A B S T R A C T S

Understanding Defectiveness in the Sciences

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Understanding Incompleteness in the Sciences

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Ideally, scientific information should have a number of features, in particular, accuracy, empirical support, and relevance. But should it be complete as well? In this paper, I argue that the gap between the ideal and the actual is significant and wide. For a variety of reasons, scientific information is often inaccurate, poorly empirically supported, and not as relevant as it should be. And although there are good reasons for still aiming at yielding accurate, empirically supported, relevant information in the sciences, the case for completeness is different. Here, I argue, incompleteness is not only ubiquitous, but inevitable. It is, thus, crucial to learn how to live with and cherish this salient feature of scientific practice. I examine the key role played by incompleteness in scientific reasoning and examine ways of making sense of it and exploring it further.

Confusion as a Scientific Defect: Logics for languages with confused terms

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One type of defectiveness in science is ambiguity or confusion in scientific concepts or terms. I will call a term 'confused' if it conflates two or more things. One common way science changes over time is by drawing new distinctions, thereby eliminating an earlier confusion that conflated two things. For example, in biology, the obsolete concept *warm- blooded* conflates three distinct concepts: *endothermic*, *homeothermic*, and *tachymetabolic*. The chemical term 'acid' also has three distinct definitions (Arrhenius's, Bronsted and Lowry's, and Lewis's). From the point of view of special relativity, 'mass' in classical mechanics confuses 'relativistic mass' and 'rest mass' (Field 1973). Many other examples of confused terms appear throughout the history of science.

What should we make of utterances containing such confused terms? Can they be understood? If so, how? And what form should our logic take, if we want to make rational inferences when using a language that contains confused terms? To address these questions, I borrow a taxonomy from free logics; free logics allow non- denoting names (e.g. 'Pegasus' or 'Santa Claus') to occur in the language. All free logics fall into one of three genera: neutral, negative, or positive. In neutral free logics, all atomic sentences containing a non-denoting name are truth-valueless. In negative free logics, every such atomic sentence is false. Positive free logics, on the other hand, allow at least some atomic sentences containing non-denoting names to be true (usually, at least 'Pegasus = Pegasus'). I will use this tripartite classification system, with 'confused term' substituted for 'non-denoting name' above.

This talk investigates what alterations (if any) should be made to classical proof systems, if we want our languages to accommodate confused terms. Although I will present and discuss all three families of options (neutral, negative, and positive), I focus on the positive case, for two reasons. First, neutral and negative logics for confused terms treat those terms as (at least functionally) semantically equivalent to empty terms, like 'Pegasus.' But confused terms seem semantically distinct from such names: there is arguably a difference between too much reference and too little. The sentences 'Warm- blooded organisms have hearts' and 'Warm-blooded organisms exist' seem importantly semantically different from 'Planet Vulcan is between Mercury and the Sun' and 'Planet Vulcan exists.' Similarly, to use a mathematical example, '$\sqrt{4}$' seems different from '1/0'.

Second, more technical work remains to be done on the positive case than the negative or neutral cases. I prove that an argument is truth-preserving on the standard negative semantics for free logic iff that argument is also truth-preserving on the most natural negative semantics for languages containing confused terms. So then one can simply use the already-developed sound and complete proof system for negative free logics, without alteration. And for the neutral case, because the most natural neutral logics for confused terms are very close to the well-studied strong and weak Kleene schemes, there is not novel work to be done there, either. The proof systems for positive free logics, on the other hand, cannot be taken over without alteration into the (plausible) positive logics for languages with confused terms.

The main technical results of the paper are that, on the most appealing positive semantics for languages with confused terms, the proof system of classical first-order logic is strongly sound—but only if the language does not include an interpreted identity predicate. If the language does contain an identity predicate, then the classical rules of $\forall-$ elimination and $\exists-$introduction are invalid. I then discuss possible replacements for these classical rules, but none of the candidates are clearly the best way to modify the proof system.

