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Storytelling, statistics and hereditary thought: the narrative support of early statistics

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Abstract

This paper's main contention is that some basically methodological developments in science which are apparently distant and unrelated can be seen as part of a sequential story. Focusing on general inferential and epistemological matters, the paper links occurrences separated by both in time and space, by formal and representational issues rather than social or disciplinary links. It focuses on a few limited aspects of several cognitive practices in medical and biological contexts separated by geography, disciplines and decades, but connected by long term transdisciplinary representational and inferential structures and constraints. The paper intends to show a given set of knowledge claims based on organizing statistically empirical data can be seen to have been underpinned by a previous, more familiar, and probably more natural, narrative handling of similar evidence. To achieve that this paper moves from medicine in France in the late eighteenth and early nineteenth century to the second half of the nineteenth century in England among gentleman naturalists, following its subject: the shift from narrative depiction of hereditary transmission of physical peculiarities to posterior statistical articulations of the same phenomena. Some early defenders of heredity as an important (if not the most important) causal presence in the understanding of life adopted singular narratives, in the form of case stories from medical and natural history traditions, to flesh out a special kind of causality peculiar to heredity. This work tries to reconstruct historically the rationale that drove the use of such narratives. It then shows that when this rationale was methodologically challenged, its basic narrative and probabilistic underpinings were transferred to the statistical quantificational tools that took their place.

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1. Introduction

Historians of statistics have shown how some of the main developments of early statistical analysis within the late nineteenth-century British school that flows from Galton to Pearson to Fisher were prompted by a desire to understand and control hereditary influences.[1] My contention in this work is that these developments were in part the consequence of a shift from an earlier stage of rhetorical and representational structure arising from age-old traditions of case collecting and storytelling (with their basically narrative underpinning), to a statistical stance. Although plausibly a more general story could be told for this development I focus mainly on the role played by hereditary concerns in medicine and natural history. I thus shed light on a restricted thread within a much wider and more complex set of historical developments that moved biologists and other life scientists from a mid nineteenth century conception of heredity as a (mysterious, complex, vital) 'force' to a late nineteenth and early twentieth century notion of a materially based, mechanistic, transmission law that could be modeled statistically. The basic claim I want to make is that statistical modeling of hereditary transmission, and the statistical inferences based on it, would not have been a move so easily made if a previous narrative grounding (in the form of exemplary case stories and genealogies) had not been present to give intuitive explanatory support to the use of accumulated tabular collections of facts.

A growing consensus points towards the idea that the basic structure of our modern concept of biological heredity was progressively constructed during the first decades of the nineteenth century in the relatively marginal domains of animal breeding, horticulture and alienist medicine.[2] In several places I have argued that a crucial contribution was made by european medical men preoccupied with stabilizing the concept of hereditary disease. In my view, by the mid nineteenth century the main criteria that medical men had accepted for considering a trait or a disease as hereditary were:

1. that it could be shown to run in families or in wider genealogical groupings such as tribes, nations, or races.
2. that it was unequivocally transmitted to the offspring by parents through reproduction at the moment of the first formation (leaving out alternative routes such as maternal imagination, nursing or contagion *in utero*); and most importantly,
3. that it was produced by a causal influence that gave not the disease itself but a predisposition to it, which made it capable of remaining latent, either during part of the life of the individual (and only expressing itself at a given stage) or by sometimes not affecting the bearer but its direct or indirect offspring (giving rise to atavisms or reversions).[3]

These features of heredity were not originally established within the medical world using any kind of inductive statistical processing of data but by what we may call extensive Hippocratic case story collecting and posterior causal case-by-case analysis.

Within a long naturalist tradition of collecting stories about peculiar and wondrous happenings, a special medical tradition can be situated which deals with strange and some-

[1] Mackenzie (1981), Cowan (1977a), Hilts (1981), Porter (1986), Waller (2001a), Gayon (1998).
[2] López-Beltrán (2004), Rheinberger and Müller-Wille (2003), Gayon and Zallen (1998).
[3] López-Beltrán (1994, 2004), Waller (2001a, 2003), Wilson (2003), Cartron (2003).

times wondrous peculiarities running through families and wider genealogical groups. Cases and stories of different kinds were collected for centuries, mainly by physicians and naturalists, and were told and retold in their writings, about possible instances of hereditary transmission of all sorts of physical rarities: big noses, left-handedness, spectacular moles. Of course, instances of the possible transmission of the weirdest traits had a greater 'memetic fitness' and were passed down through many centuries. One can trace some of these stories to ancient medical treatises, others to medieval or modern sources. Typical examples, found over and over again until the first half of the nineteenth century, range from big-headed Roman families, to red-haired clans in the Scottish Highlands, to warty families in Tuscania. A dark side to these genealogical hereditary stories is linked to the racist association of evil qualities with some ethnic groups, typically Jews and Gypsies. A wider background, which I will not address here, concerns the Judeo-Christian articulation of history and time in terms of genealogies and generations, and on the darker side with the Christian notion of hereditary Original Sin.

As with most marvelous stories, hereditary cases were often accorded different degrees of credibility, but most of them—even the quite fantastic ones—were carefully registered and transmitted from generation to generation, especially by medical men in their collections of cases. As I will argue below, the age-old Hippocratic tradition of structuring medical knowledge in the form of case stories played a core role in the continuous acceptance of the importance of story collecting, by granting it some epistemological utility.

