whose foundations are not given and are never indubitably certain. As we saw in section 5.3, not only Frege but both Bolzano and Riemann hold this pair of views as well. Frege does claim, at the end of the introduction to *Grundgesetze*, that no one will be able either to produce a better system than his or to show that his principles lead to contradiction, but this is nothing more than an expression of his confidence that he has gotten things right. The test of one's logical convictions, Frege thinks, lies in what one can do with them, and it can always turn out (as it did for Frege) that, unbeknownst to one, one has gotten something wrong. Russell's discovery of the flaw in Frege's Basic Law V, however personally devastating to Frege, nevertheless held out for him the promise of being the crucial first step in "a great advance in logic" (Frege 1902, 132). And it did so because, for Frege, "the first prerequisite for learning anything" is "the knowledge that we do not know" (Frege 1884, iii).

For Frege, as for Socrates (and Peirce), the first prerequisite for learning anything is the knowledge that one does not know. Frege furthermore draws an intimate connection between this first prerequisite and the Aristotelian model, at least for the case of mathematics. It is, he thinks, only by adhering to the model and thereby making as explicit as one can just how one understands things to be that one is put in a position to discover that one does not know something one had thought one knew. Frege explains, in evident frustration, with reference to Weierstrass's mathematical work in particular:

> He was lacking in the first requirement—knowledge of his own ignorance. He saw no difficulties at all, everything seemed clear to him, and he didn't notice that he was constantly

[6] Frege makes the point also in *Grundlagen*. Although "the mathematician rests content if every transition to a fresh judgment is self-evidently correct, without inquiring into the nature of this self-evidence, whether it is logical or intuitive" (Frege 1884, sec. 90), if we are to succeed in distinguishing what is logical from what is intuitive, "the demand is not to be denied: every jump must be barred from our deductions" (sec. 91).

deluding himself. He did not possess the ideal of a system of mathematics. We do not come across any proofs; no axioms are laid down: we have nothing but assertions which contradict one another. And when on occasion an inference does seem to be drawn from his definition, it is fallacious. If he had but made the attempt to construct a system from the foundation upwards, he could not have failed straightway to see the uselessness of his definition. He had a notion what a number is, but a very hazy one; and working from this he kept on revising and adding to what should really have been inferred from his definition. (Frege 1914, 221)

Without the rigor imposed by the model, Frege suggests, it is all to easy to delude oneself into thinking that one has grasped some concept clearly and distinctly when in actuality one is completely confused.[7]

It is a very familiar fact of intellectual life that one can think that one understands something clearly and distinctly even though, as one later realizes, in fact one does not. Because one can think that one understands something that in fact one does not, the discovery of a problem that makes one *realize* that things are not clear, or distinct, can be of immense intellectual value, the necessary first step on the way to a better understanding. And the point applies, Frege thinks, in *all* domains of (exact) science, including even logic. The laws of logic are truths, and "what is true is true independently of our recognizing it as such. We can make mistakes" (Frege 1882, 2). Of course, in most cases we have no reason to doubt the truth of this or that law of logic, the law of identity, say, that $a = a$. Such a law seems to us as manifestly true as anything could be. Because it does, we cannot imagine that it might be false. But, Frege suggests, we can imagine beings who do doubt it, which is to say that we can imagine ourselves coming to have doubts about it, just as Russell brought Frege to have doubts about Basic Law V. We cannot (now) imagine what those doubts would be; if we could, we would have those doubts. But we can imagine having doubts. As Frege puts the point: "the impossibility of our rejecting the law in question [the law of identity] hinders us not at all in supposing beings who do reject it; where it hinders us is in supposing that those beings are right in so doing, it hinders us in having doubts whether we or they are right" (Frege 1893, 15). Because we have now no doubt at all that the law of identity is true, because we (now) can find no reason whatever to call it into question, we can have no doubt now that we are right to affirm it, and correlatively, that anyone who does not affirm it is wrong. But even so, we can imagine beings who reject the law of identity, which, again, is just to say that we can imagine ourselves coming to reject it on grounds that are as yet unimaginable. Mere conviction in mathematics and logic is not enough. The ground of the conviction must be laid bare by proving what can be proved and by defining all but a handful of primitive terms.

[7] It does *not* follow that one should *begin* a new area of mathematics by laying down axioms and formulating definitions. It is only later, after one has achieved some knowledge of how things are in that new domain, that is it necessary, or even possible, to try to systematize what one knows. Nothing Frege says denies this.

Frege furthermore provides us with an account of *how* it is that we can seem to be clear about something when in fact we are not, and as a constitutive element of that account, a conception of how it is that we can discover our error. Because all our awareness of what is objective is mediated by sense, *Sinn*, we can think we have grasped what is in cases in which in fact the outlines of the sense are confused and blurred. We need, then, to distinguish between a concept and our knowledge of a concept, between, on the one hand, "the logical and objective order" and, on the other, "the psychological and historical order" (Frege 1885a, 136).

A logical concept does not develop and it does not have a history . . . If we said instead 'history of attempts to grasp a concept' or 'history of the grasp of the concept', this would seem to me much more to the point; for a concept is something objective: we do not form it, nor does it form itself in us, but we seek to grasp it, and in the end we hope to have grasped it, though we may mistakenly have been looking for something when there was nothing. (Frege 1885a, 133)

Mathematical and logical concepts are something objective that we can grasp and can fail to grasp. Furthermore, because our grasp of a mathematical concept is through an inferentially articulated sense (laid out in the definiens of a definition formulated in *Begriffsschrift*), we can discover by reasoning, by drawing inferences on the basis of the sense of a concept word, as least as far as we understand that sense, that we were after all mistaken about it. The contradictions that we fall into in the course of inquiry are, as Frege explains, due to just such faulty conceptions.

[They are] created by treating as a concept something that was not a concept in the logical sense because it lacked a sharp boundary. In the search for a boundary line, the contradictions, as they emerged, brought to the attention of the searchers that the assumed boundary was still uncertain or blurred, or that it was not the one they had been searching for. So contradictions were indeed the driving force behind the search, but not contradictions in the concept; for these always carry with them a sharp boundary . . . The real driving force is the perception of a blurred boundary. (Frege 1885a, 134)

Getting clear on the sense of a concept word through such a process of "proof and refutation," inference and counter-inference, just is to come to grasp the concept in its pure form.

According to Frege, errors in mathematics are almost invariably due to a lack of clarity about the relevant mathematical concepts: "almost all errors made in inference . . . have their roots in the imperfections of the concepts" (Frege 1880, 34)—that is, in our conceptions, our understanding of the sense. (Concepts, as the *Bedeutung* of concept words, cannot have imperfections.) And we discover those imperfections in our understanding by reasoning on the basis of concepts insofar as we understand them, that is, grasp the senses through which they are disclosed. But such a process of reasoning *can* reveal imperfections "only . . . if the content is not just indicated but is constructed out of its constituents by means of the same logical signs as are used in the computation. In that case, the computation must quickly bring to light any flaw in the concept formation" (1880, 35). This is precisely what Frege's concept-script enables, the expression of the inferentially articulated contents of concepts in a way that enables rigorous deductive proof and thereby a means to discover the flaws in our understanding of those concepts, and to improve that understanding.

Because "the aim of proof is, in fact, not merely to place the truth of a proposition beyond all doubt, but also to afford us insight into the dependence of truths upon one another," "it is in the nature of mathematics always to prefer proof, where proof is possible" (Frege 1884, sec. 2). A proof "serves to reveal logical relations between truths. That is why we already find in Euclid proofs of truths that appear to stand in no need of proof because they are obvious without one" (Frege 1914, 204). Indeed, Frege thinks that it is just this that "constitutes the value of mathematical knowledge": "not so much what is known as how it is known, not so much its subject-matter as the degree to which it is intellectually perspicuous and affords insight into its logical interrelations" (Frege 1898, 157). But of course a proof must start somewhere; not everything can be proved. The task, then, is "to make the circle of unprovable *primitive truths* as small as possible," to discover some small collection of primitive truths from which all the others can be derived (Frege 1914, 204). And as Frege notes both in *Begriffsschrift*, section 13, and in "Logic in Mathematics," often there is some leeway here insofar as two different axiomatizations of some domain of inquiry may be equally acceptable: "the possibility of one system does not necessarily rule out the possibility of an alternative system, so that we may have a choice between different systems. So it is really only relative to a particular system that one can speak of something as an axiom" (Frege 1914, 206).

In Part I of the 1879 logic, Frege introduces all the primitive signs of his language, explains his fundamental notions, and sets out his one mode of inference. None of this, as he notes in the opening paragraph of Part II, can be expressed *in* his language because it forms the basis of all expression in the language. The elucidations of Part I belong, then, only to the antechamber of mathematics, the propaedeutics (Frege 1899, 36). They are necessary not to mathematics itself but in order to ensure that a reader has the same understanding of the basic notions of the written system of signs as its author, "and here one must of course always rely on being met half-way by an intelligent guess" (Frege 1899, 37).[8] In Part II the development of the system properly begins. Nine judgments are presented as the axioms that contain, as in a kernel, "all of the boundless number of laws that can be established" in logic (Frege 1879, sec. 13), and various theorems are derived from them using Frege's one mode of inference. We need to understand just what all this involves.

First, axioms, on Frege's view, are truths that are, or should be, immediately evident; they are truths about which we have no doubts, though, again, we can turn out to have

[8] Frege makes the point again in "Logic in Mathematics." When we are dealing with the primitive signs of the language "we have to depend on a meeting of minds, on others guessing what we have in mind" (Frege 1914, 207). It was just this meeting of minds that was altogether lacking in the case of Frege's *Begriffsschrift*.

been mistaken. Because we need explicitly to recognize the truth of Frege's axioms—because inferences can be drawn only from acknowledged truths—we need to grasp the thoughts those axioms express, and in order to do that we need to grasp the senses of Frege's primitive signs. Given, for example, what is expressed by the conditional stroke, as elucidated in Part I, and given what it is to judge, namely, to acknowledge the truth of a (true) thought, it follows that what is true is true on any condition you like because what is true is true unconditionally. If, in other words, one has some acknowledged truth, some judgment, then one may infer that judgment on some condition, that is, add a condition to it, any condition you like. Frege's first axiom formulates this valid rule of inference in the form of a judgment. In the same way one needs to grasp the senses of Frege's other primitive signs in order to recognize the truth of his other axioms. For the purposes of proof, all that is granted as known regarding the primitive signs is what is made explicit in Frege's nine axioms. The axioms codify the fundamental inference potentials that are contained in his various primitive signs. Because they do, various derived rules of inference involving those primitive signs can also be established.

Axioms, on Frege's view, are (that is, should be) truths that are immediately evident. Definitions such as those Frege presents in Part III of the 1879 logic are different insofar as they are not truths but instead stipulations. What a definition stipulates is that some newly introduced, hitherto meaningless sign has precisely the same meaning (*Bedeutung*) as some collection of primitive, and perhaps also already defined, signs. Frege does sometimes claim that the two also express the same sense (*Sinn*), that the newly introduced sign is merely an abbreviation for the complex sign used in its definition, but we have already noted that this cannot be right given that Frege also thinks that proofs of theorems from (fruitful) definitions can constitute real extensions of our knowledge. Definitions in Frege's system stipulate sameness of meaning (*Bedeutung*) but not also sameness in the sense (*Sinn*) expressed. Indeed, I have suggested, we can just *see* that this is so insofar as the definiendum is a *simple* sign without any internal articulation and the definiens is a *complex*, richly articulated sign on the basis of which to reason.

Now in the Aristotelian model as traditionally understood, definitions seem to have nothing whatever to do with the proof of theorems. There are the primitive and defined terms, on the one hand, and the primitive and derived judgments, on the other, and no indication is provided of any sort of relationship between the two. This is unsurprising given that in (say) Euclid's system definitions belong to the preamble or antechamber, not to the actual system of mathematics. As we have seen, it is not the definitions but instead diagrams that formulate content in a mathematically tractable way in Euclid's system. But proofs in contemporary mathematical practice *do* take explicitly formulated definitions as their starting points. (See, for example, any textbook of modern, abstract algebra.[9]) And Frege's system reflects this. As we

[9] Of course, in such textbooks no mathematical language within which to reason is employed. Proofs are only reported in natural language, together with some abbreviations, not displayed in a specially devised

saw in Chapter 7, in Part III of *Begriffsschrift* Frege introduces four definitions and proves on the basis of those definitions a series of theorems culminating in theorem 133 showing a logical relation among three of Frege's four defined concepts. As Frege shows by example, definitions can enable the discovery of logical relations among (defined) concepts by means of deductive proof.

Frege's practice suggests that the theorems that are derived from axioms of logic are very different from theorems that are derived from definitions. But the two sorts of derivations are not merely independent of one another, as primitive and defined concepts appear to be independent of primitive and derived theorems in the classical model. As we saw in Chapter 7, in Frege's system, axioms and the theorems that are derived from them provide inference licenses that are needed for proofs that take definitions as their starting points. And this works precisely because the signs that appear in the axioms and theorems are used also in the formulation of the definitions: "the content . . . is constructed out of its constituents by means of the same logical signs as are used in the computation" (Frege 1880, 35). Now in Frege's *Begriffsschrift* example, even the "constituents" are strictly logical; *all* of the primitive notions in the system of the 1879 logic belong to logic. But even were the language to be enriched with the addition of primitive signs of arithmetic, the same would be true. The new arithmetical primitives would require the addition of new axioms codifying the fundamental inferential significance of those primitives. But they would also enable the formulation of definitions of various mathematical concepts, concepts such as that of continuity, of prime number, and of being a common multiple, all of which and more are formulated in Frege's concept-script together with some signs from arithmetic in Frege's early essay "Boole's Logical Calculus and the Concept-Script." Proofs of significant theorems in the new system would begin with such definitions and proceed in accordance with the rules codified in the axioms of the system together with the theorems that are derived from those axioms. There would, then, remain a fundamental division between, on the one hand, axioms and theorems derived from them, all of which function in the system overall as rules governing inference, and on the other, definitions and theorems derived from those definitions. Only the latter sorts of theorems, theorems proven on the basis of definitions, can (we will see) constitute real extensions of our knowledge.

system of written signs within which to reason. (We saw in section 6.4 some examples of the form of wording in such reports.) And definitions similarly are formulated in natural language. The student is only *told* what the defined concepts mean, and how to reason from them to various theorems. One *can show* those meanings and the course of reasoning that takes one from the definitions to the theorems, but only in a formula language such as Frege's *Begriffsschrift*. And because the student is only told how to think, not shown the thinking, it can take considerable mathematical ability to reconstruct what is going on in contemporary mathematical practice, much more than would be required if the reasoning were displayed, put before one's eyes as it is in a calculation in Arabic numeration and in elementary algebra. And if so then Frege's system of signs has not only the philosophical significance we have claimed for it here but also real pedagogical significance, say, for university-level mathematics.

According to the model as traditionally understood a science comprises concepts, both primitive and defined, and judgments, both primitive and derived. No indication is given of the relationship, if any, between defined concepts and derived judgments. Frege's practice, which (though less explicitly) is essentially that of mathematicians beginning in the nineteenth century and continuing still today, exhibits a much more complex structure. In Frege's practice, there are, first, axioms setting out the fundamental inferential significance of the primitive signs, as well as various theorems that follow from those axioms by a recognized rule of inference. Then there are the definitions of concepts, which are stipulations that introduce some new simple sign and assign it a meaning (*Bedeutung*) that is given in the definiens by way of an inferentially articulated sense (*Sinn*). These definitions are, furthermore, formulated using just the same signs as are employed in the formulation of the axioms. And finally there are the theorems that follow from the definitions by means of the axioms and theorems derived from those axioms. A proof of such a theorem on the basis of definitions, and according to the axiomatized rules, is (or at least can be made to be) completely rigorous and gap-free. And because it is (or can be made to be) completely rigorous and gap-free, the proof makes manifest on what the theorem depends and by what means it is justified. In this case, just as Frege remarks in the introduction to *Grundgesetze*,

> if anyone should find anything defective, he must be able to state precisely where, according to him, the error lies: in the Basic Laws, in the Definitions, in the Rules, or in the application of the rules at a definite point. If we find everything in order, then we have accurate knowledge of the grounds upon which each individual theorem is based. (Frege 1893, 3)

Because everything is made maximally explicit in reasoning in Frege's system—the starting points, the rules governing inferences, and each individual inference from the starting points to the desired conclusion—errors are more easily detected, and where no errors are found, although this is no *guarantee* that problems will not later come to light, one has very reasonable assurance that the theorem is true and good reason to think one has discovered the nature of its grounds, whether in logic alone or in the basic laws of some special science. It is in just this that the rationality of the endeavor consists. Because it makes everything maximally explicit and hence available to critically reflective scrutiny and criticism, the method gives one *cognitive control* over the domain of inquiry. And although the method does not guarantee that mistakes will not be made, does not guarantee that what seems to be clear and distinct really is clear and distinct, it insures as far as possible that such errors as there are will, sooner or later, come to light and be corrected. The practice is both fruitful and maximally robust. It is also non-foundationalist. Not only is it, as mathematics has always been, self-consciously rational, it is *purely* rational, by reason alone.

On the Aristotelian model as traditionally understood, knowledge of the basic concepts and judgments of a science is somehow immediate and certain. Hence, it can seem that a constitutive feature of mathematical practice is the search for indubitable foundations; and as a result, it can seem, giving up the foundationalist enterprise in mathematics is tantamount to giving up the model.[10] Frege shows that this is just not so. One can be a thoroughgoing fallibilist as concerns our knowledge of the truths of mathematics (and logic), and nevertheless adhere to the model as the exemplar of scientific rationality. Indeed, we have seen, *really* to take seriously the fact that there is no certainty in mathematics, that one may after all be mistaken on *any* point, *requires* that one adhere to the model.

Frege rejects the foundationalism of the Aristotelian model as traditionally understood. For him, as for Bolzano and Riemann, mathematics, although a priori, that is, non-empirical, is nevertheless an experimental science whose foundations are not given but instead must be discovered by a process of "proof and refutation," that is, self-correction. Axiomatizations and explicit definitions are, on this understanding, not so much the end product of science as a means, a vehicle of discovery. (We saw in section 5.2 that Corry holds that this is Hilbert's view as well.) If a contradiction is derived then one knows that something is wrong, either with one's axioms or with one's definitions, that there is something one has not adequately understood, that one needs to make some corrections. And this requires that Frege modify the classical model in another way as well. Where the model has concepts, primitive and defined, and judgments, primitive and derived, Frege has instead: (1) axioms and theorems of logic, (2) definitions formulated using the same signs as are used in the axioms and theorems of logic, and (3) theorems derived from definitions by means of the rules provided by the axioms and theorems of logic. These three essentially different classes of judgments play in Frege's system just the role that in Kant's thinking is played by the three forms of judgment under the title of relation in his table of the forms of judgment, namely, categorical, hypothetical, and disjunctive judgments. In both cases they are the means whereby one makes explicit one's understanding and in such a way that confusions can come to light in the form of contradictions. (The differences between them are a reflection of differences between natural and mathematical language that will be explicated below, in section 8.4.)

In Frege's modified model, theorems derived on the basis of (fruitful) definitions are essentially different from theorems derived from axioms alone and must be distinguished from them. This, I have suggested, is something radically new, something that is characteristic in particular of mathematical practice as it emerged over the course of the nineteenth century and continues today. Although they are not as rigorous in their practice as Frege is and they do not use a formula language of the sort Frege developed (although they could), what mathematicians do is to define concepts and derive theorems based on their definitions, showing thereby various logical relations that obtain among the concepts so defined. What Frege helps us to see is in what the rationality of this practice consists. As he shows, the Aristotelian

[10] Both Beth (1968, ch. 2) and Rav (1999) make this assumption.

model of science, updated to reflect developments within mathematics, provides a viable and compelling image of scientific rationality by showing, if only in the broadest outline, how it is that we achieve, and maintain, cognitive control in our mathematical investigations.

8.3 The Nature of Ampliative Deductive Proof

Frege claims (1884, sec. 91) that his proof of theorem 133 in *Begriffsschrift* is ampliative, an extension of our knowledge, despite being analytic in Kant's sense, that is, strictly deductive. In section 88 of *Grundlagen* he further suggests that ampliative proofs involve what he describes as fruitful definitions that—by contrast with definitions that merely list (conjunctions and disjunctions of) characteristics—draw new boundary lines.

> If we represent the concepts (or their extensions) by figures or areas in a plane, then the concept defined by a simple list of characteristics corresponds to the area common to all the areas representing the defining characteristics; it is enclosed by segments of their boundary lines. With a definition like this, therefore, what we do—in terms of our illustration—is to use the lines already given in a new way for the purpose of demarcating an area. [In a footnote: Similarly, if the characteristics are joined by "or".] Nothing essentially new, however, emerges in the process. But the more fruitful type of definition is a matter of drawing boundary lines that were not previously given at all. What we shall be able to infer from it, cannot be inspected in advance; here, we are not simply taking out of the box again what we have just put into it. The conclusions we draw from it extend our knowledge, and ought therefore, on Kant's view, to be regarded as synthetic; and yet they can be proved by purely logical means and are thus analytic. The truth is that they are contained in the definitions, but as plants are contained in their seeds, not as beams are contained in a house. (Frege 1884, sec. 88)

In the long Boole essay (Frege 1880, 34–5), the same imagery of drawing new boundary lines is used to distinguish what Frege thinks of as fruitful definitions, and Frege points to his "representation of generality" as what, in particular, makes possible "a true concept formation—one that didn't use already existing boundary lines" (1880, 35). Again Frege indicates that only from such definitions can theorems be derived that extend our knowledge.

> It is the fact that attention is principally given to this sort of formation of new concepts from old ones [that is, the sort that does not draw new boundary lines but only traces over parts of old], while other more fruitful ones are neglected which is surely responsible for the impression one easily gets in logic that for all our to-ing and fro-ing we never really leave the same spot. (1880, 34)

In both this essay and in *Grundlagen* Frege cites the definition of continuity as an example of a definition that is fruitful in his sense. In the long Boole essay he cites as well the definition of a limit and his own definition of following in a sequence. All these definitions involve Frege's sign for generality, his concavity, in their expression in *Begriffsschrift*.[11] Nowhere does Frege suggest that his definitions of being hereditary in a sequence and being a single-valued function are likewise fruitful, despite the fact that they too involve the concavity in their expression. As we will see, we need to distinguish more carefully than Frege does between different cases, between different levels of fruitfulness.

In Frege's imagery in section 88 of *Grundlagen*, the conclusion of a merely explicative proof is contained in the starting points of the proof as beams are contained in a house. The conclusion is, in other words, already there in the starting points; it is contained in them needing only to be brought out (as beams might be brought out by dismantling a house). In a merely explicative proof the conclusion is, as we can say, implicit in the starting points; all the proof does is to make what is otherwise only implicit explicit. In an ampliative proof, Frege suggests, the conclusion is instead contained in the starting points "as plants are contained in their seeds." As a seed has the potential to grown into a plant so the starting points of an ampliative proof have the potential to yield a conclusion that extends our knowledge. The conclusion of such a proof actualizes that potential as a plant that grows from a seed actualizes the potential of that seed. Whereas in a merely explicative proof what is implicit is made explicit, in an ampliative proof what is potential is made actual. We need to understand what precisely is the difference between these two cases.

Frege's remarks about ampliative proof suggest that only proofs from definitions, and in particular from what he describes as fruitful definitions, definitions that (in his imagery) draw new boundary lines, can extend our knowledge. It follows directly that none of Frege's proofs of theorems from axioms in Part II of the 1879 logic can extend our knowledge. Such proofs only make explicit something implicit in those axioms. If Frege is right, and we will soon see that he is, then it is not primarily the *form* of reasoning that makes the difference between ampliative and merely explicative proof, but instead the starting point, whether axioms or definitions—though, as we will also see, this can affect in turn the precise nature of the reasoning involved.