This technical result is philosophically interesting, I believe, because one of the justifications often given for developing and investigating formal languages is that ambiguity must be eradicated. Frege calls for "a system of symbols from which every ambiguity is banned" (Frege 1972, p.86), on the following grounds: "Language proves to be deficient, however, when it comes to protecting thought from error. It does not even meet the first requirement which we must place upon it in this respect; namely, being unambiguous." (ibid., p.84) And this rationale is not merely a historical antiquity. A recent textbook says: "Why bother with formal languages? Because everyday languages are replete with redundancies and ambiguities" (Smith 2013, p.25). But if the classical first-order proof system is strongly sound on both natural positive semantics for languages containing confused terms, then the demand to cleanse ambiguity from language appears otiose. However, once we allow an interpreted identity predicate into the language, the common demand to eliminate ambiguity from our language becomes justified.

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The Explanatory Economy and Counter-Examples in Cognitive Science

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Much work in the cognitive sciences involves positing laws/constraints on the behavior of internal, psychological mechanisms. Explanatory power is achieved when these laws are able to explain observed behavior. The central difficulty arises because behavior is a product of the interaction between many such systems. This makes inferences from observed behavior to laws governing underlying capacities inherently risky, as the observations alone do not determine which capacities they are responsive to. Another way of putting the problem is: because the laws posited by cognitive scientists apply to unobserved systems, there is usually a mismatch between the predictions of these laws taken on their own and observed behavior. In particular, due to the complex pathway from the workings of a particular psychological system to behavior, observable evidence is highly confounded. This creates a dilemma for cognitive scientists: on the one hand, simple generalization from observed behavior will produce regularities which do not actually govern the workings of any particular psychological system. On the other, observed behavior seems, in many cases, to be all the evidence we have to go on, and ignoring these data seems unscientific.

Consider, for example, theoretical linguistics. The job of a theoretical linguist is to uncover the laws governing the linguistic competence of a native speaker of a natural language. For example, generative theories of syntax posit unconscious rules governing the distribution of referring expressions (e.g. that reflexives such as 'herself' must have *nearby* antecedents: witness "Paolo thinks Anastasia loves herself." but not *"Anastasia thinks Paolo loves herself."). However, if linguists simply generalized from observed behavior, such rules would not result, as there are robust examples of linguistic behavior which violate them. For example, a variety of (possibly semi-idiomatic) constructions allow the use of apparent reflexives without antecedents at all (e.g. *polite* uses, as in "Achille and myself request the pleasure of your company." and logophoric reflexives, as in "Academics, such as yourself, don't understand the common man's struggles."). How ought linguists treat such apparent counter-examples?

One possible strategy is to take these observations to show that our original rules were false. Weaker rules, including clauses stating where and when exceptions can occur, could be proposed. This would align linguistic theory more closely with empirical evidence, and is the strategy adopted by various theorists, especially those in the cognitive/construction grammar tradition (such as William Croft, Adele Goldberg, and Michael Tomasello) and those working within the *big-data* tradition in computational linguistics (such as Steven Abney, Peter Norvig, and Fernando Pereira). However, this strategy leads to highly specific and complex generalizations, which merely recapitulate the observations rather than providing genuine explanations. Crucially, the recognition that linguistic behavior depends on both linguistic competence and also extra-linguistic capacities and systems means that any generalization straightforwardly abstracted from the observational data will not describe the specifically linguistic system that is the target of linguistic theory. Alternatively, one could aim to generalize only from some subset of these data. If some linguistic behavior is relatively free from the confounding influence of non-linguistic causes, then perhaps we could capture the laws governing specifically linguistic competence by generalizing only from observations of this behavior. This is the strategy adopted by mainstream generative theorists, and has been dubbed by Noam Chomsky 'The Galilean Style'. The problem with this approach of course is determining which observations are to be included and which ignored. Without a principled way of drawing this distinction, linguistics seems susceptible to confirmation bias, allowing data which confirm the theory to be included in this evidential base, but excluding data which appear to disconfirm the theory. Opponents to the generativist tradition have often made exactly this accusation.

In this way, it is clear that the database for linguistics is defective in certain ways. Generalizations on the basis of these data do not produce the kinds of laws aimed at by cognitive science. The central defect of these data is confounding. The data are products of a much wider range of causal influence than the theory aims to capture. For this reason, the central aim for a linguist is to de-confound these data, factorizing out the different influences on them and determining which aspects of these data are indicative of specifically linguistic influence. In my paper, I aim to defend the Chomskian approach by providing a principled way of determining which observations ought form the basis for linguistic observations and which ought be excluded.