We have seen that any derivation in Frege's concept-script, whether from axioms or from definitions, requires us to regard a formula now this way and now that. Conditions that are at one point seen as conditions *on* some judgment are at another instead regarded as *part of* the conditioned judgment (on some conditions). Any derivation also involves the construction of various formulae to take us from something we have, say, axioms 1 and 2, to something we want such as theorem 5. Both in cases in which the starting point is a collection of axioms and in cases in which the starting point is instead definitions, we can distinguish theorems that are merely a means to some end, as theorems 3 and 4 are, from theorems that are of some independent interest, as theorem 5 is. As the derivation of theorem 5 also reveals,

[11] If a definition is a stipulation regarding some newly introduced sign then of Frege's three examples only that of following in a sequence is strictly speaking a definition. In the long Boole essay (1880, 24–5) Frege sets out only the definiens of the concepts of continuity and of a limit.

both proofs from axioms and proofs from definitions can require a kind of experimentation to determine not only what rule it is useful to apply but also what it is useful to add as a condition. Axiom 1, we know, licenses adding a condition to a judgment, any condition you like. But one might need to experiment a little to determine that one should add *a*-on-condition-that-*b* in particular to axiom 2 on the way to theorem 5; and similarly, at various points in the derivation of theorem 133 (for instance, in the derivation of 111 from 108 using the rule in theorem 25), one needs to add one condition in particular. Hence, in both cases, both in reasoning from axioms and in reasoning from definitions, the reasoning seems to be, at least in some cases, theorematic rather than corollarial in Peirce's sense.[12] Furthermore, although most of the derivations in Part II do not, some of those derivations do involve inferences that join content from two axioms just as Frege's proof of 133 involves inferences that join content from two definitions. In the derivation of theorem 33 from axioms 31 and 28, for example, Frege constructs theorem 32, on the basis of axiom 31, to govern the passage from 28 to 33, but it is clear that we could instead take theorem 7, used in that construction, directly to license a two-premise inference from 31 and 28 to 33, one that joins content from the two axioms. And, finally, axioms can (of course) provide inference licenses. So even the fact that the proof of theorem 133 involves using an inference license derived from a definition does not seem in any essential way to distinguish Frege's proof of theorem 133 in Part III from the derivations of Part II. The *only* principled difference between the two collections of derivations seems to be that the derivations in Part II are from axioms and those in Part III are from definitions. We need to understand the difference this difference makes.

In Part II of the 1879 logic Frege derives various theorems he will need in his proof, in Part III, of theorem 133. These theorems, and the axioms from which they are derived, function in the latter proof, we have seen, as rules licensing the linear and joining inferences that take us from Frege's definitions to his theorem. And as Frege notes, the theorems that are derived from the axioms in Part II were chosen precisely because and insofar as they are needed in the proof of theorem 133 of Part III: "Apart from a few formulae introduced to cater for Aristotelian modes of inference, I only assumed such as appeared necessary for the proof in question," that is, for the proof of theorem 133 (Frege 1880, 38). Although the axioms and theorems of Part II are truths on Frege's view, they have, in other words, little intrinsic interest; they are valuable not in themselves but instead for what they enable one to prove on the basis of defined concepts. Independent of a proof such as that of theorem 133, one would have no reason to derive this rather than that theorem of logic, no reason to start, or to stop, with any particular axioms or theorems of logic. The interest of the "sentences of pure logic" that we find in Part II of Frege's *Begriffsschrift* lies in "the fact

[12] See Shin (1997) for discussion of Peirce's distinction and its relationship to Kant's distinction between analytic, merely explicative, and synthetic, ampliative, judgments.

that they were adequate for the task" that is undertaken in Part III (Frege 1880, 38). An axiomatization of the rules of logic is valuable insofar as it contains, if only implicitly, "all of the boundless number of laws [of logic] that can be established" (Frege 1879, sec. 13). But neither the axiomatization, nor the theorems that are derived solely on the basis of such axioms by themselves constitute a real extension of our knowledge of the sort that is achieved by a proof from defined concepts. And this is so at least in part because only a derivation based on definitions of concepts can establish that particular (defined) concepts—such as that of following in a sequence or belonging to a sequence—have various logical properties, for example, transitivity, and stand in various relations, such as subordination. There are a number of aspects to this.

In the case of a derivation from axioms, whether a very simple derivation such as that of theorem 5 or one that is quite involved, say, that of theorem 48 or that of theorem 51, there is a clear sense in which the conclusion is contained implicitly already in the starting points, the axioms, needing only to be made explicit. In this case, one is merely moving things around and combining and recombining them according to rules. And although the two-dimensionality of the notation enables one to regard a given formula now this way and now that, these various ways of regarding a formula do not reveal any subject matter for logic because any way at all that it is possible to regard the formula is as good as any other. It is *definitions* that introduce a subject matter; and much as they do in Euclid, definitions in Frege's system enable one to discern, between the primitive parts, on the one hand, and whole formulae, on the other, significant parts of those formulae that are built up out of the primitive signs. A science properly speaking thus involves more than merely primitive and derived truths about various basic notions, such as, in Euclid, points, lines, angles, and areas, or in Frege, negation, the relation codified in the conditional stroke, and the second-level property designated by his concavity. A science needs a *subject matter*; it involves concepts and judgments that concern a certain domain of being(s), and it is definitions of concepts by appeal to those same primitives that provide this subject matter. But again, in Euclid's system the definitions belong only to the propaedeutic, the antechamber; it is not in the definition but in the diagram that the contents of concepts are formulated in a mathematically tractable way. In Frege's system the definitions themselves formulated in *Begriffsschrift* are the starting point.

We have seen that an axiom of logic is a judgment, a truth. It is, or at least should be, immediately evident (*einleuchtend*), but it is nonetheless a substantive truth that does not go without saying. A definition, we have seen, is not a judgment; it is a stipulation. Such a stipulation does immediately give rise to an identity judgment, but this judgment is one that is, in light of the stipulation, utterly trivial. It is not merely immediately evident (*einleuchtend*) as an axiom is, or should be; it is self-evident (*selbstverständlich*). It goes without saying. But as we have also seen, although they are trivial in themselves, the judgments that derive from definitions can enable one to discover, by way of proof, logical bonds among the concepts that are designated by the defined signs. In the case of a demonstration on the basis of definitions of concepts, we are not merely joining content in a thought that can be variously analyzed, as is the case in the derivation of a theorem of logic from axioms; we are discovering logical bonds among the *particular* concepts that are designated by the defined signs. If in Frege's proof of theorem 133 we were to replace each definiendum by its definiens, then we *would* have a mere theorem of logic, one subject to many, many different analyses. The theorem would *not* be about the concepts of following in a sequence, belonging to a sequence, and being a single-valued function at all. It would not be a theorem in the theory of sequences—any more than Euler's Theorem would be a theorem about the exponential and trigonometric functions if the relevant signs were replaced by the various infinite sequences with which they are (at the level of *Bedeutung*) identified. It is *only* if we have definitions of concepts that we can forge logical bonds *among those very concepts*. And, we will see, only in that case can a proof extend our knowledge of those concepts by revealing their logical relations one to another.

A definition involves both a complex sign for some concept, the definiens, and also a simple sign for that same concept, the definiendum, and these two signs, simple and complex, although they designate the same concept, express different senses. Thus the mathematical concepts of concern to Frege are, as we argued the figures of Euclidean geometry are, intelligible unities. They are whole of parts that are nonetheless not reducible to their parts. What a proof involving definitions shows is that both these aspects of concepts are essential, both the fact that the concept is one, an irreducible whole, and that it nonetheless has parts. The simple signs, the definienda, are necessary if one is to prove something about the defined concepts with which one begins; one needs the definiendum in order to prove something about the *particular* concept that is designated by that simple sign. (Because the sign is simple, has no parts, there is clearly no way to think about it other than as designating the relevant concept.) But one needs also the definiens if the needed logical bonds are to be forged; without the definientia it would be impossible to show that one's various defined concepts stand in logical relations one to another. Only a proof that begins with definitions can show that some logical relation obtains between particular concepts of interest.

But it is equally true that only within a proof are definitions of any interest. Because definitions are stipulations, the identity judgments that correspond to them are *completely* trivial. Only definitions and proofs working *together* can yield something new. Only *within* a proof can the peculiar power that resides in definitions by their nature as stipulations regarding simple and complex signs be harnessed in ways that can extend our knowledge. In a slogan: proofs without definitions are empty, merely the aimless manipulation of signs according to rules; and definitions without proofs are, if not blind, then dumb. Only a proof can actualize the potential of definitions to speak to one another, to pool their resources so as to realize something new.

But, again, not all definitions are fruitful in the sense of drawing new boundary lines, in particular, definitions that merely list a conjunction or disjunction of marks are not fruitful. Frege's own definition of belonging, because it is merely disjunctive, is not a fruitful definition in his sense. The concept of belonging is not an intelligible unity but only an accidental unity; it is not an irreducible whole of parts but instead a whole that can be reduced to its parts: either this or that. It is convenient to have a sign for belonging just as it is convenient to have a sign for less-than-or-equal-to. But nothing new is thereby introduced. Again, no new lines are drawn. Definitions that draw new lines do introduce something essentially new precisely because they drawn new lines, which is to say that they cannot be reduced to their parts. What we need to see is that in all such cases the new lines are drawn precisely because and insofar as the concept in question contains an inference license.

Consider, first, the concept of a dispositional property such as that of solubility (in water). To say of some stuff that it is soluble is to say that if it were to be put in water then it would dissolve, that one can *infer from* the fact that the stuff is put in water (assuming that such a fact obtains) *that* it will dissolve. Such a dispositional property clearly cannot be reduced to its parts. In other cases, and in particular in the case of mathematical concepts, the question whether a concept contains an inference license or is instead reducible to its parts is harder to answer. We saw this already in the case of the concept *prime*. If one thinks of its contents quantificationally, in terms of what is the case if the concept applies—namely, that no other number (greater than one) divides it without remainder—then it appears not to form an intelligible unity, an irreducible whole of parts. Instead it would seem to be a kind of accidental unity, one that is reducible to (a conjunction of) its parts: that this number does not divide it, and that that number does not, and so on. If we instead think of its contents inferentially, in terms of what follows (given that a certain condition is met) if the concept applies—namely, that if one is provided another number (greater than one) then it can be inferred that that number does not divide it without remainder—then it must be understood as an intelligible unity. So understood, the concept *prime* is not reducible to its parts; although it has parts, because these parts form an inference license (expressed in Frege's concept-script using the conditional stroke and his concavity), the whole is no more reducible to its parts than the whole that is the concept of solubility is. And this seems intuitively to be right. That a given number is prime is not an accidental fact about it, the chance convergence of a collection of singular facts about this number and that number. It is necessary that those facts hold and that necessity is explained by reference to the inference license that, on the Fregean conception of the concept *prime*, it contains. Concepts that contain inference licenses draw new lines in just this way and for just this reason: they are intelligible unities, wholes that have parts but are not reducible to their parts. Because inference licenses are inherently conditional and constitutively general, the contents of all such concepts will be expressed in *Begriffsschrift* using Frege's conditional stroke and the concavity.

Any mathematical concept that contains an inference license will have its content expressed in *Begriffsschrift* using the concavity and the conditional stroke for the simple reason that an inference license is inherently conditional (from something as ground to something as conclusion) and also essentially general, something that can be applied in other cases. But as Frege's own definitions indicate there are two interestingly different cases. In the case of being hereditary in a sequence and also in that of being a single-valued function, what is defined is a second-level concept: both the concept *hereditary in the f-sequence* and the concept *single valued* take first level concepts as arguments to give truth-values as values. It is not objects that have the property of being hereditary in Frege's sense but instead concepts; and it is not objects but instead functions that are single valued. And this is directly reflected in the definitions of these concepts, in particular, in the use of the concavity with lower case German letters. The concavity with lower case German letters is used to form expressions for second-level concepts, from the simplest, *universally applicable* formed from the concavity taken alone, through *subordinate*, which involves also the conditional stroke, to the even more complex concepts of being hereditary and being single valued. The same is true of the mathematical concept of continuity. The concept of continuity is a second-level concept the definition of which essentially involves the concavity with lower case German letters.[13]

The notion of following in a sequence is different insofar as it is a *first-level* relation; assuming that the function is fixed, it takes two *objects* as arguments to yield a truth-value as value. It is in this regard like a concept such as that of being soluble, of dissolving when immersed in water, the definition of which would be expressed in Frege's system of signs using the conditional stroke and Frege's Latin italic letters (which also, in their way, are signs for the expression of generality): to say of some object that it is soluble is to issue an inference license, that if that object is immersed in water then (it may be inferred) it will dissolve. But the relation of following, unlike the property of being soluble, cannot be defined using a simple conditional together with Frege's Latin italic letters. To understand why it cannot, we need to compare it not with a concept such as that of solubility but instead with, for instance, the logical relation of identity, which is also first level insofar as it takes objects as arguments to give truth-values as values. In both the case of identity and in that of following in a sequence, and by contrast with the case of being soluble, the content is articulated in an inference license that concerns not what follows if an *object* falling under the concept meets some condition (say, it dissolves if put in water) but instead what follows if some *concept* meets some condition. That is, a *top-down* specification is given, one that is marked in Frege's definition of following by the use of the concavity with an *upper case* German letter. To set out the content of

[13] See my (2005), especially section 3.3, for further discussion of the role of the concavity in the formation of higher-level concept words.

the first-level relation of following, Frege invokes an inference license concerning what may be inferred about *concepts* that satisfy some condition.

In the case of identity (that is, logical equivalence—they are, as Frege sees, one and the same notion in logic), because it is a primitive logical notion in Frege's system, the inferential significance of this notion is not given in a definition but instead in an axiom. What the axiom, numbered 52 in *Begriffsschrift*, tells us is that it may be inferred given that *x* is identical to *y* that any property that belongs to *x* also must belong to *y*. That is, to say that *x* is identical to *y* is to issue an inference license to the effect that it may be inferred from the fact that *x* has some property that *y* likewise has that property, for any property you like. Although identity takes *objects* as arguments, and hence is a first-level relation, what an ascription of identity licenses is an inference about a *concept*, provided that a certain condition is met. Similarly, in the case of following in a sequence, to ascribe that relation to a pair of objects is to issue an inference license to the effect that if some lower-level property has a certain higher-level property then it may be inferred that that lower-level property has some other higher-level property as well. Because in the case of following we are dealing with a definition rather than an axiom, the definiens makes this explicit in the use of the concavity with an upper case German letter.

To say that *y* follows *x* in the *f*-sequence, to ascribe that relation to a pair of objects *x* and *y*, is to issue an inference license according to which, from the fact that some property of *x* is heritable, it may be inferred that if that property belongs to any result of an application of *f* to *x* then *y* must also have that property; alternatively, from the fact that some heritable property of *x* belongs to anything that is the result of an application of *f* to *x*, it may be inferred that *y* has that property. Again, just as with identities, the license applies not in the case in which some *object* satisfies some condition but instead in the case in which some *concept* satisfies a condition, that is, falls under another, higher-level concept. (In the case of identity, the higher-level property is that of being a property of the object named in the original identity. Being a property of a certain object is a second-level concept because it takes concepts as arguments to give truth-values as values.) To *use* the license that is contained in the definition of such a concept in making an inference, then, one needs to have already a judgment regarding some particular property that, for the first reading, it is heritable, and for the second reading, it is heritable and belongs to anything that is the result of an application of *f* to *x*. Frege does not use this rule (in either form) in his proof, and it is easy to understand why: there are no objects to which the relation of following might be ascribed thereby issuing the relevant inference license. One does not, in logic (or in mathematics), ascribe first-level properties and relations to objects but only higher-level properties and relations to lower-level concepts. What Frege does is to derive, in one case from the definition as a whole and in another from a theorem derived from that definition together with the definition of being hereditary, inference licenses regarding being hereditary, what may be inferred not if one *object* follows another object but if some *concept* has the higher-level property of being hereditary. Having shown that some property is hereditary, the relevant inference is drawn, in one case directly to replace the condition that $f(y,x)$ with the weaker condition that x follows y in the f-sequence (namely, in the inference from 131 to 133), and in the other to derive the theorem (110) that licenses such a replacement (in the inference from 122 to 124). In these cases, and only in these cases, it is a definition that, directly or indirectly, supplies the rule (bridge) licensing the inference, the rule according to which to reason as such a rule contrasts with a premise from which to reason. In every other inferential step of the proof of theorem 133, the rule is (or would have been had Frege derived the needed theorem) instead some axiom or theorem of Part II.[14]

In section 7.1 we distinguished between two forms of reasoning that seem superficially to have the same form, namely, detachment, insofar as in the one case the inference really is by detachment—that is, it is of the form *a*-on-condition-that-*b*, but *b*; therefore, *a*—and in the other it is governed by a rule that although it can be applied in the given case is essentially general, available to be applied also in other cases. Frege's rules of inference, as codified in his axioms and derived theorems in Part II, are all essentially general in their use in Part III. Although Frege in his practice employs not the rule itself but instead a particular instance of the rule, which effectively reduces the inference to detachment, we can, as we in fact did, instead treat those axioms and theorems as rules properly speaking, that is, as inference licenses that warrant the particular inferences that are made. (It is not logic that demands taking the route Frege takes but instead his logicism. For the purposes of logic, it is perfectly legitimate, when one turns to proofs on the basis of definitions, to treat the claims of Part II as rules of inference, as principles according to which to reason, rather than as claims from which to reason.) And this same point, we saw, applies also in the case of two premise inferences by some form of hypothetical syllogism. In some cases one has a straightforward application of the rule of hypothetical syllogism: if *a*-on-condition-that-*b* and *b*-on-condition-that-*c* then it may be inferred that *a*-on-condition-that-*c*. But in other cases, the premise that *a*-on-condition-that-*b* is instead something essentially general, a rule or inference license that can be applied also in other cases. Joins that are made in accordance with our general strategy are instances of the first, straightforward application of hypothetical syllogism. The strategy is to modify the identities corresponding to definitions in various ways until they share content as required by the rule of hypothetical syllogism. What superficially appear similarly to be joins by hypothetical syllogism but involve a theorem derived from the definition of following in a sequence are crucially different insofar as in these cases the major takes the form of a rule that can be applied also in other cases. It is just these two essentially different sorts of cases that explains, we will see, why it is that Frege singles out his definition of following in particular as fruitful

[14] Trivially, the definition of being hereditary licenses introducing the defined sign for being hereditary in cases in which the condition on being hereditary is satisfied. I ignore this case.

despite the fact that both being hereditary and being single valued also contain inference licenses. All three definitions involve Frege's sign for generality and hence draw new boundary lines; that is, they are fruitful in Frege's sense. But there are also essential differences between the definition of following, on the one hand, and the definitions of being hereditary and being single valued, on the other, differences that are reflected in how they can be employed in proof.

We have seen that it is important to distinguish between three levels of understanding one might have in relation to Frege's proof of theorem 133. At the first, most basic level one can discern only the rules that are applied at each step; each step takes the form of a detachment and is merely truth functional. From this perspective there is no significant difference between the derivations of Part II from axioms and the derivations of Part III from definitions. At the second, more advanced level of understanding one can discern a general strategy in Frege's proof of theorem 133, one that involves distinguishing between linear, essentially one-premise, inferences and two-premise joining inferences that combine content from two or more formulae. This strategy, we saw, fully explains the proof that following in a sequence is hereditary. It also, though we did not show this, sufficiently explains the proof that belonging to a sequence is hereditary. But, as we saw, this strategy does not fully explain the proof of theorem 133. Some steps of that proof do not use hypothetical syllogism to join two chains but instead use the definition of following in a sequence to form an inference license regarding what follows from something's being hereditary in a sequence. Unlike the joins that are explicable by appeal to our general proof strategy—a strategy that constitutively involves definitions, and in particular definitions that are fruitful in Frege's sense, that contain inference licenses in their definiens, but makes no essential appeal to any *specific* definitions—the inferential steps that involve an inference license furnished by the definition of following in a sequence are possible at all only because that definition contains an inference license in the definiens that concerns not objects but instead concepts. (Hence, its formulation in Frege's *Begriffsschrift* uses upper case German letters as contrasted with the lower case letters that are used in the definitions of being hereditary and being single valued.)

Any definition that involves both the concavity (with widest scope) and the conditional stroke in its expression contains an inference license. And any such definition, by contrast with one that is disjunctive such as that of belonging, is amenable to our general proof strategy.[15] But again, the definition of following in a sequence is used differently insofar as it provides not a ground but instead a bridge,

[15] This is not literally correct. The proof strategy can be appealed to even in the case of proofs of theorems that begin with merely truth-functional definitions. We see this, for example, in the proof of theorem 114 on the basis of two occurrences of the definitions of belonging. Nonetheless, althought it superficially appears that logical bonds have been forged among concepts in this case, in fact, in this case, because the definiens is merely disjunctive and hence only an accidental unity, there are no concepts available as the relata of those bonds. The theorem that is proven is true, but only trivially so given the

that is, the needed inference license at certain crucial steps in the proof. These steps that involve inference licenses derived from the definition of following in a sequence thus distinguish the proof of theorem 133 even from, say, the proof that following in a sequence is hereditary in that sequence, or indeed any proof that proceeds in accordance with the general proof strategy. Should we then say that it is just those steps that enable an extension of our knowledge in the one case, that of theorem 133, but not in the other, that of, for example, showing that following in a sequence is hereditary in that sequence, which involves only the general proof strategy?

In the course of a proof such as that of theorem 133, the simple, defined signs are needed if what is to be established is to be unequivocally about the concepts of interest, but their definitions are needed if anything about those concepts is to be established. Here, as already noted, we have something new that is made possible *only* in light of definitions, that is, judgments that involve, on the one hand, a simple sign, the definiendum, and on the other, a complex sign, the definiens, that exhibits the inferentially articulated content of the relevant concept, the sense through which we know it. In this case, and only in this case, content that is derived ultimately from two different definitions, if it can be brought to be identical in the two cases, can be used to reveal a logical bond between the concepts designated by the defined signs. That it is these concepts and no others that are joined is determined by the defined signs; that they *can* be joined is made possible by the fact that the contents of those signs are also given in various complex expressions, expressions that are variously analyzable. But we know also that only *fruitful* definitions—definitions that draw new lines (that are wholes of parts that are not reducible to their parts), the expression of which require employing the concavity—can be the basis for ampliative deductive proof. Not only the definition of following in a sequence but also Frege's definitions of being hereditary in a sequence and of being a single-valued function (though not Frege's definition of belonging) would seem, then, to be fruitful in Frege's sense. As marked by the presence of Frege's concavity in their expression, the latter two definitions, like that of following in a sequence, contain inference licenses about what may be inferred given that certain conditions are satisfied. These licenses are not used as such in the course of the proof of theorem 133; nevertheless, the fact that the definitions have this feature is not irrelevant to the proof. Definitions that contain inference licenses, and so involve the concavity together with the conditional stroke, are alone of a form suitable for use in reasoning according to our general strategy of linear and joining inferences. It is because, in particular, the definitions of being hereditary in a sequence and of following in a sequence take the form of generalized conditionals that our general *Begriffsschrift* strategy of linear and joining inferences can be applied in this case to show that following in a sequence is hereditary in that sequence. No definition that was not of that form, that did not contain an inference

disjunctive definition of belonging. Like the derivations in Part II, the proof of theorem 144 is merely explicative.

license, could be used to join concepts according to that strategy. (Because a conditional follows from any definition, no matter whether it contains an inference license or is merely truth-functional as the definition of belonging to a sequence is, any definition can figure in at least one join. In this trivial sense, any definition at all can figure in a proof involving our general strategy.[16]) Our general proof strategy for *Begriffsschrift* works only because and insofar as the definientia of one's definitions take the form of an inference license.

But again, although any proof from definitions according to our general proof strategy can reveal something interesting about the defined concepts, Frege indicates that his definition of following in a sequence is unlike the definitions of being hereditary in a sequence and of being a single valued function, that *only* it is fruitful. And it is theorem 133 that he says is ampliative, not any of the subsidiary theorems that are proven along the way. But if the proof of theorem 133 is ampliative in a way that the proof that, say, following in a sequence is hereditary in a sequence is not ampliative then that can only be because in the latter proof, although essential use is made of the fact that the definientia contain inference licenses, those inference licenses are never used *in* the proof to license any inferences. Excepting the definition of belonging to a sequence, Frege's definitions in Part III do contain inference licenses, as we saw. But, again, only in the case of following in a sequence is such a license used as a license *in the proof*. And even in this case it is not the license that the definiens contains that is used but instead other licenses that are derived from the definition as a whole.

We saw that the definition of being hereditary in a sequence contains an inference license, and because it does, to ascribe that property to some concept is to issue an inference license regarding that concept. To say, for example, that being a positive whole number is hereditary in the successor sequence is to issue an inference license regarding being a positive whole number to the effect that if something is the successor of a positive whole number then it may be inferred that that thing is also a positive whole number—alternatively, that if something is a positive whole number then it may be inferred that any of its successors must also be positive whole numbers. The concept *prime* should similarly be construed as containing an inference license according to Frege: to say that a number N is prime is to issue an inference license to the effect that if some number x (greater than one) is not equal to N then it may be inferred that x will not divide N without remainder.[17] In both cases,

[16] And of course we can conceive even the definition of belonging inferentially insofar as it involves the conditional stroke. In Frege's proof of theorem 133, it is proven that belonging is hereditary and this fact, suitably modified using the definition of following in a sequence as bridge, enables us to weaken one of the conditions on theorem 120 as needed. The inference here is a straightforward join by hypothetical syllogism.

[17] Frege sets out the content of this inference license in the long Boole essay (1880, 23). There the concept word is used in the context of a whole proposition expressing the thought that the number thirteen is prime. To get a concept word for the concept prime one merely needs to replace the numeral '13' in all its occurrences with a sign to indicate the argument place, for instance, 'ξ' as Frege uses it.

in order actually to draw the relevant inference, to use the license as the license it is, one needs something on the basis of which to reason. The relevant condition must be satisfied, in the one case by something that is a successor of a positive whole number, and in the other by some number greater than one and not equal to N. Nothing like this happens in Frege's proof of theorem 133. Although he shows that following in a sequence, belonging to a sequence, and what we have called connectedness are all of them hereditary in that sequence, thereby issuing an inference license in each case, no instance satisfying the condition of that license is available as the premise from which to draw an inference according to that license. (Nor could it be: the instances would have to be objects because what the concepts of following, belonging, and connectedness take as arguments are objects, assuming one has a determinate function, and again, proofs in mathematics do not descend to the level of objects but invariably stay at the level of concepts.) What *does* happen in the proof is that something is inferred about the *concept* that has been shown to be hereditary, and that inference, we have seen, essentially involves the definition of following in a sequence. The rule that provides the bridge that takes one from the claim that some property is hereditary in the *f*-sequence as ground to a conclusion about that property derives from the definition of following in a sequence.

Notice further that the other examples that Frege cites as fruitful, namely, that of continuity and of a limit, are not like the definition of following in this regard but are instead like the definition of being hereditary. They are second-level concepts the definitions of which involve the concavity with lower case letters. Because they contain inference licenses and hence take the form of generalized conditionals, they can be used in inferences that follow our general strategy of linear and joining inferences. But because the inference licenses they contain concern what follows if some *object* satisfies some condition rather than what follows if some *concept* satisfies some condition, they could not be used as Frege uses his definition of following in a sequence in the proof of theorem 133; they could not supply an inference license for any step in any mathematical proof. But if that is right then we need to distinguish two cases that Frege does not distinguish. In the one case, the definition, because its definiens has the form of a generalized conditional, can figure in a proof that proceeds according to our general strategy of linear and joining inferences and reveals thereby that various defined concepts have various logical properties and relations. Many proofs in mathematics are of this general form. The other case is more interesting insofar as the definition is supplying not merely fodder for an inference, that is, premises from which to reason, but instead the means of inference, a license or rule according to which to reason. In this case, it will be suggested, there is an especially clear sense in which although the definitions provide everything necessary for the proof, nonetheless, the conclusion is contained in the definitions not implicitly but only potentially, a potentiality that is actualized only by the course of proof.

Frege claims that in the case of a fruitful definition, one that draws new boundary lines, that is, contains an inference license, what can be inferred from it "cannot be inspected in advance," that "here, we are not simply taking out of the box again what we have just put into it" (Frege 1884, sec. 88). In a derivation involving fruitful definitions that are not used to derive inference licenses but instead merely provide premises for inferences, say, by hypothetical syllogism, there seems to be at least some sense in which what can be inferred *can* be inspected in advance. Indeed, our general strategy involving linear and joining inferences already suggests this. One can, in that case, simply read off the theorem one is trying to prove at least some of the intermediate steps that will be needed along the way. The problem of finding the path that will take one from the starting points to the desired theorem is not, in light of the general strategy, essentially different from that of finding the proof of, say, theorem 51 from axioms. There are important differences between the two cases because only in the one case is one proving based on definitions and indeed fruitful definitions and hence can employ the general strategy to show how things stand with those defined concepts. But there is also, as Frege indicates, an important distinction to be drawn between a derivation from definitions that employs only the general strategy and one, like that of theorem 133, that also uses a definition to derive an inference license that is needed in the proof. Because in the latter case the definitions are not merely supplying the premises for inference but also rules according to which to reason one cannot in this case just *see* based on the structure of the relevant formula what may be inferred. Here no general strategy is possible because what may be derived depends in particular on what rules of inference may be inferred from the particular definitions with which one is actually working.

In the case of a proof involving inferential steps that are licensed by a rule derived from a definition, although one has already in the definitions everything that is needed to derive the desired theorem, it is in an important sense not merely by logic that the theorem is derived from the definitions—as it is merely by logic that one proves a theorem from definitions using our general proof strategy of linear and joining inferences—because some steps are licensed not by rules of logic but instead by rules derived from a definition or definitions. The definition in this case does not merely provide the matter for inference, the beams to be combined and recombined. It gives one the *power* to draw an inference that could not otherwise be drawn, a power that is exercised in the course of the proof, in one's use of the rule actually to draw inferences. It is in just this sense that in a proof such as that of theorem 133, by contrast not only with Frege's Part II derivations but also with any proof from definitions that requires only our general strategy, the conclusion is contained only potentially in the definitions with which one begins, as the plant is contained in the seed. Only the actual course of proof employing the rule contained in the definition can realize the potential or power that is contained in that definition.[18]

[18] Here we clearly see something in the vicinity of Poincaré's idea that there are distinctively mathematical modes of reasoning that if reduced to logical modes are somehow destroyed. An inference that is licensed by a rule derived from a definition can be conceived purely formally, as an instance of detachment,

Perhaps it will seem that this cannot be right given that, as Frege himself says, "surely the truth of a theorem cannot really depend on something we do, when it holds quite independently of us" (Frege 1914, 207). Indeed, Frege makes the remark in the context of a discussion of the status of Euclid's postulates aimed at correcting the misimpression that postulates are somehow essentially different from axioms. A postulate is not to be seen as a rule governing the actual drawing of lines but instead refers, Frege thinks, to an objective conceptual possibility: "Our postulate cannot refer to any such external procedure [as actually drawing lines]. It refers rather to something conceptual. But what is here in question is not a subjective, psychological possibility, but an objective one" (Frege 1914, 207). "So," Frege concludes, "the only way of regarding the matter is that by drawing a straight line we merely become ourselves aware of what obtains independently of us." In a proof, whether in Euclid or in Frege, the truth that is derived obtains independently of the activity of writing, independently of drawing lines in a Euclidean diagram and of writing theorems in *Begriffsschrift*. Nevertheless, as Frege indicates, the *discovery* of that truth is not in the same way independent of the activity of writing. Demonstrations extend our knowledge not by creating truths but by showing what can be derived, and hence known, on the basis of given starting points. The possibility of such a showing is an objective possibility; nevertheless, the showing is needed if we are to come to see what is in this way available to be seen.[19] It is the proof, the actual chain of reasoning, of inferring based on acknowledged truths, that puts one in a cognitive relation of knowing to that which is proved.

A related concern arises in light of the status of definitions as stipulations: if a definition is merely a stipulation then it can seem that a conclusion derived from definitions is not properly speaking an objective truth, or at least not a very interesting objective truth but merely something provable given the stipulation. But this too is mistaken. Although a definition is a stipulation, as Frege says, it is also, along another dimension, something about which one can be right or wrong. One cannot be wrong to stipulate that some newly introduced sign is to have the same meaning as some other collection of signs. But one *can* be wrong to think that one has in that

but this is to miss the special character of the step and hence the "mathematics" of it. But notice that this is true even in the case of theorem 133, which is purely logical in the sense of involving only purely logical notions even in the definitions. What matters is that there are definitions, definitions that can license inferences in the proof.

[19] Is it significant in this context that Frege holds that geometry is synthetic although arithmetic and logic are not? I do not see that it is. To say that geometry is synthetic is, for Frege, to say that its proofs rely on truths that "are not of a general logical nature, but belong to the sphere of some special science," namely, in this case to the sphere of geometry (Frege 1884, sec. 3). Frege's proof of theorem 133 is not synthetic in this sense but instead analytic; it relies "only on general logical laws and definitions" (ibid.). Yet it is, Frege holds, ampliative "and ought therefore, on Kant's view, to be regarded as synthetic" (Frege 1884, sec. 88). The question whether a chain of reasoning can extend our knowledge must be kept separate from the question whether it depends on logic and definitions alone or also on the non-logical laws of some special science such as geometry, or if logicism is false, arithmetic.

collection of signs set out the inferentially articulated content of some concept. A concept, which is the *Bedeutung* of a concept word, is something objective; it is not up to us to decide what concepts there are in mathematics and logic. Because it is not, one's proposed definition can fail to designate any concept. It is, then, a fully objective matter what logical bonds actually obtain among concepts. We can make mistakes, even in mathematics and in logic. In particular, we can think that we have a proof of some theorem when in fact, as we may eventually discover, the conceptions on the basis of which the "proof" proceeds are flawed.

Judgment, conceived following Frege as an acknowledgement of the truth of a true thought, can succeed only if the relevant thought is true; and inference similarly, which aims to acknowledge a truth on the basis of another (acknowledged) truth, can succeed only if the ground of the inference is true *and* the passage is legitimate. But if so, then it can happen that although it might seem to a thinker that a logical bond among concepts has been revealed in the course of a proof such as Frege's, in fact it has not. The possibility, or impossibility, of showing that those bonds obtain is not in any way dependent on the proof; and it is this possibility, or impossibility, that insures that even deductive reasoning from definitions is answerable to something outside of it. One can extend one's knowledge by deductive reasoning on the basis of definitions because (among other things) the truth that is revealed in the proof is in this way independent of what we do, both what we do in stipulating in a definition and what we do in drawing an inference as licensed by some definition.

Frege claims that his proof of theorem 133 is ampliative despite being strictly deductive, that is, truth preserving, and that definitions of the sort that he describes as fruitful can in general provide the basis for ampliative deductive proofs. We have seen that any definition that involves his conditional stroke together with the concavity would seem to count as fruitful in Frege's sense. But we have also seen that there are two very different cases. Definitions of second-level concepts such as that of being hereditary or being continuous involve the concavity and lower case German letters, and cannot be used in a proof to supply an inference license. They can figure only in proofs that (at least as far as they are concerned) follow something like our general proof strategy. In other cases such as that of following in a sequence, the formulation of the definition requires using instead the concavity and upper case German letters. Such a definition can thus supply an inference license that can be applied if some concept has the relevant (higher-level) property and hence can be employed *in* a proof, even in one that is strictly logical or mathematical. Frege's proof of theorem 133 involves just such an inference license, indeed, more than one, all derived from the definition of following in a sequence. And we suggested that the proof is clearly ampliative for just this reason. Does it follow that proofs that employ only the general proof strategy of linear and joining inferences are not ampliative, that in such cases, for all one's to-ing and fro-ing one never leaves the same spot? This seems too strong. We need to recognize two different cases, both of which yield judgments that are ampliative despite being the result of purely deductive reasoning.

Definitions of concepts that draw new lines, we have seen, are distinctive in having inference licenses in their definientia. Concepts of this sort are intelligible unities, wholes of parts that are nevertheless not reducible to those parts (by contrast with concepts the contents of which are given by conjunctions or disjunctions of marks, which *are* reducible to their parts). Any conclusion (of a chain of deductive reasoning) that is ampliative is likewise an intelligible unity, now at the level of the proposition rather than at the level of a concept. Both conclusions achieved by using our general strategy for reasoning from fruitful definitions (such as that following in a sequence is hereditary) and conclusions that also use an inference license derived from a definition (such as theorem 133) are intelligible unities in this sense. They are necessary (not merely accidental) but not merely explicative. Now we need to apply the notion of an intelligible unity not only at the level of concepts and at the level of the proposition but also at the level of the whole proof. A proof such as that employing only the general strategy of linear and joining inferences is a kind of essential unity; because it employs only laws of logic in moving from one proposition or propositions to the next, there is a sense in which the whole is prior to the parts. The proof as a whole is not an intelligible unity but instead an essential unity. It is just this that gives one the impression that in this case for all one's to-ing and fro-ing one does not really leave the same spot. The proof, the reasoning, is analytic, even though in light of the definitions, and the way the definitions enable one to join defined concepts in judgments, the *conclusion* of the proof is ampliative. Only in the case of a proof such as that of theorem 133 are we dealing with intelligible unities at all levels, at the level of concepts (in the fruitful definitions), at the level of propositions (in the conclusion that extends one's knowledge), *and* at the level of the proof as a whole. In this case the conclusion is not merely by logic because some of the steps are licensed not by a rule of logic but by a rule derived from a definition. The step is necessary and truth preserving, in light of the definition, but it is not analytic in the way that an inference warranted by a law of logic is. The proof is a whole, a unity, because its steps are necessary, deductively valid. But because as some of its steps are not *logically* necessary, the proof also has real parts. It is not reducible to its parts—as for instance a step of inductive reasoning is insofar as in this case the relation between the premises and the conclusion is not necessary, not deductive—but it nonetheless has recognizable parts because not all the steps follow by logic alone. The proof is in this case an intelligible unity, a whole of parts that is not reducible to its parts. And that is precisely its charm. Not only does it give one new knowledge in the form of the conclusion, it also gives one a new sort of proof strategy, one that can be tried in other cases.[20]

[20] Mathematicians in their practice are interested both in what is proven and in how it is proven, and they are interested in the latter precisely because it can reveal new proof methods that can be employed in other cases. See Rav (1999).

8.4 Frege's Logical Advance

Natural language, we have seen, is constitutively object involving, sensory, and narrative. The ancient notion of a term, what things are called, further reflects this in its failure logically to distinguish, as we today find natural, between referring and predicative expressions. This is not merely a point about language. The world as it first comes into view is a world of things, paradigmatically living things, with their own proper natures and powers. Relative to this, our first understanding of reality, early modern mathematics and physics provide a radically transformed vision of the most fundamental nature of reality, and with it the idea of a concept that can be grasped as the concept it is wholly independently of any object to which it might be applied. This transformed vision is furthermore taken to be the truth lying behind the appearances of everyday sensory experience. What we have here distinguished as two profoundly different modes of intentional directedness on reality, namely, that afforded by the natural languages into which we are first acculturated and through which we first become *rational* animals at all, and that afforded by the symbolic language of arithmetic and algebra first developed by Descartes, were distinguished instead as a false or merely apparent conception of reality as contrasted with the true, mathematically described nature of reality. Thus, it came to seem, as is explicit in Descartes' *Meditations*, our cognitive access to reality is not by way of perception but by way of thought and acts of will by which we assent to our clear and distinct (mathematical) ideas. Inevitably, this dogmatic rationalism gave way in turn to a Humean skeptical empiricism. And only with Kant were the difficulties resolved—though at a price.

We saw in Chapter 4 that in Kant's critical view intuitions through which objects are given and concepts through which those given objects are thought are to be distinguished logically, as singular and general, but also metaphysically, as that to which we are essentially receptive or passive and that with respect to which we are active through the self-activity of spontaneity, and finally ontologically, as matter and form. Kantian intuitions are purely referential and have no descriptive or other cognitive content. And Kantian concepts are merely predicative; they cannot give objects but only provide ways of thinking about objects otherwise given. This distinction is furthermore quite explicitly a posit or hypothesis of Kant's critical philosophy, one that is introduced in order to explain our capacity for knowledge. We are simply told in the Introduction, by way of a "preliminary," "that there are two stems of human cognition, which may perhaps arise from a common but to us unknown root, namely, **sensibility** and **understanding**, through the first of which objects are given to us, but through the second of which they are thought" (A15/B29). The logical (and metaphysical and ontological) division of representations into intuitions and concepts is *not* discovered by reflection on thought and language but is instead *hypothesized* as part of an elaborate theory of cognition aimed at explaining the nature and possibility of our knowledge.[21]

Kant's problem is to understand the necessary but not logically necessary truths of mathematics and physics, how, in his terminology, there can be synthetic judgments a priori. And as we did in the Introduction, we can think of this problem from the perspective of Hume's division of knowledge into relations of ideas and matters of fact; we can understand Kant to be arguing that Hume's division conflates two very different distinctions, that between what is a priori and what is a posteriori, with that between what is analytic, by logic alone and merely explicative, and what is synthetic, not by logic and ampliative. Hume's relations of ideas are a priori and analytic (known by logic alone), and his matters of fact are a posteriori and synthetic, that is, grounded in experience. And as Hume argues, knowledge of causal relations cannot be accounted for by appeal to either category. So, he concludes, such "knowledge" is wholly unjustified, that is, not really knowledge at all. But, Kant responds, although it is true that if a judgment is analytic then it is a priori and similarly true that if it is a posteriori then it is synthetic, it is *not* true that if a judgment is a priori then it is analytic or that if it is synthetic then it is a posteriori. There cannot be analytic judgments a posteriori but there can be synthetic judgments a priori. And that every event has a cause is a paradigm of a judgment that is synthetic a priori. Such a judgment is not logically necessary, hence not analytic but instead synthetic; but it is nonetheless necessary hence not a posteriori but instead a priori. In any synthetic or ampliative judgment, Kant thinks, one must go outside the concept of the subject to an intuition, whether pure, in mathematics, or empirical, in the case of a posteriori knowledge, in order to determine whether the predicate concept ought to be affirmed of the subject.[22] In an analytic, merely explicative judgment, the predicate concept is contained already in the concept of the subject and so must be affirmed of the subject on pain of contradiction.

On Kant's account, the idea that a strictly deductive proof might extend our knowledge is utterly incoherent, a manifest contradiction. It is as much as to say that an analytic judgment is synthetic. And yet this is just what Frege claims, that his deductive proof of theorem 133 is ampliative, a real extension of our knowledge. And just as Kant does in relation to Hume's division of all knowledge into relations of ideas (known by deduction, by logic) and matters of fact (known by induction, by generalizing from instances), so we can see Frege doing in relation to Kant's division of all knowledge into analytic and synthetic. As Kant charges Hume with conflating

[21] Kant does claim in the *Jäsche Logic* (1800, 587; AK 9:86) that, though "in the doctrine of nature they are useful and indispensible," the sciences of mathematics and metaphysics "do not allow any hypotheses." This seems, however, to be false of Kant's own philosophy.

[22] The propositions of Transcendental Logic are also synthetic a priori but, we have seen, they are distinctive in being transcendental, and indeed the only transcendental propositions. Unlike the synthetic a priori propositions of mathematics, they take the form of rules rather than the form of claims. They are necessary, but they are not necessary truths as the judgments of mathematics are.

two different distinctions, the analytic/synthetic distinction with the a priori/a posteriori distinction, so Frege reveals that Kant has similarly conflated what must be distinguished, namely, the explicative/ampliative distinction with the strictly-deductive/involving-intuitions distinction. It is true that if a judgment is merely explicative then it is strictly deductive and indeed by logic alone, and it is also true that if a judgment essentially involves intuitions of objects then it is ampliative. But the converse conditionals are not true: it is not true either that all deductive reasoning is merely explicative or that all ampliative judgments constitutively involve intuitions of objects. There can be no merely explicative judgments that essentially involve intuitions of objects, just as for Kant there can be no analytic judgments that are known a posteriori. But, Frege suggests, there *can* be deductive reasoning (from the contents of concepts) that is ampliative just as for Kant there can be synthetic judgments a priori.

Kant's conception of the analytic/synthetic distinction is directly grounded in and made possible by his logical, and metaphysical and ontological, distinction between intuitions, through which objects are given in receptivity, and concepts, through which (given) objects are thought through the spontaneity of the understanding. To understand Frege's further logical move beyond Kant, as it is enabled by developments in mathematics in the nineteenth century, we need to see that the Kantian opposition of concepts and intuitions is too crude, that it conflates two essentially different distinctions, that between *Sinn* and *Bedeutung* with that of concept and object. Whereas for Kant all cognitive significance, all being for a thinker, is through concepts, and all objectivity, all truth, lies in relation to an object or objects, either as given in sensory experience or merely with respect to their form in mathematics, Frege requires us to distinguish, on the one hand, between cognitive significance (Fregean sense, *Sinn*) and concepts, which are the *Bedeutung* of concept words, and on the other, between objective significance (*Bedeutung*) and objects.

As Frege understands it, the *Sinn*/*Bedeutung* distinction is orthogonal to that between concept and object; according to him, both concept words and object names both express senses and designate or mean concepts and objects, respectively. He writes in some comments on his distinction between *Sinn* and *Bedeutung*:

> It is easy to become unclear about this [the sense/meaning distinction as applied both to object names and concept words] by confounding the division into concepts and objects with the distinction between sense and meaning, so that we run together sense and concept on the one hand and meaning and object on the other. To every concept-word or proper name, there corresponds as a rule a sense and a meaning, as I use these words. (Frege 1892, 118)

Kant, we can see in retrospect, commits just the error Frege warns against. Kantian intuitions have *Bedeutung* but, independent of the involvement of (Kantian) concepts, they lack all cognitive significance: intuitions without concepts are blind. Kantian concepts, correspondingly, express, or even are, Fregean senses, but independent of any object given in intuition they have no objectivity, no *Bedeutung*.

Kantian intuitions are both of objects and the source of all objectivity; and Kantian concepts are at once predicates of possible judgment and that through which things have any and all cognitive significance to a thinker. But as Frege sees, not all objectivity lies in relation to an object and cognitive significance is not merely predicative. On Frege's view, again, both concept words and object names express senses, and so are cognitively significant, and both concept words and object names designate (or at least ought to designate) something objective, namely, concepts conceived as laws of correlation and objects, respectively.

That not all objectivity lies in relation to an object is evident already in Descartes' mathematical practice. Although our first and most basic cognitive involvement in the world is essentially object oriented and object involving, we can and in early modern thought do achieve a fundamentally different intentional directedness on reality, one that is, as has already been suggested, *fully* realized only in the mathematical practice that emerged in the nineteenth century. Although Descartes' practice is, in intention, an exercise of pure reason, it is in the contemporary mathematical practice of deductive reasoning from the contents of concepts that we first see how this can be, and as well the respects in which mathematics as a science has always been about concepts rather than about objects. But if that is right then we need also to draw a distinction that Frege did not draw. Whereas Frege holds that the *Sinn/Bedeutung* distinction is orthogonal to the division of concept and object, so that both object names and concept words both express senses and designate, respectively, objects and concepts, we will see instead that, although the *Sinn/Bedeutung* distinction applies to any sort of language, to language as such, the distinction between objects and concepts functions differently: it serves to demarcate two fundamentally different sorts of languages, namely, natural languages and mathematical languages.[23]

A language in the sense of concern here is not merely a means of recording or reporting something of which one is antecedently and independently aware. Rather it is the medium of our (self-conscious) awareness, that through which we, as thinkers, rational beings, achieve any and all cognitive relation to reality. What Frege enables us to realize is that any such language must involve both sense, *Sinn*, and designation or meaning, *Bedeutung*. Language, that is to say, is inherently revelatory, at least in intention; by its nature as language it reveals or discloses (or in cases of failure, only purports to reveal or disclose) something objective, some aspect of reality, *Bedeutung*. But language can be that, the vehicle of our cognitive awareness, only in virtue of expressing Fregean sense. The words of natural language, for example, do not merely

[23] We saw in section 4.2 that it was Kant who first conflated the two sorts of languages in his division of concept and intuition. It is, furthermore, this conflation of the two sorts of languages that leads many philosophers and educators to think that the signs of mathematics are merely abbreviations for words of natural language, that mathematics is done in natural language. (See section 6.4.) The conflation would also seem to be responsible for the nearly universal and often fervent conviction that mathematical logic is the logic of natural language.

stand for or refer to objects and their properties and relations; they are also constitutively inferentially related. To have the use of a word of natural language, to grasp its sense, is not only to be able to apply it correctly, to know which things are called that, but also to be able to *reason* about such things and in virtue of such reasoning to have second thoughts.