I call my approach to this question 'the explanatory economy'. This approach stresses the interdependence of compatible theories in science. Crucially, I argue that apparent counter-examples can be excluded from the inductive base for a theory only when they are plausibly explained by a distinct, but compatible, theory of some other cognitive system. For example, when a grammatical theory suggests that a certain sentence will be (un-)acceptable, but it is not viewed as such by native speakers, this observation need only be viewed as falsifying the grammatical theory if it cannot be explained away by some extra-linguistic theory, such as a theory of memory or parsing. In this way, an initially defective data set can be partitioned into multiple sets, each of which is properly explained by distinct theories of different cognitive systems.

While I have focused on the case of linguistics, especially syntax, the problem that I am responding to, of data sets reflecting too wide a variety of causal influences to generalize from, is ubiquitous in the cognitive sciences and beyond. Any science of a complex system will be confronted with this problem. Given this, I believe the explanatory economy is a very generally applicable approach, and can be used to form the basis of a general approach to confounded data.

Why, not eschewing inconsistent theories, one should try to eliminate inconsistencies?

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I shall defend two apparently conflicting theses. (i) A central and justifiable means behind the development of the sciences was (and is) the maintenance of consistency. (ii) Some interesting theories are unavoidably inconsistent and have as underlying logic either a paraconsistent Tarski logic or an inconsistency-adaptive logic. A remarkable feature is that, for all those theories, the Tarski logics have a relevant implication as detachable connective, whereas the inconsistency-adaptive logics have, like classical logic, a detachable material implication.
Seeing the sciences as a patchwork avoids the conflict. Yet, doing so presupposes that a *locus* from where the totality of the sciences can be viewed—the idea is often misunderstood and needs to be explicated. Moreover, this engenders the question whether the *locus* is classical or paraconsistent. I shall claim that there is insufficient evidence for answering the question, but that some good arguments support the classical option.

Defective Information and Abduction

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Abductive inference is a key method of science (cf. Schurz 2008). It is often understood as an inference to the best explanation, where an explanation is better than another one if it shares more epistemic virtues as, e.g., having high likelihood with respect to some evidence and being simpler. The main idea is that given a set of alternative hypotheses $\{H_1, ..., H_n\}$ to explain some phenomenon E one ought to choose that H which has theoretical virtues like highest likelihood and is most simple/least complex in comparison to the other hypotheses (cf. Douven 2018).

It is clear what the epistemic value of a high likelihood consists in. E.g., if one can establish even a deductive relation between some H_i and E (as suggested by the DN-model of explanation; cf. Hempel 1965), then the likelihood is maximal; if one cannot establish such a relation, then, whatever comes close to it or approximates it better, is epistemically valuable. However, regarding simplicity, it is debatable whether it bears epistemic value or not. In this paper we examine how, based on an approach to simplicity by Forster and Sober (1994), simplicity can be epistemically justified by help of reference to defective information. The main idea is that one can spell out the truth-aptness of simplicity, i.e. its epistemic value, via constraints put forward in the literature of curve fitting, which is about constraints for selecting models in order to explain or predict data. One important constraint that is particularly relevant with respect to simplicity is directed against possibilities of overfitting defective data, which means that by a too close fit of the data, also errors within the data are fitted. As we will show in detail, the rationality of the abductive methodology hinges on a rational justification of the value of simplicity, and the latter, in turn, hinges on the presence of defective data. Hence, rationalising our current understanding and use of an abductive methodology in science hinges on the use of defective data. In this sense, abduction is a key method of employing defective data and might be considered as a "logic" for the use of defective information in science.

We will proceed in three steps as follows: In the first part, we outline different forms of abductive reasoning used in science (cf. Schurz 2008) and discuss theoretical virtues assigned to all of them. In the second part, we present traditional accounts of an epistemic justification of these virtues.