We saw already in Chapter 1 that to be rational, to have the power of knowing, is to have the capacity for second thoughts, where a second thought is not merely a change in what one takes to be so but instead a reasoned change of mind. It follows directly that the language into which we are acculturated, and which realizes us as the rational animals we are, must involve both referential relations to things that are called that (and are the *Bedeutungen* of the words of that language) and inferential relations belonging to the sense (*Sinn*) expressed regarding what is a reason for what. And although we learn these referential and inferential relations piecemeal in the course of our acculturation into language, eventually, it was suggested, they are synthesized into one whole that embodies a view of reality. The smallest unit of cognition on this account is not an object in its nature, as the ancient Greeks thought; nor is it the judgment, as Kant held. It is the whole inferentially articulated language.

But Frege did not discover the *Sinn/Bedeutung* distinction, as contrasted with that of concept and object, by reflecting on natural language. Nor could he have. Much as the Kantian notion of a concept, as that notion contrasts with that of a Kantian intuition, is made possible by developments in early modern mathematics, so Frege's understanding of the *Sinn/Bedeutung* distinction as contrasted with that of concept and object is made possible by developments in mathematics in the nineteenth century. It was those developments that provided both the impetus for Frege's invention of his concept-script and the crucial guide to how it must function. And it was Frege's subsequent reflections on the language that he had devised for reasoning from the contents of concepts in this form of mathematical practice that brought him explicitly to realize that the language he devised functions by directly expressing senses, by mapping in signs Fregean thoughts that are variously analyzable into function and argument. In *Begriffsschrift*, and we will see, only in *Begriffsschrift*, are the Fregean notions of sense (*Sinn*) and meaning (*Bedeutung*) *manifestly* different and distinct.

Natural language involves both word–world referential relations and word–word inferential relations and as such embodies a view of the world, of what there is and of how things work in the broadest sense. But there is in such a language no way clearly to demarcate the two; in natural language, sense (*Sinn*) and meaning (*Bedeutung*) are, as McDowell (1994, Lecture I, sec. 4) argues, inextricably combined.[24] This is due in part to the way the language comes about, due to the fact that it evolves in and through our interactions with salient objects in the environment as the biological and

[24] McDowell employs instead Kantian terminology here; he says that concepts and intuitions are inextricably combined. The reason he does will soon become apparent.

social beings we are. Through those interactions we acquire habits of thought, expectations about what will happen in what circumstances, and these coalesce as reasons as we become rational. Because these inferential relations are between words, more exactly, what they designate, sentences expressing such relations can take the same form as other, merely empirical or factual generalities. The only discernible difference between the two cases appears in the context of counterfactual reasoning or in claims of necessity, and often one just does not know what to say. We know, more or less, what it means to say that As are and must be Bs, that nothing could be an A without being also B, as contrasted with the merely contingent and accidental fact that all As are C. (To say that As are and must be Bs means, at least, that one can infer that if something were an A then it would be B. Contingent, accidental generalities do not license subjunctive conditionals.) And there are some circumstances in which we have strong intuitions one way or the other, but as the language continues to evolve what had seemed to be a matter of meaning, that is, an inferential relation codified at the level of sense, can be shown empirically to be false, and what is at first discovered as a matter of fact can coalesce into a relation of ideas (an inferential relation at the level of sense). As Quine famously argued, there is no in principle distinction to be drawn here between judgments that are analytic and judgments that are synthetic. The *Sinn*/*Bedeutung* distinction is at work in the language but not even a notional separation of the two is possible in the case of natural language. And because it is not, no one could discover the distinction that Frege discovered merely by reflecting on this case.

The notion of Fregean sense as that (inferentially articulated content) through which we are cognitively in relation to something objective, something about which we can have second thoughts, can be discovered only in relation to a very sophisticated and historically late form of mathematical language such as Frege's *Begriffsschrift* within which to express senses. The distinction is manifest in Frege's language precisely because what formulae of that language directly map or trace is not truth conditions but inference potential, that is, sense as it *contrasts* with meaning (which is what matters to truth). But once the notion of sense *has* been discovered it can be seen also to apply to the case of natural language. When, for instance, McDowell (1994, 3) says that "concepts mediate the relation between minds and the world," what he means by "concept" is Fregean sense.[25] As McDowell reads him, one of Kant's deepest insights is that our cognitive relation to reality is essentially mediated by (Kantian) concepts conceived as Fregean senses. And there is some truth to this.[26] But, again, because in the case of our everyday intentional involvements in the world

[25] This is made explicit only at the end of Lecture V, and has been a source of very real confusion for readers of McDowell. On the role of the notion of Fregean sense in *Mind and World* see also the Afterward, Part III, sec. 5.

[26] As we can retrospectively put the point, although transcendentally a Kantian concept is more like a Fregean concept, a law of correlation, empirically it seems to function instead as a Fregean sense. Empirically a Kantian concept is that through which things are made manifest to us.

as enabled by our acculturation into natural language, sense and meaning are inextricably combined, one can discover the notion of sense and its constitutive role in language only in relation to a sufficiently advanced mathematical tradition, one that enables a language such as Frege's in which sense and meaning are not inextricably combined. Only *after* we have discovered and clarified the *Sinn/Bedeutung* distinction for the case of mathematics can we then apply it, as McDowell does, to the case of natural language and see it in play, however inchoately, in Kant's thinking.

Nor does a mathematical language such as Frege's differ from natural language only in its clearly distinguishing between sense and meaning. The nature of meaning, *Bedeutung*, in the two cases is also essentially different. Natural language is, again, constitutively object involving, sensory, and narrative. It reveals a world of perceptible objects, paradigmatically living beings with their characteristic natures and powers, properties, and relations. It follows directly that the "concept words" of this language as much as its "object names" are object involving; *both* sorts of expressions—'snub-nosed', 'human', and 'sitting' as much as 'Socrates' and 'Athens'—are what things are called. Natural language, that is to say, knows nothing of Fregean concepts, which are laws of correlation arguments to truth-values. The words of natural language do not have sharp boundaries to their application, and they cannot be defined on the basis of a few primitive notions in the way that mathematical words can.

Natural language does not involve anything like a Fregean concept, and mathematical language, correspondingly, knows no objects but only concepts, laws of correlation that have sharp boundaries, *tertium non datur* as Frege puts it. Thus, although any language involves both *Sinn* and *Bedeutung*, the concept/object distinction is not, as Frege thought, merely orthogonal to that distinction but instead marks an essential difference between the natural languages that first realize us as rational at all and Frege's mathematical language that is an essentially late fruit of over twenty-five hundred years of mathematical investigation. And again, one does not actually need Frege's language to do the sort of mathematics for which Frege's language is designed. One can grasp the senses through which mathematical concepts are disclosed without making essential use of any written system of signs, as most mathematicians working today do, and as any of us do insofar as we are able to understand the reasoning of a proof such as that to show that there is no largest prime. The problem for the mathematician is that without such a language it can be extremely difficult to communicate either one's understanding of those concepts or one's course of reasoning to a particular theorem. For the philosopher the difficulty is to understand how the mode of inquiry works given that, without any written system of signs such as that Frege provides, one cannot see the reasoning at work in a public display.

In a well-known letter to Husserl, dated May 24, 1891, Frege presents a little schema according to which: propositions express senses, Fregean thoughts, and designate (mean) truth values; proper names express senses and designate objects;

and concept words express senses and designate concepts. And this notion of a concept, as the meaning, *Bedeutung*, of a concept word, is then related to the notion of an object that falls under that concept. As Frege remarks regarding this schema:

> With a concept word it takes one more step to reach the object than with a proper name, and the last step may be missing—i.e., the concept may be empty—without the concept word's ceasing to be scientifically useful. I have drawn the last step from concept to object horizontally in order to indicate that it takes place on the same level, that objects and concepts have the same objectivity. (Frege 1891, 63)

If, as I have suggested, both the "concept words" and the "object names" of natural language are object involving, that is, revelatory of objects with their properties and relations, and mathematical language instead discloses (mathematical) concepts with *their* properties and relations, Frege's schema is not quite right. There are no mathematical, or logical, objects, and the only (Fregean) concepts there are are mathematical and logical ones. (When we use the word 'concept' in relation to natural language, very often what we seem to mean is something like Fregean sense.) But there is something right both about the idea that concepts and objects have the same objectivity and about the idea that with concepts it takes one more step to reach the object. The only objects there are are empirical objects, or more generally, empirical reality. This reality is directly revealed through the medium of natural language. But as we will see, if only in outline, in Chapter 9, empirical reality is also, though very differently, disclosed through the medium of mathematical language, and this involves two distinct steps just as Frege suggests regarding concepts. The concern of the mathematician is mathematical concepts. The task is to grasp them in their pure form and to understand their various logical properties and relations. Mathematicians do not, *qua* mathematicians, take the second step to objects, empirical reality. But physicists do, and in twentieth-century physics in particular, as it contrasts with early modern physics, the mathematics physicists employ directly discloses reality. Mathematicians discover objective truths about mathematical concepts and in so doing discover how things could be. Physicists, using those concepts, discover how things are.[27]

Although both natural language and mathematical language are revelatory of reality, and as such involve both sense and meaning in Frege's technical sense, they are nonetheless very different insofar as natural language reveals sensory objects together with their properties and relations and mathematical language directly discloses instead (Fregean, non-sensory) concepts together with their properties and relations, and only indirectly, in physics, discloses objects (or whatever is discovered in fundamental physics to be the elements of reality). This difference is

[27] It is worth recalling that, as we saw in section 5.2, Riemann's understanding of his mathematical work, at least on Stein's (1988) account of it, was in terms of the idea of exploring conceptual possibilities; it aimed "to open up the scientific *logos* in general, in the interest of science in general" (Stein 1988, 252).

further reflected in, for example, the fact that natural language is inherently narrative while mathematical language is not. Natural language tells of what happens; mathematical language, even in its use in physics, we will see, instead tells of what, in some sense timelessly, is. Natural language also has (as already noted) many, many words none of which can be defined in terms of others and all of which are inferentially related to at least some of the others. A mathematical language such as *Begriffsschrift* by contrast has only a few primitive notions in terms of which all other concepts can be strictly defined. Those defined concepts are furthermore *internally* inferentially articulated, and it is on the basis of this internal articulation that relations of entailment between different concepts can be proven deductively. But the most significant and fundamental difference between the two sorts of languages, we will see, lies in what we can think of as the relative priority of *Sinn* and *Bedeutung* in each, which is in turn indicative of their respective relationships to our receptivity and spontaneity.

Natural language is not invented or devised but rather evolves. It merely happens much as the living things that are the fruits of biological evolution merely happen. And any subsequent self-conscious attempts to change it must be taken up by other speakers if they are to succeed. Natural language belongs to no one in particular and answers to no one in particular. It is ours simply in the sense that we experience the world through it, and speak it together, thereby keeping it alive. And because we are in this way passive with respect to it, the reality (*Bedeutung*) it reveals has a kind of priority over the sense (*Sinn*) through which that reality is revealed. That is, as McDowell has argued, because thoughts are in this case object dependent, where the relevant object is missing, the corresponding Fregean thought must go missing as well. It follows directly that "a subject may be in error about the contents of his own mind: he may think there is a singular thought at, so to speak, a certain position in his internal organization although there is really nothing precisely there" (McDowell 1986, 145). Given how profoundly object involving, *world* involving, natural language is and must be, in cases in which we are under an illusion that there is something where there is nothing, the relevant thought is simply not available to be entertained *because* that (would-be) thought is object dependent as it is. In this case, thoughts without content really are, as Kant says, empty; that is, as McDowell (1994, 4) glosses, they are not, in that case, really thoughts at all. Our capacity to know reality as it is revealed in and through natural language is an essentially receptive, passive capacity.

The situation is very different in the case of mathematical language. First, mathematical language is invented or devised; it is self-consciously created and does not need to be taken up by others as a condition on its having been created. And again this is a point not so much about any actual written language such as Frege's, as it is about the grasp of mathematical concepts through senses that mathematicians can achieve, with or without a written system of signs within which to work. A solitary mathematician can develop important areas of mathematics; those developments do not wait on being understood by others to be actualized as the mathematical advances they are. Similarly, for more than a century no one other than Frege understood Frege's language, and it could have happened that the earth and all that is on it was utterly destroyed without anyone else ever understanding it. The language would nonetheless be precisely the language that is it.[28] Furthermore, as has been repeatedly emphasized, Frege's language and the nineteenth-century mathematics that inspired it are the culmination of over two millennia of intellectual work on the part of mathematicians. We are not, in other words, merely passive with respect to either. Nineteenth-century mathematics and a language such as Frege's do not just happen. They require devoted and sustained intellectual work.

Frege claims in *Grundlagen* that "often it is only after immense intellectual effort, which may have continued over centuries, that humanity at last succeeds in achieving knowledge of a concept in its pure form, in stripping off the irrelevant accretions which veil it from the eyes of the mind" (Frege 1884, vii). To achieve knowledge of a concept in its pure form is to get the sense right and given that we begin with an essentially sensory grasp even of mathematical concepts—a conception of number as a collection of units, of a circle as a two-dimensional plane figure, of addition as putting together, and so on—it takes real intellectual work, and we have seen, revolutionary transformations, to rid our conceptions of mathematical concepts of this sensory content. An *adequate* mathematical understanding, and correspondingly, an adequate mathematical language, one that discloses mathematical concepts as they are, is thus an achievement of the highest order. It is an achievement of the spontaneity, the self-activity, of reason. It follows that in this case the notion of sense has a kind of priority insofar as we can discover in the course of our mathematical investigations that the sense through which we had taken ourselves to grasp some concept is confused in some way or even wholly mistaken. Frege's Basic Law V is an obvious example. The *Begriffsschrift* formula for that law does express an inferentially articulated Fregean thought even though, as Frege learned from Russell, it does not designate any truth-value, either the True or the False. But it does express a thought. Indeed, it is only because it expresses a thought that one can discover that it designates no truth-value. Were no thought expressed one could not draw the inferences (more precisely, the pseudo-inferences given that Basic Law V is not true) that reveal the problem. The harmony between thought and reality that is always already given in the case of natural language is in mathematics a very hard-won achievement.

Developments in mathematics in the nineteenth century, and the language those developments inspired Frege to create, thus enable in turn a fundamental and

[28] This phenomenon in the case of mathematics does not show that Wittgenstein's private language argument is wrong, but it does show that it does not apply directly in the case of mathematics. Mathematical languages, and one's grasp of senses of concepts more generally in mathematics, can be private in a sense, but that is only because, as we saw in our discussion of the Aristotelian model of science (in section 8.2), there are other ways in mathematics to enforce a principled distinction between what the mathematician thinks is right and what is right.

profoundly transformative advance in logic, the discovery of the *Sinn*/*Bedeutung* distinction as that distinction contrasts with the concept/object distinction. It is this distinction that we need in order to understand how it is that through the acquisition of language one acquires the eyes to see things as they are, literally so in the case of natural language and figuratively speaking in the case of mathematical language. In both cases, our cognitive access to something objective, *Bedeutung*, is mediated by our grasp of an inferentially articulated sense, *Sinn*. But, we have seen, there are also very striking differences between the two cases, beginning with the fact that what natural language reveals is a world of sensory objects with their (sensory) properties and relations; mathematical language discloses that same reality only indirectly, through mathematical concepts that are then applied in fundamental physics. What the mathematician is directly concerned with is not objects but instead concepts.[29] And in this case there really can be empty thoughts, perfectly contentful thoughts that have no truth-values, that are neither true nor false. Thus, for Frege, as for the intuitionist, bivalence fails. One cannot in general infer from the fact that the (mathematical) thought is not true that its negation is true; it is only the negation of (mathematical) thoughts that are themselves false, that is, that have a truth-value and in particular the truth value False, that can be inferred to be true. In Frege's system of signs this is marked by the content stroke: much as affixing the judgment stroke aims to mark a thought as true (and because judgment is an acknowledgement of truth, can fail to achieve its aim) so affixing the content stroke to a collection of signs in Frege's language aims to mark a thought as truth-evaluable, as *having* a truth-value, either the True or the False (perhaps on some condition). And as one can fail in one's attempt to acknowledge the truth of a thought (because the thought is not true) so one can fail in one's attempt to acknowledge even that a content is judgeable, that is, that it is true or false (perhaps on some condition). One does so fail in attaching the content stroke to a thought that, like Basic Law V, is neither true nor false.[30]

Bivalence fails at the level of thoughts for Frege because he recognizes, at least for the case of mathematics, that some perfectly well inferentially-articulated thoughts are neither true nor false. Bivalence nevertheless holds (*pace* the intuitionist) for the case of concepts. Concepts—that is, mathematical and logical concepts, the only sorts of (Fregean) concepts there are—have sharp boundaries in the sense that they settle for every possible object (or lower-level concept in the case of higher-level concepts) as argument whether the concept applies or not. Hence, to discover in a particular case that we do not know what to say, or worse, that the concept appears both to

[29] Isaacson (1994) also suggests that mathematics is about objective concepts, and he has in mind contemporary mathematics in particular in putting forward this claim. Isaacson is furthermore concerned in particular to understand the relationship between mathematical truth and mathematical knowledge. What he does not do is develop these ideas to the level of clarity and detail that we are able to here.

[30] This account of the content stroke is different from that put forward by Frege in *Grundgesetze*. I do not consider that latter conception an advance in understanding over what Frege had achieved already in the 1879 logic and here propose what I take to be Frege's best wisdom.

apply and to not apply, just is to discover that we have not yet grasped the concept in its pure form. To say that the concept is objective is to say that it is there to be grasped, that we can get it wrong, as we may eventually discover, but also that we can get it right.

8.5 The Achievement of Reason

Frege's formula language of pure thought was not understood and has never before been used by anyone other than Frege. Nor has anyone since Frege attempted, so far as I know, to devise independently a language within which to reason from defined concepts in mathematics. Mathematicians seem, on the whole, to be quite content merely to report their reasoning.[31] In a way this is unsurprising given that it is part of the self-understanding of practitioners of this form of mathematics that their work should be unconstrained by any particular sort of symbolism. The Kantian opposition of discursive and intuitive uses of reason may also be playing a role insofar as, at least on Kant's account in the first *Critique*, reason in its discursive use can only be reported and is not essentially written in the way that reason in its intuitive use is. That this Kantian division of uses of reason, the one intuitive and the other discursive, has discouraged any attempt to devise a language such as Frege's is further indicated by the vehemence with which mathematicians for much of the twentieth century eschewed any and all appeals to diagrams in their work.[32] Of course, the use of symbols was not frowned upon in the same way but that may well have been because symbols were not conceived (following Kant) constructively, as formulating content on the basis of which to reason, but were instead taken (we saw in Chapter 6) merely to abbreviate words. But although unsurprising, the fact that no one has taken up the task of devising a mathematical language for this new form of practice that first emerged in the nineteenth century is also significant insofar as it reinforces the idea that this form of mathematical practice really is by reason alone. Even Frege did not devise his language first and foremost as a language for use by the working mathematician. He devised his language in order to show that arithmetic is merely derived logic. As Frege makes clear in the Preface of *Begriffsschrift*, it was the demands of the project of logicism not those of the new mathematical practice that provided the impetus for *Begriffsschrift*.

[31] Thurston (1994) is an exception. He laments the "dysfunctional" habits of communication of mathematicians, both in their interactions with each other and in their interactions with students in the classroom (see especially the first paragraphs of section 3, p. 42). At the end of section 3, he urges that more effort should be expended on "communicating mathematical *ideas*," and "on understanding and explaining the basic mental infrastructure of mathematics." And this, he goes so far as to suggest, "entails developing mathematical language that is effective for the radical purpose of conveying ideas to people who don't already know them" (Thurston 1994, 45). What Thurston wants is what Frege already provides.

[32] For discussion of this history, and some of the reasons there has recently been a revival of interest in the role of various sorts of diagrams and images in logic and mathematics, see Mancosu (2005).

> So that something intuitive {*etwas Anschauliches*} could not squeeze in unnoticed here [in the reduction of the notion of ordering-in-a-sequence to that of a logical ordering], it was most important to keep the chain of reasoning free of gaps. As I endeavored to fulfill this requirement most rigorously, I found an obstacle in the inadequacy of the language; despite all the unwieldiness of the expressions, the more complex the relations became, the less precision—which my purpose required—could be obtained. From this deficiency arose the idea of the 'conceptual notation' presented here. (Frege 1879, 104)

For the first time in its twenty-five hundred year history, mathematics simply does not need a system of written signs within which to do its work. And the reason it does not is that with this form of mathematical practice the power of reason as a power of knowing is, for the first time, *fully* achieved.

We have seen that Frege's most fundamental logical advance was the discovery of the *Sinn*/*Bedeutung* distinction, more exactly, the discovery that we need to distinguish, in a way that Kant did not, between the notion of a concept as the *Bedeutung* of a concept word and the notion of sense, what is expressed by various expressions in a language, on the one hand, and between the notion of *Bedeutung*, which is the notion of something objective that is designated by an expression, and that of an object, on the other. But we have seen that we also need to distinguish as even Frege did not between natural language, which is revelatory of a world of sensory objects with their natures and powers, their properties and relations, and mathematical language through which concepts, with their properties and relations, are disclosed. Natural language is, furthermore, immediately revelatory relative to mathematical language. Although it constitutively involves Fregean sense and is essentially inferentially articulated, it is nonetheless immediately revelatory insofar as it takes no work on our part to have things revealed to us in our experience of the world. In the case of natural language, there is always already a pre-established harmony between the knower and the known, between thought and reality. Although fallible and capable of improvement, our powers of perception, which we have as the rational animals we are, just are (passive) capacities to take in how things are.[33] The case of mathematics is very different insofar as it takes, must take, serious intellectual work and real intellectual growth and maturation in order for us, the community of inquirers, to achieve knowledge of mathematical concepts in what Frege describes as "their pure form." The power of reason as a power of *knowing*, unlike the power of perception as a power of knowing, is not and cannot be there from the beginning, a power we have as the rational animals we are. It can only be achieved in historical time through our self-conscious efforts to understand and to know what is there to be understood and known.

[33] That our powers of perception can be improved with training and practice is a familiar fact of everyday life, though not one philosophers have taken much notice of. It was brought to my attention by Sydney Keough's work on expert perception in her senior thesis "Having a Taste for What's There" (Haverford College, 2011).

We have, as the rational animals we are, the power rationally to reflect and the ability to discover on reflection that what had at first seemed true (or good) is not really true (or good). This critically reflective power is, however, not in itself a power of knowing (at least at first) because and insofar as it has and needs something given on which to reflect. This does not imply that there is something merely subjective about the whole enterprise—as we are wont to think. The pre-established harmony of thought and reality that is enabled by natural language is *real*, and it follows from that harmony that thoughtful, critically reflective people will mostly, though not infallibly, get things right. McDowell forcefully makes this point for the case of ethics, what is good, in "Two Sorts of Naturalism." There is, as he argues, "no addressing the question [whether the space of reasons really is laid out as it seems to be] in a way that holds that apparent layout in suspense, and aims to reconstruct its correctness from a vantage point outside the ways of thinking one acquired in ethical upbringing" (McDowell 1996, 189). But the point holds equally for the case of intellectual virtue concerned with what is true in our experience of things. One's eyes are (or at least ought to be) opened both to what is true and to what is good in the course of one's upbringing, both to their value as reasons and to the particular reasons that really are reasons in each case. In both cases, only what McDowell has taught us to think of as a modest account, always already situated in the midst of an understanding of what is true and good, is possible.[34] Reason has in this case no power to discover through its own unaided resources, independent of what is given in experience, either what is true or what is good.