There, we will also briefly sketch the mentioned curve fitting approach to simplicity and show how a particular form of abduction can be justified on the basis of defective data. In the third part, we aim at a generalisation of this approach. The main idea is that in the curve fitting literature simplicity is measured via the number of parameters of a model, where a model is a polynomial and a parameter is a coefficient of the polynomial. However, it remains open how the notion of simplicity spelled out in these terms relates to the notion of simplicity as is often used in other abductive inferences, namely as the number of axioms or laws used in an explanation (cf. Baker 2016). We show how the latter notion is related with the former by help of structural equations. By applying an idea of Forster and Sober (1994) we show how probabilistic axioms or laws can be reformulated as structural equations; these in turn can then be used to assign numbers of parameters to such axioms or laws, and hence allow for applying established complexity measures which simply count the number of parameters. By this, one can provide an exact translation manual for the *number of parameters approach* to the *number of axioms and laws approach*; this can be employed, e.g., in transferring the epistemic value of simplicity granted for the former domain to the latter one. And by this, abductive inference in general can be rationalised on the basis of defective information in science or, the other way round, defective information can be rationally employed by abductive inference.

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Can we have a hierarchical model of Scientific Understanding? A response to Batens

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Here, I aim at providing interesting responses to two important questions from the philosophy of science, namely: *Can we have a hierarchical model of Scientific Understanding*? and *What is the role of scientific understanding when explaining the tolerance of defectiveness in the sciences*?

First, according "to hierarchical models of science, our scientific knowledge (...) forms a knowledge system that has two properties: (i) it is stratified, and (ii) the items of some layer are or should be justified in terms of items of a higher layer" (Batens, 1991: 1999). In [Batens, 1991] it was argued that hierarchical models of scientific knowledge face important difficulties, such as lacking stable justificatory mechanisms that can avoid infinite regress or the absence of (robust) relations that can explain how to increase the order of our knowledge system. In contrast, contextual models tend to be more satisfactory in both respects −specially when explaining the ways in which scientists rationally deal with defective information in their day-to-day practice.

Second, in recent years much attention has been paid to a different epistemic phenomenon in the science, namely, *scientific understanding*. It has been claimed that the value of understanding seems to surpass that of knowledge; concretely, that "knowledge may easily be acquired through the testimony of experts; understanding, by contrast, seems more demanding and requires that an epistemic agent herself puts together several pieces of information, grasps connections, can reason about causes, and this too suggests an added value" (Baumberger, Beisbart & Brun, 2017: 3). However, while many different ways to attain and assess scientific understanding have been put forward by epistemologists of science, there is still no unanimous view on how to characterize it and so, depending on the particularities of the cases of understanding that are studied, different philosophical approaches have been presented.

In light of the above, if Batens' view is correct about the difficulties that any hierarchical model of knowledge would face, it seems that, because understanding encompasses knowledge, any hierarchical model of scientific understanding will suffer from at least the same problems that the models of knowledge do.

Here I contend that this is mistaken. I argue that, even if accepting Batens' arguments against to hierarchical models of knowledge, it is still possible to provide a hierarchical model of scientific understanding. Furthermore, I contend that such a model can be explanatory as to why and how scientists tolerate certain defects in their day-to-day practice.

In order to do so, I proceed as follows: First I discuss Batens' view on hierarchical vs. contextual models of scientific knowledge and explain how Batens' contextual models can be explanatory of the tolerance of defective information in the sciences –especially contradictory information. Second, I introduce the phenomenon of scientific understanding and present some of the different ways to characterize it. Third, I propose a hierarchical model of scientific understanding such that integrates some of the most important views on what is this epistemic phenomenon. And, finally, I address how this model can be explanatory of the temporal tolerance of contradictions in the sciences.

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Idealisations and the No-Miracle Argument

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Models are widely recognised as a central component in scientific practice. It has become commonplace to assume that a scientific theory is best presented as a collection of models.

Scientific models often intentionally caricature their targets, distort well accepted scientific laws by approximation and idealisation, sometimes combine incompatible theories, or use pure fictions or superseded theories for explanatory purposes. This use of falsehoods could seem puzzling for a scientific realist who thinks that the aim of science is to achieve some kind of truth.

Models are not truth-bearers: we do not say that a model is true or false, but rather good or bad. Yet the realist can make sense of her position by taking theoretical models, qua abstract entities, as truthmakers: they satisfy descriptive statements, including theoretical laws and descriptions of their targets. We can say that a model, or part of a model, is veridical if it satisfies true statements.

The problem is that among the statements satisfied by successful models, some can be considered true—and for the realist, this must be importantbut others are known to be falseand presumably, it does not matter. But both participate in the model's explanatory or predictive success. This threatens the validity of the nomiracle argument for scientific realism, based on the success of theories: how can a model's success justify the truth of our theories if the fact that it satisfies falsehoods contributes to its success as much as the fact that it satisfies true statements? (Sorensen 2012)

I examine various realist strategies to answer this problem, and argue that they fail. The following claims have been used to defend the immunity of idealisations:

- idealisations can, in principle, be de-idealised (McMullin 1985), which can only make the model better;
- idealisations are harmless (Elgin et Sober 2002): if corrected, they "wouldn't make much difference in the predicted value of the effect variable";

- idealisations point to parts of the world that are explanatorily irrelevant (Strevens 2008);
- the function of idealisations is to isolate relevant aspects of the target system (Mäki 2009);
- idealised models display "modal robustness": less specific veridical characteristics are preserved through change in acceptable idealisations (Saatsi 2016)

A common aspect of these strategies is to assume that if idealisations possibly contribute to a model's goodness by means of virtues such as cognitive traceability or isolation, they do not essentially contribute to its predictive success. So predictive success can be considered an indicator of veridicality, for the components of models that really contribute to it.

One problem is that some idealisations could be indispensable for predictive success (Batterman 2005). Saatsi addresses the case of ideal gases, and claim that one can postulate that there exist a yet unknown veridical model that makes the same predictions as the idealised one. I argue that this response is problematic, because what contributes to predictive success cannot be identified.

But even if the components of models that contribute to their predictive success could be identified, another problem looms. According to the no-miracle argument, interpreted as a metaabductive strategy (Psillos 1999), scientific realism does not explain empirical success *per se*, but the success of inference to the best explanation (IBE) for producing extendable theories. The realist explanation is that IBE is truthconducive.

However, if idealisations contribute to other virtues than predictive success, such as simplicity and cognitive traceability, then they are constitutive of good explanations. This is reinforced by the fact that fictions or superseded theories are often employed for explanatory purposes (Kennedy 2012; Bokulich 2016). This can be mounted as an argument against the truth-conduciveness of IBE: very often, the best explanations are fictitious rather than true, and if accepted scientific theories are the best explanations we have for some classes of phenomena, then they might be fictitious rather than true.

Bokulich claims that the use of fiction is compatible with truth as an aim for science, because explanatory fictions retain the modal structure of their veridical counterparts while increasing understandability. I argue that this approach still undermines the truth-conduciveness of IBE, as it accepts that fictions can make an explanation better. Furthermore, it suggests that modal structure is not, by itself, explanatory, which also undermines structural realism. At most, one should take IBE to be empiricaladequacy-conducive rather than truth conducive, perhaps with a modal notion of empirical adequacy.

In light of this, the best way to understand the functioning of science is along the line of (van Fraassen 1980)'s constructive empiricism, and its distinction between belief and acceptance: theories can be accepted by scientists for cognitive purposes, as fictions can be, but scientists need only believe that they are (modally?) empirically adequate.

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Avoiding defective causal explanations: Epistemic utility theory as a guide to variable choice

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Scientific explanation, in many cases, amounts to identifying the causal mechanisms that brought an event about. For each effect, however, there are usually a number of distinct descriptions of the cause, which differ in how finely they single out the particular event that explains why the effect came about. A norm that has been put forward in order to determine the appropriate level of granularity is *proportionality* (Yablo, 1992). Broadly, proportionality states that a causal variable ought to be chosen so that it is true that had the causal variable taken a different value, the effect would not have occurred.

Norms of variable choice, including proportionality, are usually justified either by claiming that they can account for a number of causal intuitions, or by showing that following them yields pragmatic advantages. In this paper, I show that epistemic utility theory (Pettigrew, 2010) can provide a solely epistemic vindication of the norm of proportionality. This means, an agent that adopts a proportional causal explanation will be shown to generally end up having a set of beliefs that is at least as close to the truth as if she had adopted a causal explanation of different granularity. More precisely, for every possible relevant state of the world, the probability to hold a maximal number of true belief is at least as high as for any other set of beliefs. In my paper, I further show that this can dissolve a number of purported counterexamples to the norm of proportionality, as, for example, outlined by Shapiro and Sober (2012).