In ancient Greek thought perception provides the principal modality of knowing, and we saw that this is true for Plato no less than for Aristotle insofar as knowledge of the Forms is for him a kind of seeing with the mind's eye. Knowledge in both cases is grasp of a thing as what it is, in its nature. And this is so even in mathematics, we saw, insofar as it is constructions that serve as the paradigm of what is known in ancient Greek mathematics. With Descartes, there is a complete reversal: the form of intellection that is involved in Descartes' new mathematical practice becomes the model even for our perceptual engagement with things. But although the power of reason as itself a power of knowing makes its appearance here, it nevertheless still needs something given on the basis of which to be exercised, namely, clear and distinct ideas (or, in Kant, the forms of sensibility). And because it does, what in Descartes is held to be a power of knowing becomes in Kant's new form of philosophical practice a power of critique by which to discover the limits of our power of knowing as a necessary corollary of the conditions of the possibility of knowing. Not only does reason have no contents of its own, it is simply impossible—on Kant's account of deduction, that is, of purely logical inference—that by reason

[34] The notion of modesty comes up in McDowell's (1987) and (1997) discussions of the shape a theory of meaning can take, but it seems clearly to apply in the cases of concern here as well.

alone one might extend one's knowledge.[35] It is precisely because it is not by reason alone but by reason guided throughout by intuition that judgment in mathematics can be synthetic, can constitute a real extension of our knowledge on Kant's account.

The question whether by reason alone knowledge might be possible—whether, that is, reason might itself, as pure reason, be a power of knowing—appears, in the wake of Kant's critical philosophy, to have two distinct parts, one regarding the contents of knowledge and another addressed to how by deductive reasoning one might extend one's knowledge as contrasted with merely making explicit something that otherwise remains only implicit. And the two questions are distinct because, it is thought, logic concerns form rather than, as contrasted with, content. Because what concerns only form cannot teach us anything about contents, it is clear on the Kantian conception of logic as formal that deductive reasoning cannot yield anything new that was not contained already in one's starting points, however obtained. The problem of truth and knowledge is completely intractable in the setting in which logical form is set over against content and so truth.

Nineteenth-century mathematical practice, as is made manifest in a proof such as that of Frege's theorem 133 written in *Begriffsschrift*, shows that this Kantian opposition of logical form and content as it matters to truth—which is needed in the wake of Descartes' revolution in mathematics in the seventeenth century—has been wholly superseded. As Frege himself says of his proof of theorem 133,

> we see in this example how pure thought {*reine Denken*} (regardless of any content given through the senses or even given *a priori* through an intuition) is able, *all by itself*, to produce from the content *which arises from its own nature* judgments which at first glance seem to be possible only on the grounds of some intuition. (Frege 1879, sec. 23; emphasis added)

Concepts and judgments that had seemed to have ineliminable intuitive content are revealed to be purely logical. Both the concepts Frege defines and the means by which theorem 133 is established belong to reason alone, and yet we have seen that there is a clear sense in which the derivation is ampliative, an extension of our knowledge: the conclusion, theorem 133, is contained in the definitions not merely implicitly but potentially. Much as an equilateral triangle is potential in a given straight line within the context of Euclid's system insofar as it can be constructed on that line, so theorem 133 is potential in Frege's definitions within the context of the rules of logic insofar as it can be derived from those definitions, though not solely according to those rules. Because the concepts of fully modern mathematical practice are inferentially articulated in the sense of containing inference licenses as part of their contents they, at least some of them, have the potential to be used as such in a proof, and this means, as Frege says, that what may be inferred from them cannot be inspected in advance. Not only the

[35] Reason does have its own concepts in a sense on Kant's account but because nothing corresponding to those concepts of reason, as Kant understands them, can possibly be met with in experience, those concepts do not properly speaking provide content that is proper to reason.

definitions we formulate and the conclusions we draw but even the proofs themselves, we have seen, can be intelligible unities, unities that are necessary without being essential, without, that is, being such that their wholes are prior to their parts. And, I have already suggested, supplementing *Begriffsschrift* with primitive signs of arithmetic to enable the formulation of the contents of other concepts of mathematics would not significantly change things. In particular it would not render either the contents or the reasoning any less pure because and insofar as those primitive signs and those concepts are likewise wholly inferentially articulated. Although we begin, and must begin, with essentially sensory conceptions of concepts in early mathematical practice, over the course of history that sensory content is progressively stripped away until eventually we achieve concepts of reason, what Kant calls ideas. We could not begin with such concepts but we can through our intellectual work, our strivings for truth, eventually achieve them. And unlike such ideas as they appear in Kant's philosophy these ideas are fully contentful. They provide the subject matter for the science of mathematics.

The power of pure reason as a power of knowing is a fallible capacity for judgment and inference regarding the inferentially articulated concepts of mathematics and logic. It is a priori in the sense that in order to know in mathematics (as in philosophy) one cannot rely on testimony, either the testimony of one's own senses or that of another person; one must recognize for oneself, through the use of one's own reason, that a thought is true. One must acknowledge the thought as true in an act of what we have called (in section 4.5) expressive freedom, and except in the case of an identity judgment corresponding to a definition one can do this only by way of an inference from something the truth of which one acknowledges by means of a rule that one likewise recognizes to be valid. And one can be mistaken on either or both counts. If one is not mistaken, if all the relevant concepts have come to be known in their pure form through one's grasp of senses that clearly display their inferentially articulated content, and if one has a correct proof, that is, has performed a valid series of inferential steps from the starting points to the theorem, then one's acknowledgement of the truth of that theorem puts one in a cognitive relation of knowing to the truth in question. There is thus both an active and a passive moment in the case of such judgments. The moment with respect to which one is passive is the moment of truth. The thought that one aims to acknowledge as true must actually be true for the judgment, the acknowledgement, to succeed. But the truth of the thought, although necessary, is not sufficient for knowledge. The potential of the thought to disclose an aspect of reality is fully actualized only in one's act of acknowledgement. One must do something, draw the relevant inferences, if one is to achieve knowledge of a theorem in mathematics, or logic. One cannot know what is not true any more than one can see what is not there, but nor does one know what one only entertains even as very likely to be true. So long as what one has is only a conjecture one has no knowledge. In mathematics, and in logic, knowledge is achieved by a course of inquiry, and in particular, a (successful) proof, starting with adequate, fruitful definitions.

To realize that the science of mathematics neither has nor needs any given foundation on which to build is to recognize that it is not the products of this science but instead the practice of it that holds the key to an adequate understanding of how this science works as a mode of inquiry. It is the activities of inquiry—the analysis of concepts, the formulation of definitions, the axiomatization of the rules of inference involving the primitive notions, the formulation of theorems and their proofs (whether or not any of this is done in a mathematical language such as Frege's)—that provide the checks and balances that, although they do not guarantee that one will get things right, enable one to improve one's grasp of concepts and correct one's proofs. Because this practice is constitutively self-correcting through the processes of proof and refutation, and judgment is an act of acknowledgement understood as an exercise of expressive freedom, the power to make manifest the otherwise latent structure of reality, successfully to make up one's mind through this critically reflective activity of inquiry is to come to be in a cognitive relation of knowing to the relevant bit of reality. And no matter how confident one is that one has gotten things right, it can always turn out that one was mistaken. The power of pure reason as a power of knowing is, like any power, fallible. It can get things wrong. But it can also get things right, that is, come to know how things are in reality. In mathematics the reality in question is not of course empirical reality, how things actually are, but instead how they could be. For reasons that will become clearer in Chapter 9, the concepts of mathematics, insofar as they are concepts of pure reason, are concepts of how empirical reality can be. As employed in fundamental physics they reveal how physical reality most objectively is, the same for all rational beings. The concepts of mathematics (and logic) are not, then, in the world in the way that the entities fundamental physics studies, including even the space-time of general relativity, are in the world (though if they were, the conception we are after here would be correctly described as a form of Platonism).[36] Nor is it correct to describe them as mental entities, though we can, through our grasp of the relevant senses, come to be in a cognitive relation of knowing to them. They are fully objective insofar as they are the possible ways things could most objectively be, the ways any rational being might in time come to recognize as the possibilities of things.

We have our account of how reason, *pure* reason, is realized as a power of knowing over the course of our intellectual history. And I have suggested that this account further enables us to understand how reality, the same reality that is investigated in fundamental physics, is revealed in our everyday perceptual experience. We can now clarify this further by reflecting again on the notion of a power that was invoked in Chapter 1. A power, we said (following Rödl), is a fallible capacity to achieve some

[36] Compare Frege in *Grundlagen* (1884, sec. 87): "in the external world, in the whole of space and all that therein is, there are no concepts, no properties of concepts, no numbers. The laws of number, therefore, are not really applicable to external things; they are not laws of nature. They are, however, applicable to judgments holding good of things in the external world: they are laws of the laws of nature."

end, for instance, in perception to take in, *see* how things are. Powers, so conceived, are successfully exercised only in propitious circumstances. The ancient Greeks could not quite get this notion of a power into view, at least for the case of knowing, and they could not because they understood the unity of the knower and the known (perceiver and the perceived) as an essential unity the parts of which are intelligible only in relation to the whole. Because the relationship of knower and known is in this way constitutive, according to the ancient Greeks, it is unintelligible that one might fail, even if only on occasion, to get things right. As Burnyeat explains,

> the problem which typifies ancient philosophical inquiry in a way that the external world problem has come to typify philosophical inquiry in modern times is quite the opposite. It is the problem of understanding how thought can be of nothing or what is not, how our minds can be exercised on falsehoods, fictions, and illusions. The characteristic worry, from Parmenides onwards, is not how the mind can be in touch with anything at all, but how it can fail to be. (Burnyeat 1982, 33)

The notion of a power, say, of seeing or knowing, cannot fail on this conception. It is not through an exercise of the power of, say, sight that one misperceives; the power as a power of seeing is infallible on the ancient Greek view. The point is not merely that *seeing* is inevitably successful, as of course it is because if unsuccessful then one does not see but only seems to see. It is that the *power* to see is inevitably successful. Just as it is not qua doctor that one misdiagnoses a patient so it is not qua perceiver that one misperceives. The power is not fallible as it is on Rödl's account, and it is not because the unity of knower and known, perceiver and perceived, is taken to be an essential unity.[37]

Where ancient thinkers begin with the successful case conceived as an internal constitutive relation between the knower and the known, and then have no way to understand the nature of error, early modern thinkers take mind and world to be only accidentally related. And now it is impossible to understand not the fallibility of our powers of knowing but instead the *efficacy* of those powers, the fact that despite their fallibility they are *powers*, and indeed powers of *knowing*. Knowing requires that there be an internal, non-accidental relationship between the knower and the known, but if mind and world are only accidentally related then there is and can be no such relationship between them. Indeed, the practice of science seems, at this dialectical moment, to *show* that the relationship is only accidental insofar as the project of science is to understand how things are *independent* of the contingencies of their appearance to us as the particular sorts of animals we are. But again, if mind and world, knower and known, are only accidentally related then there simply cannot be any such thing as a *power* of knowing.[38]

[37] Heidegger (1982, 65) describes this as an erroneous objectivizing that is based on our naïve and natural understanding of things, the mistake of taking our intentional directedness on reality to be an "extant relation."

[38] This mistake is, correspondingly, an erroneous subjectivizing of intentionality, according to Heidegger (1982, 64), the mistake of holding that the intentional structure of comportments is something immanent in the subject.

What is needed to achieve a stable conception of the (fallible) power of knowing is precisely the further revolutionary transformation we have been concerned with here, the realization that our powers of knowing are constitutively mediated by the Fregean, inferentially articulated senses that we grasp with our grasp of language, whether natural language or a sufficiently advanced mathematical language. Because *all* awareness (more exactly, all self-conscious awareness, awareness of the sort we rational animals enjoy) is ineluctably mediated by Fregean senses—whether our awareness of reality as it shows up for beings sufficiently like us (as mediated by natural language) or our awareness of reality as it would show up for any sufficiently advanced rational animal, whatever its form of life (as mediated by mathematical language)—we can understand *both* how we can fail sometimes to get things right *and* how we can succeed in getting things right. But to understand this, we have seen, one needs to have in view both sorts of language, *and* to have a clear understanding of their similarities and differences. In particular, we need to see that in the case involving natural language veridical perception has a kind of primacy over illusory perception. In virtue of our acculturation into natural language we just do have the power to take in how things are, though we can, in unpropitious circumstances, be mistaken. In the exact sciences we do not just see how things are but instead must judge all things considered, self-consciously and critically reflect in the course of our on-going investigations in order to make up our minds on a matter. And here, because what we are after is not the truth of our everyday lives but fully objective truth, which is the same for all rational beings, it is our failures that have a kind of primacy. And because they do our powers of self-correction are in this case much more significant and far-reaching. In this case there really is a sense in which *everything* can turn out to have been wrong. Indeed, this is just what happened when in the nineteenth century the science of mathematics was *reborn*, raised anew as a purely conceptual enterprise. We can make sense of this because (as we now know) mathematics has from the beginning involved what we can now recognize to be (internally) inferentially articulated Fregean senses on the basis of which to reason in mathematics.

Neither the ancient Greek conception of knower and known as forming an essential unity nor the early modern alternative in terms of an accidental unity will do. The unity of knower and known is instead yet another instance of an *intelligible* unity. It is a whole of parts that is nonetheless not reducible to its parts. Much as a side of a triangle in Euclidean geometry is *at once* a straight line and hence intelligible independent of any reference to triangles *and* a side, which is not so intelligible, so a knower is *at once* an embodied being, a body among others that is intelligible independent of any reference to what it knows (or does not know) *and* constitutively minded, always already in a cognitive relation of knowing to reality. We *are both*, both bodies (certainly not bodies together with something else in virtue of which we are minded) *and* self-conscious rational beings, that is, knowers.[39] And we need to keep both in view together, as aspects or facets of *one* reality, if we are adequately to understand our fallible powers of knowing. We can understand the successful exercise of our powers of knowing only by focusing on the irreducible unity that is the knower and the known. But to understand the failures of those same powers, the fact that they are inherently fallible, we must attend instead to the independently intelligible parts. It is, finally, by attending to the dialectical history we have been concerned here to trace, first and foremost in mathematics, that we learn explicitly and self-consciously to do both these things together.

We have learned that the realization of reason as a power of knowing is and can only be achieved in the course of our intellectual history, through our own "immense intellectual effort" as Frege (1884, vii) describes it. Much as nothing could be born a rational animal but instead can become rational only through its acculturation into an on-going, living (natural) language, so no rational animal has reason as a power of knowing save by a course of extensive study within a sufficiently advanced intellectual tradition. One needs not only to be born into such a tradition and acculturated into its natural language but also to become educated; one must explicitly and intentionally learn to do mathematics, and ultimately to do the sort of mathematics that is now the norm in the discipline, if one's own reason is to be fully realized as a power of knowing. And for just the same reason, we can fully understand this practice, and in particular how it is that pure reason can be a power of knowing, purely rational and yet fully contentful, answerable to something objective, only if we have that history in view. Our autonomy conceived independently of our historicity cannot provide what is wanted. Pure reason as a power of knowing is by its nature an *achievement* of reason both in the sense that it is achieved by the exercise of reason in the course of our mathematical investigations over the course of history and in the sense that it is through this historical process of intellectual transformation and growth that reason itself is fully achieved, its potential as a capacity of knowing made fully actual. Reason realizes itself through its self-activity as reason. The realization of reason is self-actualization.

8.6 Conclusion

An adequate philosophical understanding of the science of mathematics is possible only in the light of its historical development because the practice of mathematics as the purely rational enterprise that it has been revealed to be is possible at all only as the very late fruit of a historical process of discovery and intellectual maturation, one that at once builds on and transforms all the mathematics that had gone before. And as is characteristic of essentially historical processes generally, we can fully comprehend those earlier practices only in light of what mathematics eventually

[39] Compare Rödl's (2007, 131) conception of "a true materialism, which conceives material reality not only as an object of intuition, but as human spontaneity."

comes to be, in its full and final realization as the rational practice it is. But even this is not enough, as we have seen. If we are to understand current mathematical practice, we must see it at work, see the modes of reasoning it involves and the nature of the concepts with which it is concerned. And in order to do that we need a system of notation of the sort Jourdain describes, a system of signs capable of embodying those modes of reasoning. We need a *Begriffsschrift*, which, we now know, is just what Frege provides. We can understand current mathematical practice, the nature of that practice as a science, but only by attending to reasoning in the system of signs that Frege provides.

As Frege shows by example in his derivation of theorem 133, a strictly deductive proof can nonetheless be ampliative, a real extension of our knowledge. But such a proof is ampliative only because it is part of a larger system, one that conforms to the Aristotelian model of a science. At least in mathematics, the idea of self-correction is intelligible only in terms of the Aristotelian model. And now we are in a position to see something else as well, how our understanding of this capacity for self-correction also undergoes subtle transformations over the course of history. We know that the capacity for self-correction, for second thoughts as contrasted with mere changes of mind, belongs to us as the rational animals we are. To be rational just is to have the capacity for second thoughts. What is much harder to see is that it is this capacity that is *constitutive* of the rationality of inquiry in the exact sciences. Kant already had the basic idea but only with developments in mathematics in the nineteenth century can we see that in mathematics as much as in natural science is this true. It is only with those developments that our power of reason is finally realized as a power of knowing *because* it is a self-correcting power, a power of rational and critical reflection that can call *anything* into question as reason sees fit, where this furthermore requires the degree of cognitive control that is made possible only because and insofar as one's systematic work approximates the Aristotelian model of a science. It is by making explicit the contents of one's concepts, as far as one understands them, the basic inference patterns on which rest all the other modes of inference at one's disposal, and one's path of reasoning from those concepts to theorems, that one is put in a position to discover one's errors and so to correct them, and thereby to achieve knowledge of things as they are, the same for all rational beings.

Over the course of this study, we have distinguished three different modes of our intentional directedness. Our first, and most basic, intentional comportment is that of perceptual immediacy as exhibited in our capacity to take in manifest facts. Objectivity, to this way of thinking, consists in the distinction between an object and one's experience of that object, a distinction that is grounded in the fact that natural language is at once object involving and inferentially articulated. With Descartes' new mathematical practice a new sort of intentional involvement is realized, though it is one that is hardly intelligible as such. The idea of objective knowledge as an achievement of reason and the expression of our freedom makes its first appearance but it cannot be sustained because it is as yet wholly unclear how reason could possibly be answerable to what is. Kant sees that knower and known must be internally related, but not how they can be. For him the unity of knower and known (that in our perceptual experience is a pre-established harmony grounded in the nature of natural language) must be constituted in a transcendental synthesis.

Objectivity on the early modern view lies in the God's eye view, the view from nowhere as it contrasts with the view from here. For reasons that were rehearsed in Chapter 4, our knowledge, by contrast with the knowledge of an infinite being, can only be of things as they appear to us. Developments in mathematics in the nineteenth century together with Frege's logical advances provide us with yet a third mode of intentionality, that of a fully self-conscious mediated immediacy. Although the mathematical languages that we develop, and the mathematical conceptions we form in these languages, are our own historical products, they are (or at least can be) nonetheless revelatory of fully objective features of reality. Objectivity does not lie in the God's eye view, the view from nowhere. Instead objectivity is what is the same for all rational beings. It is an achievement of reason. With developments in mathematics in the nineteenth century, developments that were often self-consciously aimed at uncovering the conceptual foundations of various parts of mathematics, mathematical thinking succeeds in breaking free altogether of the "bounds of sense." In becoming explicitly conceptual, through reason alone, mathematics becomes pure not only in the sense that it has always been pure, that is, a priori, but also in the sense of being uncontaminated by the contingencies of our biology and social and cultural history. The concepts of mathematics as it is currently practiced are concepts that *any* sufficiently advanced rational being, *whatever* its biological endowment and social and cultural history, could discover and understand. In a fully achieved modernity, objectivity is what is the same for all rational beings. This is the truth in logicism: not that mathematics is merely a more developed logic but that it is a purely rational enterprise, the work of pure reason.

9

The View from Here

We are each of us born mere animals and become rational, capable of critically reflective thought and of knowledge, only through our acculturation into a socially evolved tradition, a distinctively human form of life. Becoming rational, we also now know, is by the same token to come to have the world in view. And the world as we first find it is rich with significance for us, an organic, meaningful whole within which we have our rightful and natural place. But the world so experienced cannot satisfy the demands of reason. Although we are at home in the perceptible world of change and becoming, our knowledge of this world seems, on reflection, to be somehow partial and essentially perspectival, not a view of things as they really are. Insofar as it is knowledge of merely conditioned being, our everyday experience of the world points inexorably beyond itself to something unconditioned, to things as they are in themselves, the same for all rational beings. And thus a new narrative, that of reason's own becoming, is begun.

This narrative of the achievement of reason, its two and a half millennia long maturation into what is properly a *power* of knowing, is what we have traced here. It is a story of discovery and growth, revolution and transformation. But it is also a story of failure, in particular, of the failure of recent philosophy to see what was before it in the world-historic shift in mathematical practice in the nineteenth century, which includes also Frege's mathematical practice, and in fundamental physics in the twentieth. We have told already of the profoundly transformative developments in mathematical practice in the nineteenth century, of Frege's extraordinary project of providing a mathematical language for this new form of mathematical practice, and of at least some of the fundamental philosophical lessons about the nature of language and its role in our cognitive involvements in the world that we need to learn from Frege's work. What we have yet to see, even in barest outline, is the role of twentieth-century developments in physics.

As Frege's work, and in particular the language that he developed within which to reason deductively from concepts, enables us to realize, the transformation in mathematical practice that took place over the course of the nineteenth century realizes a new shape of spirit, the standpoint of pure reason. This standpoint supersedes the sideways-on view of early modernity, and it does so through the realization that the sideways-on view constitutively involves a confusion of two different distinctions, that between *Sinn* (sense) and *Bedeutung* (signification or meaning), on the one hand, and that between concept and object, on the other. So long as these two distinctions are not distinguished but instead conflated—as, we have seen, they are in Kant's distinction of concept and intuition—it will inevitably seem that all cognitive significance and hence all awareness is "inside," while "outside" is brute, merely causally efficacious physical reality. It will seem that our self-conscious experience of things is due to the causal impacts on our sense organs, and ultimately our brains, of physical stuffs such as light waves and moving particles. Once we have learned to distinguish these distinctions, that conception of our being in the world simply falls away.

And if we further recognize that although the *Sinn*/*Bedeutung* distinction applies to language as such, that is, to any medium through which reality is disclosed to a rational being, the concept/object distinction instead marks the essential difference between a fully realized mathematical language, on the one hand, and a natural language, which is socially evolved, essentially sensory, narrative, and object-involving, on the other, then we can begin to understand how and why we need to recognize both of the modes of intentional engagement that they enable. To achieve the standpoint of reason is in this way to *recover* a sane conception of our everyday being in the world, to understand—as, in their way, the ancient Greeks already had—that we, as the rational animals we are, have the capacity to perceive and thereby to know things in the world as we first find it. The one significant difference between what we now know of our everyday being in the world and what the ancient Greeks already knew is that we recognize as they could not that this mode of being is not a biological endowment, something we might share with other animals, but instead essentially social, cultural, and historical, that we become self-conscious rational animals only through our acculturation into an evolved public (natural) language.[1] And we can know this because we, but not the ancient Greeks, have and know ourselves to have also another mode of being in the world insofar as we have the capacity to take up the standpoint of pure reason, to think and so to know things in the world as they are disclosed through the medium of a self-consciously achieved mathematical language.

Having learned to distinguish the *Sinn*/*Bedeutung* distinction from that of concept and object, we can finally understand how language is the vehicle of our cognitive involvement in the world, how it is that language at once gives us the eyes to see and by the same token the world, reality, a face by which to be seen. But there are, we now know, two very different ways this can work, through the medium of natural language and through the medium of a fully realized mathematical language. The view of reality afforded by natural language, at least by the natural languages of the sort of rational animal that we in fact are, is simply that of everyday life, a view of living beings with their natures and powers and of perceptible things more generally.

[1] It goes with this that we recognize as the ancient Greeks could not that independent of the emergence of living things there is no significance at all to inanimate physical nature.