While the argument for proportionality in deterministic analyses of causation is straightforward, it is harder to provide a counterpart in the context of probabilistic causation. Pocheville et al. (2017) present an account of proportionality in cases of probabilistic causation along the following lines: if adopting a fine-graining C' of the causal variable C would not alter the conditional probability distribution over the effect variable, then C is proportional. If we adopt this probabilistic counterpart to proportionality, however, cases can arise in which proportionality is in conflict with the norm of *abstractness* (see, e.g., Kinney, 2018). Abstractness requires that a causal explanation be as abstract as possible in order to provide general causal explanations for classes of events, not only single occurrences. To illustrate this, think of the following example. We have to decide which of the two causal explanations we adopt:

(1) The cape's being red caused the bull to charge

(2) The cape's being crimson caused the bull to charge

Clearly, (2) is a fine-graining of (1). Further, assume the causal probability of the bull charging given the cape is red is 0.7, but 0.95 given the cape is crimson (and, say, 0.1 given the cape is not red). Proportionality recommends (2) as the adequate causal explanation, whereas abstractness recommends (1).

I show that epistemic utility theory can be used to develop a probabilistic notion of proportionality that arbitrates between proportional and abstract causal explanations given the specification of the epistemic context we are concerned with, so that a unique recommendation as to which level of granularity is adequate can be obtained.

Here is the general framework for determining the most proportional causal variable among a set of potential causal variables that represent differently fine-grained versions of one another: we imagine that we observe that a variable takes a specific value (which we define as the effect) and ask the question how good our expected epistemic situation is if we adopt a certain causal relation as part of our set of beliefs. In choice-theoretic terms, this means that the acts available correspond to the adoption of differently fine-grained causal explanations. The partitioning of the states of the world correspond to the values of the most fine-grained among the potential causal variables that we consider. The more coarse-grained is - by assumption - a disjunction of two or more values of the fine-grained variable. The likelihoods of the states of the world are the conditional probabilities of the values of the causal variable given the observed effect. It suffices to simply assume that the utility of giving a true causal explanation is strictly greater than giving a false one, and that those two utilities are the same for all levels of granularity. It follows readily that generally, adopting a proportional causal explanation is the dominant strategy in the case of deterministic analyses of causation.

For the somewhat more complex case of probabilistic analyses of causation, by the same strategy we obtain a threshold for the ratio of the utility of a true and a false causal explanation. If the ratio is below the threshold, the more fine-grained causal explanation yields greater expected epistemic utility. The ratio of the utilities is what I call an epistemic context. It determines how valuable it is to arrive at a true belief in comparison to avoiding a false belief.

In the final part of the paper, I want to address two considerations. First, I want to argue that such a context-dependent notion of proportionality is reasonable in the light of how we intuitively think about causation. Second, I want to argue why the framing of the choice situation as a counterfactual scenario exerts normative force on the choice of variables in a causal model.

Dealing with Incomplete Information in Extensions of Russellian ESR: A Look at the Space-Time Case

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Russell's work in philosophy of science has been a subject of renewed interest since the publication of Demopulos and Friedman's (1985) where they drew important connections between Russell's theory of theories: structural realism and the issue of realism in philosophy of science. In this talk I argue that if we fill out the details of a Russellian structuralist approach to science by adding some very plausible assumptions to the logic plus some extra-logical assumptions about possible perceivers in the manner in which Russell intended (1948) then it is possible to extend the explanatory power of *ESR* to cover at least some of the objects of classical physics e.g. space-time and it is to be expected that additional assumptions pertinent to the relevant domains in very much in the spirit of the program should be capable of generalization to the rest of the sciences. In this sense, it is possible for the Russellian 's to solve at least one of the problems of broadly understood defective information: the problem of incomplete information. [1]

Russell's structuralist approach grows directly out of the relation-arithmetic developed by himself and Whitehead in Principia Mathematica (1910) and his attempt to relate the logico-mathematical techniques developed in that work to the problem of the applicability of mathematics, particularly to physical science. Russell (1919, 1927, 1998) mentions at least three relevant aspects:

> **(i)** inasmuch as a mathematical theory can be said to be true of a domain of objects whenever they satisfy some formal structure, then it doesn't matter what are the specific objects which satisfy the axioms nor whether they are complex or simple and
>
> **(ii)** given some epistemic assumptions regarding the physical world involving the relation of percepts to their causes (Helmholtz-Weyl; Mirroring Relations), it is possible for us to know a great deal about

[1] The problem of inconsistent information might be addressed by weakening the underlying mathematical-logic (1979).

its structure, if by structure we understand the notion on similarity of relations;

(iii) the relational structures in the world are of the same logical-type as perceived relations given co-punctuality between percepts and non-percepts.[2]

These elements make Russell an epistemic structural realist (*ESR*ist). The ESRist who intends to use a Russellian upward approach (Votsis, 2005) will distinguish between observables and unobservables in the indirect realist sense and furthermore will be committed to the claim that scientific knowledge of the world's structure obtains in virtue of the Helmholtz-Weyl principle (different effects, different causes) and the Mirroring Relations-Principle (relations in the world mirror the logico-mathematical properties of relations between percepts). But if so, how can this sort of structural realist explain defective scientific (incomplete) information? How does the relation-arithmetic structural account of science, of the Russellian *ESR*ist accommodate that possibility which doesn't easily fit the classical logico-mathematical notion of structure? How is such a possibility explicable by appeal to the very slim epistemological anchor afforded by the assumed truth of *HW* & *MR*? What is missing for a full-fledged development of his structuralism amounts to: (i) an explicit characterization of distinguished structure which allows for an objective distinction between intended and unintended attributions of structure to the physical world i.e. some notion of Naturalness or Foundedness (Lewis, 1983; Demopoulos & Friedman, 1985); (ii) a thorough investigation of how his method of dividing problems in: logical, physical and epistemic (Russell, 1914; 1927) can solve philosophical problems in physics when embedded in this framework and (iii) an extension of the methodology from a very solipsistic basis to a methodology encompassing data outside one physical body (1927, 1948).

In this talk I discuss these points explicitly connecting the approach Russell undertook in *The Analysis of Matter* to his project for developing postulates of non-demonstrative inference in *Human Knowledge* (1948) and show the fertility of these assumptions by investigating how it is possible to in broad outlines recover physical space-time (Maudlin, 2012) as an artifact of representation. It has been argued in the literature on the philosophy of space and time (Maudlin, 2012; Dasgupta, 2015) that Leibnizian arguments either show that there are multiple empirically indiscernible possibilities for a space-time to be or redundant structural elements in our fundamental

[2] In his letter to Max Newman, included in his Autobiography.

space-time theories given substantivalist assumptions and that relationism is unworkable. I will close this talk by suggesting ways in which the artifactualism borne out of a Russellian *ESR*ist approach can ameliorate some of the discomforts inasmuch as some of the assumptions required by our representations necessarily give rise to artifacts of representation with redundant structure somewhere, but that this is a feature in this case and doesn't carry heavy-duty metaphysical burdens.

Concept Defectiveness and Amelioration

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Concepts, like theories, come in various shapes and sizes. Some are narrow, others broad. Some are rigorous, others irreparably tethered in intuition. Some embody ideals of simplicity and unity, others exhibit intricate and tangled parts. Concepts can also be said to perform their epistemic duties more or less adequately and tend to succeed one another in history. In this talk, I explore the parallel lives of scientific concepts and theories with a view to an improved understanding of the structure and dynamics that underlie their formation, proliferation and elimination. To be more precise, I take a closer look at what happens when scientific concepts rival each other and offer some practical suggestions as to how we might go about picking winners. Among the various cases under consideration, I include those that concern ceteris paribus clauses, reasoning by analogy and debates that are at an impasse. The general picture I draw is one of science that can learn from its past mistakes by utilising formal tools (particularly logic) to diagnose and remove defective elements, the ultimate aim being that of providing more refined concepts and, by extension, a better understanding of the world.

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