The view of reality afforded by mathematical language is more complex. What mathematical language directly discloses are concepts, concepts of objects, of properties, relations, and functions, and finally, concepts of mathematical structures, of whole systems of (kinds of) objects in particular sorts of relations. It is these concepts that are deployed in turn in twentieth-century fundamental physics. Through its work of clarifying the senses of concept words and thereby providing us cognitive access to the concepts those words signify, the science of mathematics provides the language that can serve in turn as the medium of the physicist's cognitive engagement with physical reality.

With the rise of early modern mathematics and science we learned to conceive both ourselves and the world around us in a fundamentally new way, and, so it seemed, a *better* way. The new conception both of ourselves and of the world in terms of the essentially modern notion of a law was to *replace* the ancient conception. Everyday perceptual experience, which had hitherto been understood as an actualization of our natural capacity to take in things as they are, was now taken to provide only the data for our newly realized capacity for formulating theories of how things *really* are, theories that were to explain the data that sense experience provides. To the early modern mind, things are *not* as they appear to be in our perceptual experience; experience cannot be taken at face value. Our experiences of things are *only* appearances to be accounted for by what, according to our theories, is actually going on. Not natural language but mathematical language gets things right on the early modern view, not by disclosing how things are, as everyday experience seemed at first to do, but instead by representing it. The theories we formulate in modern, Newtonian scientific practice are representations, models formulated in the language of early modern mathematics that are supposed to correspond to, provide a picture of, what is physically real.

A characteristic mark of early modernity is the thought that we must *choose* between the ancient understanding of being in terms of the notion of a form of life and early modernity's mechanistic understanding of being as a kind of clockwork, whether natural or rational. Much as Aristotle understood even inanimate matter on the model of animate beings, as having its characteristic principle of motion within it as constitutive of its nature as what it is—it is in the nature of fire to go up, of earth to go down, and so on—so we, insofar as we are merely early modern, understand animate being, in particular animal life, on the model of a mere mechanism. As Aristotle understood intellection on the model of perception so we (insofar as we are early moderns) understand perception on the model of intellection.[2] Neither, we ought now to see, will do. We do not have to choose, and we must not choose if we are fully to understand things as they are. Living beings, in particular, cannot be

[2] This either/or is furthermore inevitable at this stage in our intellectual maturation in virtue of the fact that early modern understanding arises out of a kind of gestalt or figure/ground switch in our more primordial form of understanding.

adequately understood as complex mechanisms. As we saw already in Chapter 1, a living being is *essentially* an instance of a form of life, of a species with its characteristic capacities and powers.

Indeed, twentieth-century fundamental physics seems to show that the reductive, mechanistic understanding characteristic of early modernity is inadequate even as regards inanimate nature, at least as a whole, that is, in our understanding of the cosmos, and in our understanding of the most fundamental nature of matter. Both in special and general relativity and in quantum mechanics we find, we will see, a very different kind of physical theory from that which is characteristic of Newtonian mechanics. Both in special and general relativity and in quantum mechanics, mathematics is not used to construct models of posited underlying mechanisms but instead directly discloses the essentially mathematical structure of the relevant bit of reality. And insofar as the mathematical concepts that are in play in special and general relativity and in quantum mechanics have been stripped of all sensory content, insofar as they belong to pure reason alone, reality as it is revealed in these domains is fully objective, things as they are, the same for all rational beings.

9.1 Einstein's Revolutionary Physics

To give up the sideways-on view bequeathed to us by early modernity is, among other things, to realize that it is from within the world that we know it. This is of course obvious in our everyday lives—at least it is obvious before the rise of early modern mathematics, physics, and philosophy contrives to convince us otherwise. But it is equally true, we will see, in Einstein's physics. Although in special and general relativity reality is disclosed not sensorily but to reason itself, it is nonetheless disclosed to an essentially embodied rational being, one that is manifestly embedded within the world that is the object of our scientific investigations. What Einstein's revolutionary new physics reveals, so it will be argued, is that having passed beyond the inside/outside perspective of early modernity we at once recover our everyday being in the world and fully realize our capacity to think, that is, to grasp in pure thought, the most fundamental features of physical reality, in the first instance, the nature and structure of space-time.

In special and general relativity we achieve a radically new conception of space and time, and of the ways in which matter and energy interact with space-time, one that supersedes, on the one hand, the Newtonian conception of absolute space and absolute time, and on the other, Newtonian gravitation as a force that acts instantaneously over the distances between massive bodies. Adequately to understand it, we need to remember that the Newtonian conception of space and time that is superseded by Einstein's account is itself made possible only by the profound transformations in mathematical practice first enacted by Descartes. Our everyday conception of space is not that of a container, a totality of relative positions, as it is in Newton's physics, but instead a conception grounded in our experience of objects, landmarks, and of how, as we discover in our journeys through the world, they are arranged relative to one another. And our everyday conception of time is similarly grounded in our (everyday) experience. Time passes and as it passes things come to be, grow and change, and eventually pass away. The time of our lives as the rational animals we are is, as we can think of it, narrative time within which stories, with their characteristic beginnings, middles, and ends, unfold in their own characteristic ways and inexorably toward their end. As things have their proper place according to their natures in our everyday conception of space so they have their proper time—each its own season—in our everyday conception of time. It was only with Descartes' new mathematical language, and as an integral part of that language a metamorphosed understanding of space as an antecedently given whole within which things might but need not be found, that we learned, following Newton, to understand space and time in absolute terms.

On Newton's conception of it, absolute space is a kind of container within which material objects are to be found—though, again, the container could have been empty. This conception is furthermore required, Newton argues, in order to explain the fact that although the notion of velocity, that is, of the (constant) speed and direction of an object's motion, makes sense only relative to something else assumed to be stationary, the notion of acceleration apparently does not. We *feel* acceleration (and deceleration), for instance, when, while driving, we step hard on the gas, or on the brake, or take a sharp corner. And clearly other objects, say, a bag of apples laid on the passenger seat, are affected by acceleration, and deceleration, as well: brake hard and the apples will end up on the floor. But acceleration and deceleration are only changes in velocity. So they ought, like velocity, to make sense only relative to something else. Because in this case the something else clearly cannot be chosen at random, as it can in the case of velocity, what acceleration is relative to, Newton concludes, is absolute space, from which it naturally follows that true motion and rest are likewise relative to absolute space. Of course we cannot determine the absolute velocity or rest of a thing, as we can determine at least the fact of acceleration, though not by how much, relative to absolute space. But once we recognize that absolute space is needed in any case, we know that absolute velocity and rest also make sense.

Just the same applies to time Newton thought. Although we live our lives in the ever-passing present, time, like space, is a kind of absolute container of relative temporal locations, before, during, and after. We cannot know where we are in absolute time, whether near its beginning, if it has one, near its end, if it has one, or some time in between; but we are nonetheless at some time in particular in absolute time. It is this conception of space and time as two independent and absolute relational wholes within which everything happens that would be completely overturned by Einstein, first in special relativity according to which space and time are not two but indissolubly one, and then in general relativity according to which this one space-time is a field that is continuously transformed through its interactions with massive bodies in an ever-evolving cosmic dance.

In 1905 Einstein published his special theory of relativity aimed at resolving what he saw as a difficulty with the then-standard account of electricity and magnetism due to Maxwell. Much as an object that appears to be at rest to one perceiver (the house across the street, say, which appears at rest as I look at it through my window), is in motion from the vantage point of another perceiver (say, that of an observer in space), so what appears to be an electric field from one vantage point can appear to be a combination of an electric and a magnetic field from another. Depending on the vantage point, and so on whether the system is taken to be moving or at rest, Maxwell's equations yield different answers regarding the electric and/or magnetic forces acting on the system. Conceptualized from the standpoint of early modern science, this is no more surprising than the fact that a body that appears to be at rest from one vantage point appears to be moving from another; for, as we have just seen, one distinguishes in early modern thought between absolute and relative space and hence between absolute and relative motion. Because we do not, and cannot, know our absolute location nor from which vantage point things are really moving or at rest, we can measure only relative to some inertial frame of reference, and our descriptions of a moving body will be different in different reference frames. In just the same way, and partly as a result, one's description of the forces, whether electric or magnetic or both, that are acting on a system will be different in different frames of reference. Only relative to absolute space in which the motions of bodies appear as they are in reality could it be determined which forces, whether electric or magnetic or both, are actually acting in the system. Einstein rejects the account. According to him, electric and magnetic fields are not different entities but instead are one and the same, the electromagnetic field as it shows up from different perspectives. There is no absolute space but only relative spaces, that is, various inertial frames within which to make measurements.

In special relativity there is no absolute space and no absolute time. What there is instead can be illustrated by the relation between a length of stick suspended in the air and the shadow it casts on the wall and floor.[3] Suppose, first, that the stick is suspended from one end so that its length is parallel to the wall and perpendicular to the floor. The shadow the stick casts on the wall will then be the same length as the length of the stick, and there will be no shadow cast by the length of the stick on the floor. Similarly, if the stick is suspended so that it is perfectly horizontal, parallel to the plane of the floor and perpendicular to the wall, then the length of the shadow on the floor will be equal to the length of the stick, and there will be no shadow due to the length of the stick on the wall. At intermediate positions, as we raise one end of the stick towards the vertical, shadows will be cast on both floor and wall by the length of the stick, shadows the lengths of which form the sides of a right triangle the hypotenuse of which is the length of the stick. Hence, no matter what the actual

[3] This illustration is due to Spielberg and Anderson (1987, 168).

orientation of the stick, its length can be determined by measuring the length(s) of the shadow(s) it casts on the floor and/or wall: the length of the stick equals the square root of the sum of the squares of the lengths of the shadows it casts on the floor and wall.

In special relativity, analogously, the elapsed time between two events and the distance between them, although they will appear different to observers in different inertial frames, together (like the two shadows cast by the stick) determine an invariant space-time interval between the two events. If T is the elapsed time between the two events, as measured in some one frame of reference, and L is the distance between them, as measured in that same inertial frame, then the space-time interval between them is equal to $\sqrt{(c^2T^2 - L^2)}$, where c is the speed of light and is the same in all inertial frames. Measured distance and measured time are not, then, objective in themselves; there is no answer to the question how long something really is (that is, its length in absolute terms) or how much time has elapsed (in absolute time). And the problem is not that we cannot know such facts (though there really are such facts); it is that the questions simply make no sense in the new framework. Nevertheless, like the shadows cast by our sticks, measured distance and time (in an inertial frame) enable us to determine something that *is* objective (absolute), that is, the same in all inertial frames, namely, the space-time interval.

We do not find it odd to think that what happens at different times and the same place when viewed from one inertial frame (say, within the car one is driving) will appear to happen at different places relative to another inertial frame (for instance, from the perspective of a person standing on the street watching the car pass by). But we do find it odd to think that what happens at one and the same time at different places relative to one inertial frame happens at different times relative to another. Nevertheless, this is an immediate consequence of the fact that, as special relativity reveals, the speed of light is the same in all inertial frames. No matter how fast one is moving, whether in the direction of the light's motion or directly away from it, the speed of light is, and is measured to be, precisely the same. (We will soon see why that is.) It follows that two events that happen at different places—so that no matter where a perceiver is relative to those two places it will take some non-zero stretch of time for information about those two events to reach that perceiver—will be perceived as differently temporally related by perceivers in different inertial frames. What appears to be two simultaneous events (at different places) from one vantage point, will appear to be two events one after the other, that is, at different times from one another, from another vantage point. Only events that occur at the same place and the same time will be perceived as simultaneous by perceivers in different inertial frames.

In special relativity, space and time are not distinct and separate aspects of reality but instead indissolubly one, and any object's motion through Einsteinian space-time, though from within an inertial frame will be measured to have both a spatial and a temporal component, is moving through space-time itself at the speed of light.

Hence, the faster one moves through the spatial dimension of space-time, the slower one must move through its temporal dimension. And at the limit, for the case of something moving through space-time at the speed of light, that thing must be at rest with respect to time; because all its space-time speed is being taken up within the spatial dimension, there is none left to enable it to move through time. Time does not pass for something traveling at the speed of light. And that is why the speed of light is measurably a constant no matter what one's motion relative to it. As one's own motion relative to that of light increases, one's passage through time slows, and as a result the measured speed of light is from one's own perspective the same as it would be if one were not moving at all relative to the light. Again, because one's total motion through space-time is always at the speed of light, the faster one moves through space, the slower is one's passage through time.

To our everyday way of thinking, objects have definite shapes, dimensions, and speeds. There is a fact of the matter how large a thing is, or what the distance is between two things, and a fact of the matter whether a thing is moving or at rest, and if it is moving, how fast it is moving. On the Newtonian view, there is again a fact of the matter what the dimensions of a thing are, and a fact of the matter whether a thing is at rest or moving, and if so how fast. But these facts are not, for the Newtonian, discoverable from our limited perspective. We can measure only from within some inertial frame, which may or may not be moving relative to absolute space. Our view is only the view from here. It is the God's eye view, the view from nowhere, that is the perspective from which things show up as they *really* are. Einstein's account is very different. According to him, the relative, which we can measure, does not *contrast* with something absolute, which we cannot measure, but is rather *that through which* we have access to what is absolute, that is, invariant. There is, on Einstein's theory of special relativity, no absolute space and no absolute time but only (absolute) space-time. It follows directly that there is no God's eye view to oppose to our own. Our view, the view from here, *is* the view from nowhere, according to Einstein. It is as embodied knowers—limited, finite, perspectival beings in the world—that we know things as they are, the same for all rational beings. It is from *within* reality as it shows up for us finite, perspectival, embodied and embedded beings that reality is knowable and known, not from some mythical outside, or sideways-on, or God's eye, view.

On Einstein's account—according to which a measurement of a space interval together with a measurement of a time interval gives, by way of a simple calculation, a space-time interval that is the same in all inertial frames—a measurement is not a datum to be explained by a theory picturing reality but is instead itself a mode of access to reality. The measurements do not themselves provide what is wanted; by contrast with our everyday measurements, they do not immediately disclose an aspect of the world. But through a simple calculation they do provide a mediated access, not in the sense of data for a theory but immediately. The measurement itself gives us what we want, access to what is, the same for all rational beings whatever the inertial frame they find themselves in. Much as Frege helps us to see that our cognitive access to reality is in every case mediated by a sense, and that different senses can disclose one and the same objective entity, one and the same *Bedeutung*, so Einstein helps us to see that our access to the bit of reality that is a space-time interval is in every case mediated by a kind of sense, a facet of the real that we grasp through measurement and calculation, and that different "senses," that is, different measurements and calculations in different inertial frames, can disclose one and the same objective reality. What to our first mode of intentional directedness seemed immediate, the capacity directly to measure both space and time intervals, and to our second mode of intentional directedness seemed ineluctably mediated, merely our limited perspective on things, is revealed, in Einstein's special relativity as in Frege's *Begriffsschrift*, as a mediated immediacy, a mode of access to the real but one that is essentially mediated.

In special relativity, electric and magnetic forces are similarly revealed as facets of something fully objective, namely, the electromagnetic field. As Lange explains:

> there is no fact about whether the magnet is moving and the wires are at rest or vice versa. There is no fact about whether both electric and magnetic fields are present, or only a magnetic field. There is no fact about whether the forces causing the current to flow are electric or magnetic. Rather, the only facts are the *relative* motions of the magnet and wire, as well as the existence of an electromagnetic field characterized by the invariant quantities . . . involving both ***E*** [electric field] and ***B*** [magnetic field]. Seen from different reference frames, the electromagnetic field appears as different combinations of ***E*** and ***B***. (Lange 2002, 191)

Just as it unifies space and time, showing them to be merely aspects of one and the same entity, so Einstein's theory of special relativity unifies electric and magnetic forces by showing that they are aspects of one and the same entity.

Einstein's was not merely a new theory about the nature of space and time, and about electric and magnetic forces. It was a new kind of theory, one that Einstein, in 1919, described as a theory of principle as contrasted with a constructive theory. A paradigm example of a constructive theory, according to Einstein, is the kinetic theory of gases, which "seeks to reduce mechanical, thermal, and diffusional processes to movements of molecules" (Einstein, quoted in Brown 2005, 71). According to this theory, when gases appear to expand on being heated, or to exert greater pressure on having their volume reduced, what is in fact going on is that little particles are bouncing off each other, and the walls of the container they are in, with increasing rapidity. Such a theory is a paradigm of Newtonian reductive, mechanistic scientific theorizing. One explains the appearances by positing an underlying reality of particles and forces acting on them. A theory of principle is very different insofar as it does not proceed by positing a theory of (underlying) entities together with the forces acting on them to explain the appearances, the data, but instead begins with some overarching universal principle from which the account of physical reality is to be deduced. The paradigm for Einstein of such a theory is that of thermodynamics, which begins by assuming that certain types of perpetual motion machines are physically impossible and from there deduces the theory.

Einstein's theory, unlike earlier constructive theories, is a theory of principle, one that begins with two assumptions—that the speed of light is the same in all inertial frames and that the laws of physics hold in all inertial frames (Cushing 1998, 232)—and then determines what follows. We can furthermore discern the overall orientation of this new form of physical practice in the way Einstein re-conceives certain well-known equations, the Lorentz transformations, not as (empirical) descriptions of phenomena that are then to be explained by some constructive (reductive, mechanistic) theory but as themselves directly disclosing objective reality. As Friedman (2001, 63) puts the point:

> Einstein's revolutionary move lies in his *interpretation* of these [Lorentz] transformations—as not simply representing special dynamical properties of electromagnetically constructed objects as they move relative to the aether [as in the Lorentz-Fitzgerald theory], but rather as constitutive of the fundamental geometrical-kinematic framework of what we now call Minkowski space-time. For Einstein, we might say, the Lorentz transformations change from being properly empirical laws (that is, dynamical laws) governing a particular variety of force (electromagnetic force) to geometrical-kinematic constitutive principles articulating a radically new space-time structure—a space-time structure which, from a classical point of view, is simply incoherent.

From within the Newtonian block universe—that is, taking the Newtonian conception of space, time, and motion for granted as the background for our empirical investigation—we discover, by making the appropriate measurements, that the speed of light appears to be the same in all inertial frames. This is, on the Lorentz-Fitzgerald view, merely an appearance, one that is to be explained by the fact (hypothesized in the theory) that bodies in motion contract in a way that exactly compensates for their motion relative to that of light. That is, the Lorentz transformations are conceived dynamically; the lengths of moving bodies really do *change* on this view. Einstein, as Friedman notes, takes a radically different view of those same transformation equations. What they reveal, Einstein thinks, is not that the lengths of bodies change as they move relative to the motion of light, but that there is no such thing as the (absolute) length of a body. Except at the limit—in the case of the speed of light, which, we have seen, leaves no room for motion through time—the notion of motion, and with it the notion of the length of a spatial object together with that of a time interval, are all completely relativized. They are not intrinsic properties of things but instead frame relative. Nevertheless, we have seen, measurements of spatial and temporal intervals that are made from within any given frame, though they provide only one perspective on things, do reveal something invariant, namely intervals of space-time. What we had taken to be the real is now revealed to be only a shadow cast by the choice of an inertial frame within which to measure, only a facet or face of what is. Again, from the perspective that Einstein achieves the absolute is not opposed to the relative, as it is in Newtonian physics, but given through it. It is the view from here, not some mythical God's eye view, that is the view from nowhere, the same for all rational beings.

It is furthermore worth remarking that Einstein seems quite self-consciously (though of course not in quite our terms) to adopt the standpoint of reason in the reflections that led him to special relativity. He remarks in the opening sentence of his famous 1905 paper on relativity "that Maxwell's electrodynamics—as usually understood at present—when applied to moving bodies, leads to asymmetries that do not seem to attach to the phenomena" (quoted in Lange 2002, 190); and as Einstein would later explain, "the thought that one is dealing with two fundamentally different cases [in electrodynamics as classically understood] was for me unbearable. The difference between these two cases could not be a real difference but rather, in my conviction, only a difference in the choice of reference point" (quoted in Lange 2002, 192). As Lange (2002, 193) remarks: "it is hard to avoid concluding that Einstein regarded these asymmetries as supplying a powerful *argument* against what was then regarded as conventional wisdom." But if they were, then (as Lange indicates) it would appear that the standards of argumentation had somehow shifted insofar as those same asymmetries had not, for Einstein's predecessors, provided any such argument. Unlike his predecessors, Einstein was not concerned only to "save the phenomena" but also to satisfy the demands of reason itself.[4] He does not begin with the various observations and look for an underlying mechanism to explain the appearances, as Lorentz had, but instead begins with a demand of reason, the demand that symmetries be respected: if there is no asymmetry in the phenomenon, there should be no asymmetry in the account that is to explain it. But of course nothing in the phenomenon demands this. The symmetry demand, like its close cousin the principle of sufficient reason, is instead a demand of pure reason—only this time, unlike earlier appeals to the principle of sufficient reason invoked in the context of early modern modes of thought, the demand pays off.

According to special relativity nothing can move through space-time faster than the speed of light. But if so then Newton's theory of gravity, according to which there is instantaneous action at a distance, must be wrong. So, around 1907, Einstein began to search for a new theory of gravity; and he would find it with the realization that gravity and acceleration are one and the same phenomenon, that what classical physics had understood as a mere coincidence—that the measured effects of gravity and of uniform acceleration are indistinguishable—in fact reflects a fundamental feature of reality. Space-time, according to general relativity, is "curved" by massive bodies and its curvature in turn affects how bodies move through it: "Space tells matter how to move. Matter tells space how to curve" (Misner, Thorne, and Wheeler, quoted in Brown 2005, 150). And unlike Newtonian gravity, which acts instantaneously,

[4] This same requirement of satisfying the demands of reason, we saw in section 5.2, is evident in the orientation of nineteenth-century mathematicians such as Riemann and Dedekind.

the rate at which space-time is warped by matter is the speed of light. There is no instantaneous action at a distance, but only the rippling effects of massive bodies on the curvature of space that happen at the speed of light. Thus, although in special relativity space-time is absolute and immutable, in general relativity its shape is constantly being changed by the massive bodies in it. As we are now to see, space-time, with its variable curvature owing to the presence of mass and energy, just is the gravitational field.[5] Special relativity is merely a special case, that of an empty cosmos in which the gravitational field is everywhere zero.

The space-time of general relativity is an evolving structure (field) that is disclosed through Einstein's field equations. And those equations have led in turn to a revolution in our understanding of the life of the universe, one according to which it begins with a "big bang" and is subsequently characterized by the expansion of space-time, that is, the gravitational field. Even the whole cosmos, it now appears, has a story to tell, one that has a beginning, a middle, and an end. There are on this account no forces acting on bodies from the outside such as one finds in Newtonian constructive theories. Indeed, there are no "physical correlates" of any kind but only mathematically described structures. Einstein's theory is not a model or representation of reality. Instead, the mathematics of Einstein's relativity directly discloses to pure thought a fundamental aspect of reality, namely, the space-time field that is the cosmos.

9.2 The Quantum Revolution

Einstein's theory and practice, I have suggested, accord with the new mode of intentional involvement, and in particular, the mediated immediacy, that Frege helps us to see is characteristic of the mathematical practice that emerged in the nineteenth century. Much as mathematicians such as Riemann were engaged in a new conceptual and deductive form of mathematical practice in place of the computational, algebraic methods of Euler and others, so Einstein was engaged in a new sort of practice of physics. And it is one that does not merely use mathematics in theorizing about particles and forces underlying the appearances (as, for example, the kinetic theory of gases does) but instead provides a mathematical account of the structure of the whole of space-time. The situation in quantum mechanics is much less clear, and our discussion of it is, correspondingly, much more tentative, exploratory, and incomplete. The aim is *not* to provide any sort of an account or even the beginnings of an account of what is going on in quantum mechanics, but only to reflect a little, in light of the account we have developed, on central features of some

[5] This field can, furthermore, be thought of as essentially like any other field, say, the electromagnetic field. What we had historically thought of as two quite distinctive sorts of entities, namely, space and time, are now to be seen instead as a (unified) field essentially like other fields that are revealed in fundamental physics. (See Brown 2005, 159–60.)

approaches currently being explored and to provide some indication of a possible alternative.

On the conception of scientific practice that is the legacy of early modernity, fundamental scientific advances tend either to reveal hitherto unknown particles and forces that underlie and explain something already known or to expand the purview of our vision so as to embrace ever-larger blocks of physical reality. Although we start with the view of "middle-sized dry goods" afforded by our everyday being in the world, we acquire in the course of our scientific investigations the capacity both to "zoom-in" on ever-smaller bits of reality, out of which the bigger bits are taken to be composed, and to "zoom-out" on ever-larger expanses of the cosmos, as if seen from further and further away. In neither direction is there any prospect from within the practice, save for those provided by various practical constraints, for the process to reach a natural end. If a stable and intellectually satisfying end is to be reached, in either direction, that will only be because a fundamentally *different* sort of account has been provided. Einstein's theory provides a different sort of account on the "zoom-out" side insofar as it eschews the sideways-on view and takes up instead a standpoint within the reality it aims to know. Einstein's practice in physics is that of a limited, spatiotemporally situated observer; as it reveals, it is the view from here, not some mythical God's eye view, that is the view from nowhere, the same for all rational beings. Quantum mechanics similarly, we will see, can be understood as providing something analogous on the "zoom-in" side, not an account of yet smaller and smaller particles conceived as the ultimate, or at least more ultimate, building blocks of material reality but a new kind of theory aimed at providing an adequate mathematical description of material reality.[6]

In quantum mechanics, an isolated physical system such as an electron is characterized by a state function that provides the probabilities of the results of observations, that is, of measurements that can be made on the system. If no measurement is made then changes in the state function are continuous and deterministic, and are given by Schrödinger's equation. If a measurement is made, say, of the position of some particular electron, then the wave function characterizing that electron "collapses" at least in the sense that now, for our system, the probability of its being in the position we found it in is one, and the probability of its being in any other location drops to zero. And from now on, so long as no further measurements are made, this probability remains fixed as the wave function continues to evolve according to the (revised) Schrödinger equation. The mathematics, the Schrödinger equation, provides only predictions, probabilities that the system has this or that

[6] Dirac was among those recognizing the inherent limitations of the classical, mechanistic and reductive, approach to determining the ultimate structure of matter. His understanding of the alternative was in terms of the notion of size in relation to our capacity to observe without compromising that which is observed: "We may define an object to be big when the disturbance accompanying our observation of it may be neglected, and small when the disturbance cannot be neglected" (quoted in Cushing 1998, 297). One can of course take the general point without also embracing Dirac's particular way of conceiving it.

feature. These predictions have nonetheless proved extraordinarily accurate. No one has any doubt that the mathematics is right, at least as far as it goes. Even in cases in which we have so far been unable, for practical reasons, to test some prediction of quantum mechanics, it is universally assumed that the prediction will be borne out. Quantum mechanics is the most spectacularly successful theory in the history of science. It is also the most puzzling.

The most basic puzzle is due to the form the equation takes, the fact that it provides only probabilities. When Schrödinger first formulated it, he interpreted it classically, as a description or representation of some actual spread-out, waving stuff. But that seems not to be right insofar as measurement never reveals such stuff but only *one* of the possibilities given by the equation. According to Max Born, what the mathematics provides is instead what we can think of as a probability wave, one whose peaks and troughs give not the reality itself but instead the probabilities of the outcomes of various sorts of measurements. There is, furthermore, reason to think that unlike other appeals to probability—grounded in our practical inability to work out the actual outcome on the basis of given initial conditions, say, in predictions of whether it will rain tomorrow—the probabilistic character of the wave equation is fundamental, that it in some way reveals, or at least reflects, the true state of things. It is as if, before the measurement is made, the system is in a superposition of states, of all possible outcomes of measurement, that it really is not, before the measurement is made, determinately in one or other of the states that measurement might reveal.

The most famous experiment to show this is the double-slit experiment in which particles, say, electrons, are one by one fired at a barrier with two slits in it beyond which is a detector. As the mathematics predicts, the frequency distribution of hits on the detector is not the sum of the distributions that would result if each slit were opened separately but instead that of a wave going through both at once and thus giving rise to a characteristic interference pattern. The mathematics predicts, that is, not two bands on the detector, one for each slit, but instead an array of bands such as would be produced by a stream of waves going through the two slits. Although the particles are shot individually and each hits the detector at one particular location, over time, as many hits are made, the detector shows an interference pattern that is typical not of particles but of waves. This interference pattern strongly suggests that the probability wave that is the Schrödinger equation is somehow real, not merely a reflection of our ignorance as in standard appeals to probability.

As Heisenberg showed, the mathematics also predicts a kind of uncertainty, that certain features of the particles are coupled such that knowing, by measuring, about one of them precludes knowing about the other. Indeed, the mathematics tells us, the more precise one's knowledge is of the one the less precise must be one's knowledge of the other. It is not that we discover empirically, on measuring, that measurement somehow changes the system, that measuring one aspect, say, the velocity of the particle, tampers with the particle so that now some other feature, say, the position of the particle, cannot be determined, even though before the measurement of velocity it was possible to measure instead the position. Rather the mathematics itself predicts this for such complementary features, and of course the prediction is borne out by our measurements.

Finally, and most puzzling of all, the mathematics predicts what is known as non-locality for entangled particles, and again these predictions have been borne out with spectacular accuracy by measurements. Imagine, first, that a particle is split into two particles of equal mass that fly off in opposite directions at the same speed. We can then measure the velocity of one of the two and the position of the other, and thereby apparently determine *both* the velocity *and* the position of both particles (because they are equal and opposite). One cannot, given Heisenberg uncertainty, measure both the position and the velocity of one and the same particle, but apparently it is perfectly meaningful to say that independent of being measured such a particle nonetheless has a definite position and velocity. And what this shows, Einstein, Podolsky, and Rosen famously argued, is that quantum mechanics is not a complete account of quantum reality; the mathematics insofar as it predicts uncertainty is leaving something out. It was John Bell who took the crucial next step, the one that reveals what is generally interpreted as non-locality. As he discovered, the mathematics predicts, and measurements have since confirmed, that the measurement that is performed on one of a pair of entangled particles affects, apparently, the results of certain measurements on the other—no matter how far apart the two particles are when the measurements are made. Brian Greene (2005, 114) offers this colorful imagery: entangled particles "are like a pair of magic dice, one thrown in Atlantic City and the other in Las Vegas, each of which randomly comes up one number or other, and yet the two of which somehow manage always to agree. Entangled particles act similarly, except they require no magic. *Entangled particles, even though spatially separate, do not operate autonomously*." A measurement on one particle of an entangled pair seems to affect in a quite determinate way the wave function of the other, even though it is impossible (assuming that nothing can move faster than the speed of light) that any information passed between the two. The results of measuring the two particles are correlated, as the mathematics predicts and experiments confirm, despite the fact that the particles themselves seem to be entirely separate. Before the measurements are made on the two particles, there is only some probability of this outcome or that, and yet measuring one seems to affect what one will find on measuring the other. It is as if, when a measurement is made on one particle of an entangled pair, the other "knows" instantaneously that the measurement has been made and what its result is. We can put this point in terms of collapse: when you measure, say, the spin of the one, thereby collapsing its wave function in a certain way, this instantaneously collapses the wave function of the other in just the same way. This is non-locality, action at a distance. The mathematics predicts it, and experiments confirm it.[7]

[7] There are many nontechnical discussions of these and other results predicted and confirmed in quantum mechanics that can provide more detail than is possible here. See, for example, Albert (1994), Ghirardi (2004), Price (1996), and Rae (2004).

The mathematics of quantum mechanics provides probabilities of outcomes of measurements that do not seem merely to reflect our ignorance but (somehow) what is actually going on. It is, again, as if the relevant bit of reality is in a superposition of all possible states prior to measurement, and then the wave function that characterizes this superposition collapses into one of the possible positions when a measurement is made. But if measuring causes a collapse of the wave function in this way then we need to know more exactly than we generally do just what constitutes a measurement. And as Price (1996, 200) explains, "what makes the problem so intractable is that to the extent that quantum mechanics claims to be a universal theory, applicable to all physical systems, we should expect it to be applicable inter alia to those physical systems which may be used as measuring devices." If we assume that not only particles such as electrons and photons but also objects such as detectors that are made up of such particles can be in superpositions, then it seems we shall have also to conclude either that an observer can be in a superposition of mental states or (given that we never experience such a superposition) that somehow *minds* effect the collapse, that the collapse occurs precisely at the moment when a self-conscious being comes to be aware of the result of the measurement.

There are two key assumptions in play here. The first is the assumption that quantum mechanics is a universal theory applicable to everything, that everything can be accounted for by or reduced to fundamental physics. The second is the principle of psycho-physical parallelism, the requirement that, in von Neumann's words (quoted with approval by Everett in his dissertation outlining the many worlds interpretation), "it must be possible to describe the extra-physical process of subjective perception as if it were in reality in the physical world—i.e., to assign to its parts equivalent physical processes in the objective environment, in ordinary space" (von Neumann 1955, 418). It is these assumptions that generate the measurement problem, the question when exactly in the chain of events the collapse of the wave function occurs. And once one has that problem the only way to avoid it is to deny that there is any collapse. And then all that is left to do is to decide on which side of the non-collapse one will set up camp, on the side of measurement and the mind (the instrumentalist interpretation championed by Bohr), or on the side of the wave function and the world (the many worlds interpretation first articulated by Everett).

The idea that the wave function collapses upon measurement provides a way of thinking about the relationship between the mathematics of quantum mechanics and the measurements we make. Because the mathematics is probabilistic and the measurement effectively settles which possibility is the case, we find ourselves wanting to say that what was originally in a superposition collapses into one definite state. And that, given the assumptions just outlined, leads directly to the measurement problem, which can then be avoided only by denying that there is any collapse, either instrumentally, by treating the mathematics as merely a useful tool of prediction, the measurements alone as real, or many worlds style, by treating the mathematics alone as real, the measurements relegated to the realm of subjective appearances. As should already be obvious, this whole dialectic arises only because it is assumed that the perspective of science is and must be that of the sideways-on view.

Applied to the case of quantum mechanics, the sideways-on view suggests that what is "outside" is the probability wave evolving according to Schrödinger's equation and what is "inside" is what we learn as the result of some measurement. Given this picture, the problem of the collapse of the wave function inevitably gives rise to two opposing positions each of which is motivated by the manifest defects of the other. There is only the inside and the outside so only two possibilities: either we are stuck on the "inside" or it is as from the "outside" that we see what is really going on, the appearances on the "inside" being nothing more than the effects on creatures like us of such a reality. The first is the instrumentalist interpretation according to which what we *know* is what we have measured; the wave function does not describe anything but is merely a mathematical tool for making predictions about the outcomes of measurements. Because we cannot know the state of the system except by performing some measurement on it, the question of what the state of the system is before we measure is simply meaningless. The second is the many worlds (or many minds) interpretation according to which the Schrödinger equation accurately describes reality; the appearance of collapse, the fact that measurements invariably yield only one of the possible outcomes, is taken to be merely subjective, an appearance, not really real. What the mathematics describes is a superposition of states and that is what is the case. When a measurement is made and, as it seems, the observer finds the system to be in one particular state, what has happened, according to the many worlds (minds) account is that the world, or at least the observer's mind, has split into as many worlds (minds) as there are possible outcomes, in each of which the mind observes one of them.

The inside/outside conception of mind and world that is the legacy of early modern science, when carried over to the case of quantum mechanics, generates a dilemma, and it is one that is explicit in, for instance, Max Tegmark's defense of the many worlds interpretation. As Tegmark thinks of it, there are two ways to perceive a mathematical structure: "On one hand, there is what we will call the *view from outside*, or the *bird perspective*, which is the way a mathematician views it. On the other hand, there is what we call the *view from inside*, or the *frog perspective*, which is the way it appears to a SAS [self-aware substructure] in it" (Tegmark 1998a, 16). The frog perspective is that of the Copenhagen and instrumentalist interpretations, and, as Tegmark notes, that of everyday human language. The bird perspective is that of the many worlds interpretation, and of mathematical language. And we simply must choose, he thinks: either the outside view or the inside view, either mathematical language or natural language, either the Copenhagen interpretation or the many worlds, either, as Tegmark (1998b) puts it, many words or many worlds. As he writes (1998b, 858):

The reader must choose between two tenable but diametrically opposed paradigms regarding physical reality and the status of mathematics:

- **Paradigm 1**: The outside view (the mathematical structure) is physically real, and the inside view and all the human language we use to describe it is merely a useful approximation for describing our subjective perceptions.
- **Paradigm 2**: The subjectively perceived inside view is physically real, and the outside view and all its mathematical language is merely a useful approximation.

  What is more basic—the inside view or the outside view? What is more basic—human language or mathematical language?

But we do not have to choose, and we must not choose. We need both the perspective of natural language, the language with which it all begins, and that of mathematical language, which is an achievement of reason, the work of millennia. It is only the sideways-on view of early modernity that leads us to think otherwise, and that view is dismantled with the full realization of reason as a power of knowing.

On the Copenhagen interpretation of quantum mechanics, an act of measurement causes the state function, which left undisturbed evolves deterministically in accordance with Schrödinger's equation, to collapse. What had hitherto been a superposition of states collapses into some one state. On both an instrumentalist account and on an Everett-style many worlds account there is no collapse; but there is also no reason to prefer either the "inside" view of the instrumentalist or the "outside" view of the many worlds theorist. We need to give up the assumptions that generate the two possibilities; the real problem is the sideways-on view that underlies them. Instead, as in Einstein's relativity theory, we can hold that the measurement gives the relevant bit of reality but only through a facet of it, one way it can show up to embodied and embedded perceivers such as we are. To this way of thinking, the relevant bit of reality is given by the Schrödinger equation but we have access to this reality only from this or that perspective as constituted by a confirmatory measurement we make. If so, then we are quite like Abbott's (1884) Flatlanders whose experience of a three-dimensional object is of the changing form of something merely two-dimensional. The basic idea is as follows.

Imagine a cube passing through a plane and suppose that it can pass through the plane in only three ways: with one face parallel to the plane, with one edge first touching the plane (and the cube itself otherwise symmetrically oriented with respect to the plane), or with one vertex first touching the plane (and again the whole cube otherwise symmetrically oriented with respect to the plane). If the first of these three possibilities is realized then from the two-dimensional perspective of the plane a square will appear, persist a while as the cube passes through the plane and then disappear. If instead it is an edge that first meets the plane then what first appears is a line that then widens until it is a square and then begins to shrink back down to a line, then disappears. And if it is a vertex that first meets the plane, that vertex will become a diamond that grows for a while and then begins to shrink back down to a point before disappearing. These three ways the cube can show up in passing through the two-dimensional plane correspond to the possible outcomes of measurement. It is furthermore easy to work out the probabilities of each sort of outcome given that a cube has six faces, eight vertices, and twelve edges. The cube itself is not in any sort of superposition; nonetheless it is essentially probabilistic, from the point of view of an inhabitant of the two-dimensional plane, what the outcome will be. What is observed depends on the orientation of the cube as it begins its passage through the two-dimensional plane. The measurement (observation of the appearance of the cube) does not cause anything to happen, any "collapse"; instead it gives what is in fact a facet, one face, of the reality in question. The measurement gives us access to what is real, but only as mediated by our embodied and embedded situation in the world. It confirms that we have gotten the mathematics right and it is the mathematics that reveals the reality with which we are concerned.

In our discussion of Einstein's special relativity we noted that Einstein does not regard the Lorentz transformations as descriptive of a phenomenon to be explained by a theory of what is really going on but instead as directly disclosing the relevant bit of reality. As we also noted, what to everyday experience is taken to be the real, namely, time intervals and spatial lengths and distances, are now to be seen as only facets of what is fully objective, namely, space-time intervals. The first is a point about the mathematics employed in relativity theory; the second is about the measurements we make. In order more fully to bring out how we might similarly think in the case of quantum mechanics, it is helpful to consider again the way mathematical notation functions on Frege's view of it, and in particular two interestingly different ways we can think about Fregean sense in relation to what is objective, *Bedeutung*. First, a sense provides a mode of presentation of a thing (be it an object or a concept), what we can think of as an aspect or facet of it. So, to take a simple example, all of the expressions '$7 + 5$', '3×4', and '$\sqrt{144}$' designate one and the same number but they nonetheless express different senses. Similarly, the sine function, say, can be designated by a certain infinite series. The second way to think about Fregean sense in relation to the objective focuses instead on whole formulae and the thoughts (Fregean senses) they express. Such thoughts, we have seen, can be carved up into function and argument in various different ways to yield sub-sentential expressions, simple or complex, that designate various objects and concepts. For example, the formula '$2^4 = 16$', as Frege reads it, only expresses a thought independent of an analysis into function and argument. If we take '2' as marking the argument place then the sentence is seen as ascribing the concept *fourth root of sixteen* to the number two. If we instead take four as the argument then we are dealing instead with the concept *logarithm of sixteen to base two*. And if we take the argument to be sixteen then the concept is *fourth power of two*. (Other analyses are also possible.) Suppose now that we wished to know whether the thought expressed by '$2^4 = 16$' is true. In order to answer that question we must pick an analysis so as to ask whether the relevant concept gives the value True for the given argument. As we can think of it, we can either interrogate the number two, asking whether it falls under the concept *fourth* *root of sixteen*; or we can interrogate the number four, asking whether it is a logarithm of sixteen base two; or we interrogate the number sixteen, asking whether it is the fourth power of two. It does not matter which number we interrogate, but we must interrogate one or another in order to answer our question.

Now we need to think about the status of objects such as numbers and concepts such as being a fourth root of sixteen relative to this conception of the language. Although at first they might have seemed to be what is really real, now they show up only relative to ways of regarding the sense, only from one perspective on it among others. What now appears invariant is the *sense*, the thought expressed, and also the truth value that is the *Bedeutung* of the whole formula. The thought conceived one way appears to be about the number two, but conceived another way it appears instead to be about the number four, or about the number sixteen. Independent of any analysis into function and argument, the thought is in a kind of "superposition" of all possible analyses that is "collapsed" when we perform our function/argument analysis. The formula '$2^4 = 16$' directly gives the Fregean thought, which is not about any particular number—although, again, to interrogate that thought, determine its truth value, we must regard it as being about something in particular.

In much the same way, we can think of the equation giving the probabilities of various outcomes as directly giving the reality we are interested in. But the only way we can interrogate it is by measurement. The reality is not given by the measurement or even by a whole collection of measurements; it is given by the mathematics. But because we are doing physics, which is not an a priori science, the way to interrogate that reality is by making measurements and in this case, by contrast with that of '$2^4 = 16$' taking one measurement can preclude taking another (Heisenberg uncertainty)—as if having once read '$2^4 = 16$' as being about the number two one could not go back and see it instead as about the number four. So measurement clearly changes something. But again the fact that the mathematics itself predicts this is significant. Much as Einstein does, we can regard the relevant mathematics not as descriptive of some phenomenon to be explained by a theory about what is really going on but as directly disclosing the reality we are investigating. In our imagery, the mathematics is disclosing the cube of which our measurements provide only glimpses.

Again, this should *not* be taken as a positive suggestion. The aim is only to indicate the kind of possibilities that open up when we self-consciously give up the sideways-on view and take instead the standpoint of reason. It provides *a* way we might conceive the relationship of our measurements and the mathematics of quantum mechanics. On this sort of account it is the mathematics itself that reveals the structure of quantum reality, so far as it is revealed; the measurements are only confirmatory of the mathematics. But the image also seems to commit us to something more, namely, a hidden-variables view of quantum mechanics insofar as there is in our image a fact of the matter regarding the orientation of the cube, which would explain why it appears as it does on an occasion, only we do not know, before measuring, what it is. But this too, we shall see, can be called into question.

The mathematics of quantum mechanics predicts precisely what we find on repeated measurements, for instance, interference in the double slit experiment, Heisenberg uncertainty, and what appears to be non-locality for entangled particles. What is wanted, in keeping with the overall account we have outlined, is not a constructive, that is, reductive and mechanistic, theory that posits some underlying physical reality to explain the appearances, but instead a way of thinking directly about the mathematics involved, how we should think about what it is revealing to us about the fundamental structure of matter, not what matter is "made of" but what it is in its own nature. Again, the aim here is not to provide such an account; it is only to begin to alter our understanding of the space of possibilities within which such an account might be sought. And to that end, it will be useful to reflect on the nature of various sorts of temporal processes.[8]

It is a familiar fact of our everyday lives that we can be doing something, get interrupted, and as a result never finish what was started. Any ordinary instance of making something, say, a cake or a bookshelf or a trip somewhere, is like this. Even in instances in which one is interrupted for some reason and so never actually makes the cake, or bookshelf, or trip, still it is true that one *was* making a cake, or a bookshelf, or a trip. And the same can be said about various things that non-rational animals do. A bird is building a nest even if the nest does not get built because, say, a hungry cat fatally interrupts things. Such happenings, I will say, occur in narrative time; they are like stories that have a natural end, somewhere they are going, but can be left unfinished.

Think now of a stone that has become dislodged and is, as we would say, rolling down some hillside. The stone is stopped by a felled tree halfway down the hill. Was the stone's trip interrupted? Aristotle might say so insofar as the tree impeded the stone's natural motion, but we would not. A stone can roll all the way down a hillside but if it does not because it is stopped by some such thing as a felled tree then that was not what it was doing. It was simply rolling and would continue to do so until something stopped it. Similarly, although I can be extinguishing a fire and be interrupted, the rain cannot be extinguishing a fire and be interrupted. Rain can extinguish a fire; that is, it can rain long enough on a fire that the fire goes out. But until it has in fact extinguished the fire, the rain is simply raining, as it happens, on a fire. In this case, as in the rolling stone case, not only can the process not be

[8] The ideas to follow were first motivated by my reading of some of Price's work on retrocausality. That work, like most other work on interpretations of quantum mechanics, seems to presuppose various framework assumptions we inherit from early modern science that, if the account we have developed here is correct, need to be called into question. Price seems to assume, for example, that it is appropriate in physics to take up some form of the God's eye view, the view from nowhere, in his case, the view from nowhen, that of block time, as it contrasts with the time of our everyday lives. (See Price 1996, 124.) And he wants a classical causal account of quantum phenomena because, he thinks, "big things are collections of little things" (Price 1996, 153). From the standpoint of reason, such assumptions are far from obvious. Nonetheless, I follow Price in thinking that in reflecting on what quantum mechanics is telling us we need to reflect on the nature of processes in time.

interrupted (because it is not going anywhere in particular), there is no sense in which it *was* doing that even in cases in which the result comes about. Even if the rain does extinguish the fire, and the stone does roll all the way to the bottom of the hill, we cannot say, even if only retrospectively, that it was doing that all along. Such merely physical processes simply happen, one thing followed by another followed by another. They happen in block time.

The two sorts of processes just outlined differ along two dimensions. First, a process in narrative time can be interrupted, though a process in block time cannot. And second, a process in narrative time is teleological, a doing toward some end, while a process in block time is not. In block time, one thing is followed by another, and then another; there is nothing more to it than that. It furthermore seems clear that the idea of a process that can be interrupted but is not teleological is incoherent. If a process is not heading somewhere in particular then it cannot be interrupted along the way. How about a process that cannot be interrupted but is nonetheless teleological, a doing towards some end? To make sense of this possibility we need to suppose that although, if it is not completed, one cannot say that it was nonetheless happening (since that would amount to an interruption), nevertheless, if it *is* completed then it is right to say that that was what was happening all along. Having a brilliant career, or a successful marriage, or a happy life may be like this. If the arc of such processes is not completed—one's career starts out brilliantly but then fizzles, one's marriage turns sour and never recovers, one's happiness is destroyed late in life by a terrible tragedy—then it seems right to say that one was not after all having a brilliant career or a successful marriage or a happy life but only for a time seemed to be. In such cases, the process cannot be going on but get interrupted as cake-making can be going on and get interrupted. If it gets "interrupted" and remains unfinished, then it was not after all going on. But if it does get completed then, unlike the case of the rain or the rolling stone, it seems right to say that the process *was* going on all along, that one *was* having a brilliant career, a successful marriage, a happy life. And this seems to be true of also of evolutionary processes. In the biological case, for example, it seems incoherent to say that some species or other was evolving, coming to be, but that the process was interrupted so that the species never appears; nevertheless, it seems right to say, once the relevant species is on the scene, that it had been evolving towards what it in fact became. Processes in evolutionary time are such that there is no fact of the matter regarding what is now going on except in the light of what will be, how things in fact turn out.

To have been baking a cake one needs a past of a certain sort, one that involves measuring flour, breaking eggs, and so on, even if no cake eventuates. To have been embarked on a brilliant career one needs not only a past of a certain sort, one that involves some measure of success or at least promise, but also a future of a certain sort. If no brilliant career eventuates, then one was not after all embarked on a brilliant career. Similarly, for something to have been the evolutionary precursor of some species, there needs to be not only the right sort of past but also the right sort of future, one in which the latter species does come into being. Evolutionary processes (and brilliant careers, successful marriages, and happy lives) thus raise questions that can be answered only retrospectively, not merely because we are ignorant of where they will end up but because it is not settled where they are going, or even that they are going somewhere, until they get there. In the case of merely physical processes, the rolling stone, say, or a leaf that falls from a tree, we also cannot say where it will end up until it ends up there, but this is due merely to our own ignorance. Classical physics together with all the facts do settle it. Evolutionary processes are not like this. They are instead like processes in narrative time insofar as once they have happened, they exhibit something like a narrative arc, a beginning, a middle, and an end. They are, that is, teleologically structured, though, again, it is only settled (in fact, not merely as knowable) at the end of the process that there is such an arc through the process as a whole. We can apply this idea of an evolutionary process to the case of quantum mechanics as follows.

As already indicated, it is common in discussions of quantum mechanics to raise the question whether the probabilities that the state function gives are objective probabilities in the sense of giving us information about the system or whether instead they reflect only our ignorance, until we measure, of what is actually the case. If they give objective probabilities then it can seem to follow that the state is in a superposition of all the relevant possibilities. If the probabilities are instead a reflection only of our own ignorance then the system is already in some definite state, one and only one of those possible, only we do not know which. We make our measurement in narrative time; a measurement is a process of setting things up, initiating the experiment, and so on, and it can be interrupted. But we heirs of early modernity expect that the thing we aim to measure is in block time, that it is fully what it is at every moment, and hence that either the system is in a superposition (and Schrödinger's equation gives us everything there is to know about the system) or it is not in a superposition (and there is something of which we are ignorant). But now we can see that there is also a third possibility. If we think of quantum processes as processes in evolutionary time then the either/or, either superposition or ignorance, does not apply. In that case, the process of completing the measurement is what settles how things were before the measurement was made insofar as what the system is doing before the measurement is made is in part a function of how things end up, that is, the particular measurement that is ultimately made. If quantum processes occur in evolutionary time then what state a system is in now is a function not only of the past, including past measurements on it, but also of the future, what measurements will be made, how things will turn out. By measuring we thus can bring it about that something in particular was true in the past in much the way the emergence of some species brings it about that it was the case in the past that the evolution of that species was taking place. On such a view, there is neither a superposition of states nor ignorance. The mathematics provides everything that could possibly be provided prior to any measurement. It leaves nothing out despite being probabilistic because what it “leaves out” has in fact not been settled yet. It tells us with what probability this or that future will be realized provided that the appropriate measurement is made, and that just is what is now settled, before a measurement realizes some future in evolutionary time.[9]

Evolutionary processes do not merely happen in time, whether the flowing narrative time of our everyday lives or the block time of classical physical processes such as stones rolling down hills. They are instead constitutively, profoundly temporal; their being is in time, and cannot be understood apart from it. As we can think of it, their being is to become, that is, to evolve not merely in the sense of changing but in the sense of becoming something in particular, the kind of thing that, as it turns out, it was always already on the way to being. And because of this there is in evolutionary time a kind of symmetry between past and future just as there is in the case of processes in block time. In the case of block time—as contrasted with the time of our lives in which we can know the past but not the future (that is, the past is determined in a way that the future is not)—we can use the past to predict the future, and, at least in principle, the future to predict the past. Either is determined given the other. In the case of evolutionary time, there is again symmetry only this time it is a symmetry of ignorance, that is, indeterminacy: until things have in fact turned out some way or other, there is no fact of the matter either what was or what will be. Perhaps what the mathematics of quantum mechanics is disclosing to us is the reality of quantum processes unfolding in evolutionary time.

9.3 Completing the Project of Modernity

Our ancient (and everyday) mode of being in the world as enabled by a socially evolved natural language reveals a perceptible world of, in the first instance, living beings. It is a world in which we are at home as the animals we are. To the early modern mind such a self-understanding is a misunderstanding and must be rejected in favor of, on the one hand, a mechanistic conception of nature as the realm of law, and on the other, a conception of ourselves as autonomous, where autonomy is conceived in opposition to nature (the realm of natural law) but at the same time on the model of nature, as self-rule. Our contingency and finitude seem, however, to limit this autonomy. Although, as Kant almost saw, our autonomy is not opposed to but completed by the objectivity of the world, the fact that we are finite beings for whom something must be given in sensory experience ultimately entails, for Kant, that we know only things as they appear to creatures like us, not things as they are in themselves. Our contingency and finitude are, on the Kantian view, incompatible with true autonomy and full objectivity.

[9] Adequately developed, such an approach might shed light on what otherwise appears to be non-locality. Although there is no predicting the outcome, or even the current state, of an evolutionary process until it has come out, it could be that entangled particles share an evolutionary history in the sense that knowing, by measuring, how one comes out suffices to establish as well how the other does.

But as I have argued, we are not only contingent, finite beings but also essentially historical beings, beings whose rationality is fully realized only in time, through a process of maturation, transformation, and growth. And once we see this we are in a position to come to realize as well that contingency and finitude are not merely compatible with autonomy and objectivity but constitutive of it. Paradoxical as it may at first seem, *only* a finite being can be fully autonomous, capable of knowledge of things as they are in themselves, the same for all rational beings. It is Frege who provides us with the conception of language as the medium of disclosure of reality that is required in order to understand this. But we also need something more. We need to distinguish, as Frege did not, at least explicitly, between natural language, which is constitutively narrative, sensory, and object involving, and mathematical language, which is not. *And* we need to recognize that we do not have to choose between these two sorts of language; we need to realize that if one is more basic along one dimension, the other is more basic along another. Both sorts of language, and the modes of intentional engagement with reality they enable, are needed in any fully adequate understanding of the striving for truth in the exact sciences.

The advent of modernity in the seventeenth century constituted a rupture with the past, a decisive turn away from the old to the new. The task of science, as science came to be understood in the seventeenth and eighteenth centuries, was to discover the reality behind the appearances, how things really are given how they appear to us. The developments in the exact sciences begun in mathematics in the nineteenth century and furthered in fundamental physics in the twentieth, I have suggested, take a very different shape. These developments constitute not merely another break, another turn this time away from the merely modern, but instead the full realization of the intellectual aspirations of modernity, though in a hitherto unimaginable way. And at the heart of these developments is Frege's discovery of the logical distinction between the two distinctions, on the one hand, that between *Sinn* and *Bedeutung*, and on the other, that between concept and object. It is this logical distinction of distinctions that is, we have seen, the key to a fully adequate conception of truth and knowledge in the exact sciences.

Descartes achieved a new mode of intentional directedness on reality through a metamorphosis in his most basic understanding of what is, and as a result learned to distinguish in a way hitherto inconceivable between the sort of meaning we find in the world in our everyday lives and the new sort of mathematical meaning that was disclosed in his new science, *mathesis universalis*. From the perspective Descartes achieves, the ancient view of the world, which just is the view of our everyday lives, is shot through with confusion. In particular, attributes that properly belong, and can belong, only to minds are projected onto bodies; they are taken to belong to bodies as if bodies themselves could be minded. Thus it comes to seem that although we find it natural to take things, an apple, say, actually to have sensory properties such as color—the apple, we take it, is itself red—in reality it is inconceivable that a mere object could have the very color I experience it as having. In reality, the apple is mere physical stuff and has only the properties that mere stuffs can have, namely, mathematically describable properties. The redness we experience it as having is only a projection, not really a property of the apple at all.

This early modern view in which minds, our self-conscious experience and thought of things, are to be conceived in isolation from mere (unselfconscious) things, including our own bodies, thus gives rise to the sideways-on view according to which "outside" are mere stuffs while "inside" are meanings. Forgetful of the fact that the early modern view of reality is after all a *view* and as such fully meaningful to us, albeit in its own distinctive way, it comes to seem that with modernity we achieve a perspective on things that is somehow outside the subject-object nexus altogether, that we achieve, at least in thought, the God's eye view, the view from nowhere. In our becoming explicitly self-conscious of our own self-consciousness, it comes to seem, that is, that our subjectivity is not merely our being towards objective reality, our mode of being in the world, but itself something objective, something somehow in the world in much the way other things are in the world. Subjectivity, consciousness itself, must, then, be available to the scrutiny of science. Somehow consciousness is to be explained in terms of the workings of the brain. The sideways-on view, although in fact a metaphysical thesis made possible by Descartes' advances in mathematics, comes in this way to seem to be simply a *fact* about us, something we have discovered in the course of our empirical inquiries.

With modernity comes the idea that it is a finding of science that the sideways-on view is correct, that *really*, as scientists have shown, there is "outside" only mere stuffs and "inside" (somehow) meanings. "Outside" are causes and objects; "inside" are norms and concepts. This conception gives rise in turn, as McDowell has shown, to an intolerable oscillation between, on the one hand, an empty coherentism, and on the other, a vain appeal to what Sellars calls the Myth of the Given. And as McDowell indicates, it is Frege's notion of sense that enables us to dismount from the seesaw. But, I have urged, we need not only that, more exactly the conception of language as a medium of disclosure that it enables, but also to distinguish between two radically different sorts of languages. We cannot merely turn our backs on the practice of science, directly recover a more Aristotelian conception of our being in the world, because we know as Aristotle did not that we (and other living beings) are not natural in precisely the way that non-living things are natural. Furthermore, as we also now know, it was a development in science, in mathematics in particular, that provides the resources that are needed to achieve a stable conception combining the insights of the ancient Greeks with those of early modernity. The Fregean notion of sense comes into view only with the development of a mathematical language of the sort Frege provides; for, we have seen, only in this case is there a clear distinction to be drawn between sense and signification. It is from Frege's mathematical language that we first learn to conceive of language as a medium of disclosure, a lesson that can then be applied also to natural language. But if that is right, then instead of turning our backs on the practice of science and the sideways-on view that it has engendered, we need explicitly and self-consciously to take up that view and transform it from within, by appeal to the self-same disciplines that engendered it, just as we have done here.

According to the conception that is enabled by a Fregean understanding of language, we are, as the rational animals we have become through our acculturation into a socially evolved natural language, always already cognitively oriented on reality. Having learned to distinguish the *Sinn*/*Bedeutung* distinction from that of concept and object, we can now understand how language is the medium of our cognitive involvement in the world, how language at once gives us the eyes to see and by the same token the world a face by which to be seen. In place of the modern inside/outside picture, the sideways-on view that conflates Frege's two distinctions, we now have a conception according to which we have two fundamentally different modes of intentional directedness on reality, one that is mediated by natural language and directly reveals aspects of that reality, and another that is mediated by a fully realized mathematical language. In this latter case, one's cognitive relation to reality, we have seen, is not direct but instead occurs in two steps. First, in mathematics, one achieves cognitive grasp of the concepts of modern mathematics, concepts of reason, through the clarification of the senses of concept words. Then in fundamental physics, one employs those concepts in coming to know things as they are, the same for all rational beings. In both cases Fregean sense (*Sinn*) is that through which, as the medium or vehicle of awareness, we are in direct cognitive contact with reality. But, again, there are two very different ways this can work, through the medium afforded by natural language and through the medium afforded by a fully realized mathematical language of the sort Frege developed. The view of reality that is afforded by natural language is simply that of everyday life, a view of living beings with their natures and powers, and of perceptible things more generally. The view of reality that is afforded by mathematical language is not like this. What mathematical language directly yields is knowledge of concepts of various sorts of entities; it is most immediately the medium of our cognitive grasp of mathematical concepts. Those concepts are then employed in turn in fundamental physics and enable us to discover how things are, the same for all rational beings.[10]

Much as Newtonian physics is a necessary stage on the way to Einstein's physics but one that must eventually be superseded, so the sideways-on view, with its meaningless "outside" and meaningful "inside," is a necessary stage on the way to the Fregean view according to which we are always already cognitively directed on reality. As Newton paves the way for Einstein so Kant paves the way for Frege. The incoherent inside/outside picture thus gives way to a conception according to which

[10] It may be wondered where other sciences fit into this picture, sciences such as geology, chemistry, biology, and psychology. Such sciences are not, and I think could not be, maximally objective in the sense that fundamental physics is. They ineluctably involve our distinctively human perspective on things; and because they do, they perhaps inevitably are constructive sciences that posit laws and forces to explain the observed phenomena. This is even more obviously true of the social sciences.

we, now fully modern, must recognize as involving two essentially different modes of intentional directedness, one that we have as the embodied rational animals we always already are, and one that we have achieved through the course of intellectual maturation and growth that has been traced here and has come to fruition only very recently. And although on the early modern view the exact sciences reveal how things really are as contrasted with everyday experience which is merely how things appear to creatures like us, this distinction between reality and appearance is revealed, in contemporary science, to be misplaced. As we now can see, the world as it is disclosed in everyday experience is just the world; it is fully real.

But our everyday view of reality is a view that is available only to beings sufficiently like us, to beings with our sort of animal life. The view afforded by the exact sciences, because and insofar as it is purely rational, that is, mathematical in a way that is divested of any and all sensory content, is different. It is, in principle, accessible—though only in time, through a process of maturation and transformation—to any rational being, whatever its biological and social inheritance. The view afforded by the exact sciences is in just this sense maximally objective, the same for all rational beings. But, again, that does not mean that it is in some way more real, as if our everyday view were merely an appearance. To think that the view achieved by the exact sciences is not only more objective but also more real is to fall back into the sideways-on view, the incoherent conception of a meaningless "outside" and meaningful "inside."

Whether we adopt the everyday perspective of natural language or that of a fully realized mathematical physics, the world is fully meaningful to us; it is the objective correlate of our subjectivity, the reality on which we are intentionally directed, whether perceptually or intellectually. But the world as disclosed in fundamental physics is also inevitably impoverished. Because and insofar as it is the same for *all* rational beings, *whatever* the shape of their lives (including their lives as practicing scientists), the only value it can bear is that of truth. The world as disclosed in everyday life is, by contrast, rich with other sorts of value as well, not only truth but also, for instance, moral and aesthetic value. Rational beings with radically different forms of life from our own, owing to their very different forms of life and in particular to their very different ways of perceiving and acting, could (and presumably would) find different moral and aesthetic values in the world from those we find.[11] But, again, that does not make those values any less real. The idea that values other than truth are merely subjective impositions is merely another excrescence of the modern inside/outside picture that Frege shows us how to dismantle. The lives of rational animals, precisely because they are the lives of such animals, that is, of

[11] Why think that they would find such values at all? The answer lies in the fact that to be an animal, with no matter what sort of form of life, is to have sensory-motor capacities. Rational animals, then, necessarily have the ability to perceive and to act; and because they are not merely animals but rational animals, they can find beauty in what is perceived and goodness in what is done.

rational beings who perceive and act as well as judge, are inevitably rich not only with the value that is truth but also with aesthetic and moral value. Although early modern thinkers were puzzled by this, and reasonably so, we should not be. We should no more expect such values to be absent from the world as it is disclosed in our everyday lives than we should expect them to be present in the world as it is disclosed to pure reason in fundamental physics. The world disclosed in fundamental physics is the world as thought, as grasped in judgment; it is the world as it is the object of contemplation, the same for all rational beings. It is not the world as it is the arena of our actions and perceptions, including our actions and perceptions as practicing mathematicians and physicists (and philosophers). Again, we need both, both the rich but not fully objective view of reality with which we begin and the impoverished but fully objective view of reality that we achieve through the course of our intellectual inquiries culminating in a language that is the language of reason itself.

On the account we have developed, the concepts of fully modern mathematics that have been developed over the past two centuries are concepts of reason. Because they are, they can in turn provide us a view of reality that is fully objective, the same for all rational beings. And they do so, we have seen, directly in the following sense. Whereas in classical physics there is always a physical correlate for the mathematical concepts that are employed, in contemporary physics, there is no physical correlate but only the mathematics. "The ultimate 'natural kinds' in science are those of pure mathematics" (Steiner 1998, 60); "the ultimate language of the universe is that of the mathematician" (Steiner 1998, 5). Steiner (1998) argues that this "Pythagoreanism" of contemporary physics is profoundly anthropocentric, and hence that its spectacular success is deeply mysterious, even magical. Those conclusions follow, however, only if one thinks, as Steiner does, that mathematics has no proper subject matter of its own, that "concepts are selected as mathematical because they foster beautiful theorems and beautiful theories" (Steiner 1998, 64). Because, Steiner thinks, mathematics, at least as it has been practiced since the nineteenth century, is merely formal, without content of its own (because it is deductive and eschews Kantian intuition), the only criterion he can discern for distinguishing real mathematics from mere games with constructs is the mathematician's (merely human) sense of beauty. And then it really does seem to be a mystery how mathematical concepts so identified might be revelatory of fundamental features of reality. But Steiner's is not, we have seen, the only way we can think about the practice of mathematics in the wake of developments in the nineteenth century. The alternative, Frege's way that we have pursued here according to which mathematics is properly a science with its own subject matter, does not only *not* make a mystery of the role of mathematics in contemporary physics, it *explains* this role. Because and insofar as the concepts of modern mathematics are *fully* rational, that is, wholly non-sensory, they are precisely the sorts of concepts that can reveal things as they are, the same for all rational beings. That our most objective account of reality is mathematical is, on the account we have developed here, not at all "a wonderful gift which we neither understand nor deserve," as Wigner (1967, 237) famously claimed. It is precisely what we should expect.

9.4 Conclusion

Early modernity bequeaths to us a puzzle about our knowledge of things as they are that takes the form of an inconsistent triad of claims: that the world is radically mind independent, that the spontaneity of thought is wholly unconditioned, and that judgment essentially involves rational constraint by what is. What we have since learned is that this triad is not inconsistent—though it must seem so to a thinker such as Kant who has learned to draw a logical distinction between concept and object, and hence sees the problem, but has not yet learned to make the distinction between *Sinn* and *Bedeutung*, on the one hand, and concept and object, on the other, that is required in its solution. It is this latter distinction of distinctions due to Frege, combined with the fact of our historicity, that has enabled us to understand how a radically mind-independent reality and an unconditioned spontaneity are not only compatible but in the end made for each other. Because our spontaneity is unconditioned, because there is for it no Given, nothing unquestionable in principle, we can in time develop the language that gives us the eyes to see things as they are, the same for all rational beings. It is in just this way that the radical otherness of the world, its full objectivity, completes the spontaneity of thought, in just this way that the spontaneity of thought and the objectivity of the world form an intelligible unity. As we are now in a position to see, judgment, as the acknowledgement of the truth of a (true) thought, realizes a cognitive relation of knowing between a rational subject and a bit of empirical reality. The power of reason is fully realized as a power of knowing.

Although we begin, as the rational animals we are, with a view of reality that is shot through with the contingencies of our particular biological, social, and historical being, we also begin with a yearning for something more, for a kind of knowledge that is not merely partial, limited, and frustratingly perspectival. And in the end we achieve it, not by somehow (impossibly) transcending our limited, perspectival bodily being in the world, but by realizing, finally, that *only* such a being can know things as they are. Judgment is an act not of productive freedom but of expressive freedom. It leaves everything as it is. To judge truly is to make manifest the otherwise latent structure of reality. But it is only in the end that we can understand this. Only with the realization of reason as a power of knowing can we recognize ourselves as knowers of things as they are, the same for all rational beings.

Afterword

In the nineteenth century mathematics was transformed to become as it remains today a wholly conceptual enterprise, the work of pure reason. And with this transformed mathematical practice came a new problem for philosophy. Ancient diagrammatic practice had posed for philosophy the problem of the ontological status of its objects, of the being of the intelligible unities that form, so it was thought, the subject matter of the science. Early modern algebraic practice posed instead the problem of our ability to know its synthetic a priori propositions, propositions that are again intelligible unities insofar as they are necessary but not logically necessary. Contemporary mathematical practice poses the problem of its form of reasoning, its proofs, which as at once ampliative and deductive constitute yet a third sort of intelligible unity. Frege solved this problem in 1879. And in so doing he provided the key to any adequate account of truth and knowledge in the exact sciences.

The philosophical problem of truth and knowledge in mathematics concerns how mathematics works as a mode of intellectual inquiry into objective truth, and it can be adequately resolved only after that practice has matured into a purely intellectual endeavor. Although mathematics can only begin as a form of inquiry that is dependent on our sensory and motor capacities—manifestly so in Euclidean geometry but, Kant saw, evident also in the constructive algebraic practice that dominated mathematics in the seventeenth and eighteenth centuries—eventually all sensory content is stripped away to reveal a mode of inquiry reliant solely on pure reason. Both the concepts of contemporary mathematics and its methods are purely rational.

But knowing that contemporary mathematics is a purely rational enterprise is not sufficient for an adequate conception of mathematical inquiry. One needs also, we have seen, to achieve a conception of judgment as an act of spontaneity, of expressive freedom, that when successful is an exercise of reason as a power of knowing. To judge, acknowledge the truth of a true thought, is to make up one's mind through inquiry, that is, in the case of mathematics, through proof—though, as we now know, proof is a means of inquiry only because and insofar as it is embedded within a practice that conforms to the Aristotelian model of science and hence is constitutively self-correcting. And even this is not yet sufficient. For none of this can really make sense without our having wholly and unequivocally dismantled the sideways-on view that we inherit from early modernity. Transformations in the exact sciences in the seventeenth century required that we embrace the image of the mind as an inner space standing over against a merely causal, normatively inert physical reality; our learning to think of our mindedness in opposition to physical reality, and to distinguish in principle between reasons (in the mind) and causes (in the world), was an essential stage in our intellectual maturation. But as we now know, this was, though necessary, only a stage, one to be gone through and then left behind. The natural languages into which we are acculturated at once give us the eyes to see and reality a face by which to be seen, and it follows directly that we rational animals are always already in cognitive contact with reality. But we can come adequately to understand how this can be only by reflecting, as we have here, on the historical unfolding of our intellectual investigations from the first appearance of reason through to its full realization.

And here we come to see something else as well, something that, unlike the problem of truth and knowledge in mathematics, or that of the relation of mind and world, the tradition had no inkling of, namely, that what all this historical unfolding was *about* (or at least one principal thing it was about) was reason's realization as a power of knowing. This is something that could not have been foreseen. Only after reason has been so realized is it possible to trace out the trajectory *as* that of realizing reason as a power of knowing. It happens in (what we have called) evolutionary time. And that is why our account of truth and knowledge in mathematics, of how mathematics works as a mode of intellectual inquiry, has taken the form it has, that of a narrative, a story with a beginning, in the first appearance of reason, a middle, in which reason is transformed in the seventeenth century, and an end in reason's full realization as a power of knowing in the nineteenth century.

But again, although the nineteenth century developments in mathematics mark the end of our story, it is those same developments that are what make our story possible at all. Frege designed his concept-script as a language within which to reason deductively from defined concepts, and it was in the light of this language that he learned to distinguish the *Sinn/Bedeutung* distinction from that of concept and object, where this distinction in turn is the key to an adequate understanding of the nature and role of language in our cognitive lives. Only the development of a sufficiently advanced mathematical language can enable us to understand how the acquisition of language makes possible our cognitive relation to reality. Our acculturation into natural language and an essentially social form of life is what first opens our eyes to the world of everyday experience, but it is our acculturation into a sufficiently advanced mathematical language and culture that opens our eyes to fully objective reality, things as they are, the same for all rational beings, and thereby enables us adequately to understand even our everyday being in the world.

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For the second time in its long history mathematics was transformed in the nineteenth century. Physics similarly was transformed, for the second time, in the twentieth century. Philosophy was not transformed but has, until now, remained merely Kantian. This story of reason's realization aims to take the first step. What comes after will be for others to tell. One cannot jump over one's own shadow.

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