Historians of biology have not given proper attention to the role of storytelling (and narrativity in general) in the construction of concepts in biological sciences. There survives a deep prejudice against these resources within the natural sciences and their epistemology. Among medical men, as Kathryn Montgomery Hunter (1991) has shown, case stories and anecdotes are widely used in the day-to-day depiction and structuring of information, but kept in the cupboard when serious methodological issues are discussed. According to her, the prejudice that began in the nineteenth century with the onslaught on storytelling (and the promotion of statistical approaches) is nowadays dogma among medics.[4]

The inductive and constructive effect of accumulated stories (narratives) has only recently been given attention in the science studies literature. Although Lorraine Daston and Katherine Park some years ago showed us how isolating the wonderful after the Renaissance eventually forged 'a new category of scientific experience, the fact detached from explanation',[5] the further realization that singular stories (narratives), particularly in the medical tradition, could also become a locus of fact construction, and furthermore as a vehicle for localized inductive generalization, has only been explored in a few scattered works.[6] As Laín-Entralgo insightfully showed some decades ago, the medical strategy of collecting case stories is particularly apt for organizing factual domains in which irregularity and unpredictability are dominant. The empirical domain of what I have elsewhere called 'the hereditary' was for many decades and up to the middle of the nineteenth century progressively constructed by the continuous application of a storytelling strategy.[7] With hindsight it can be said that the idea of hereditary transmission of all sorts of peculiar constitutional or psychological traits fought its way to the forefront of the medical

[4] Montgomery (1991).
[5] Daston and Park (1998), p. 220.
[6] Laín-Entralgo (1950), Montgomery (1991), Epstein (1995).
[7] López-Beltrán (1994), Müller-Wille and Rheinberger (2004).

world by the weight of accumulation of stories, and the epistemological gains and losses of this procedure were built into early notions of heredity.

This fact is perfectly evidenced in Prosper Lucas' influential *Traité de l'hérédité naturelle* (1847–1850) in which he produces many case stories from all sorts of sources as positive evidence for each subdivision of his hairsplitting analysis of heredity. It has been written that for many of Lucas' contemporaries, it was his work which left beyond reasonable doubt the reality of hereditary transmission, and the existence of a particular domain of causation one could call 'heredity'.[8]

However, its excessive reliance on storytelling was vigorously criticized. With it one could 'prove' anything or its opposite. Some of Lucas' readers, like Francis Galton or Alphonse de Candolle, reacted strongly and typically against him, claiming that the only way to put the study of heredity on a firm grounding was to give it a basic statistical framework which could avoid the subjectivity and capriciousness of tales.[9]

I want to argue in what follows that the statistical enterprises that followed such suggestions made nevertheless implicit use of the basic structural achievement of the previous storytelling procedures, and used some of the presuppositions that validated medical narratives. I want to show that the tradition of collecting case stories, anecdotes and other narratives served an important role in establishing that a peculiar kind of causality (a non-deterministic, predispositional one) was at work during hereditary transmission. The job of stabilizing some causal sequences by looking at these stories helped produce the criteria I have given above for identifying the hereditary cause. Such narratives thereby fulfilled a precondition for the field of research we call biological heredity. Through the accumulated effect of narratives, a special kind of causal action was isolated and defined. In particular, *latent* causation became in a way the emblem of hereditary transmission, as different cases and stories established its peculiarities. A special kind of such cases based on genealogical following of traits was particularly suited for this task.

In the rest of this piece I will argue first that the long tradition of case history collecting within the medical world was an effective device for achieving some kind of descriptive and explanatory generality for the notion of hereditary transmission. In other words, that accumulation and classification of stories were shown to be a good resource for representation and conceptual structuring. Secondly, I argue that early statistical inferences regarding heredity would not have been possible if narratives had not been in place to give intuitive explanatory support to the use of classified collections of facts. Some of that support can be traced to what we may call the probabilistic Principle of Coincidence.

2. Case stories in medical history

The long tradition in Western medicine of using case studies (both singular case stories and collections of similar cases) as a way of depicting and organizing information was

[8] Chalmers (1910). The existence of an age-old empirical research programme is described there, which was focused basically on the gathering and organizing of facts about the transmission of hereditary characters from parents to children. Its principal upholders were physicians interested in hereditary ills. On the whole it was a tradition of research that did not fit easily with the rest of the brand new investigations of heredity that he describes with excitement in other sections of Chalmers' piece. Lucas was given credit there for taking that programme to its culmination in his (1847–1850).

[9] See Galton (1875), pp. 118–119, and Candolle (1873, 1885).

thoroughly investigated by the Spanish historian Pedro Laín-Entralgo in his trail-blazing though scarcely known *La historia clínica: Historia y teoría del relato patográfico*.[10] One of the most striking results of Laín-Entralgo's work is his description of how the use of case stories (and their interpretations) through the centuries has oscillated between two extreme positions: the first considers each case story as a singular occurrence, a particular storyline or narrative, only instructive as a tale might be—as a singular ocurrence not corresponding to any type. The second extreme is considering the value of cases only as indicators of possible typical paths—as token stories that are useful as long as they point outwards, towards a structuring inductive ascent towards typicality; and as long as they can be deployed in the production of an adequate classification of disease. Viewed in this light, stories can represent a general notion, a concept.

In a very famous piece Italian historian Carlo Ginzburg maintains that the Hippocratic tradition in medicine was basically a 'clue' or 'symptom' organizing activity in which stories were constructed in order to account for individual ocurrences as such. He describes this activity as thoroughly qualitative, and sees its object as individual situations taken as singular, contingent occurrences. Some historians of medicine, such as Oswei Temkin, agree with this description. Laín-Entralgo provides a counterbalance to this view. He shows that at several stages during the development of the tradition, cases are not seen or used as singular, token descriptions. In the process of choosing the elements that build up a case there is a tendency towards a specific kind of structuring that indicates that *typicality* is the aim, and the narrative points outside of its inmediate referent (the singular case) towards some kind of generality.[11]

Nelson Goodman insightfully wrote that often, when the elements of a narrative are rearranged, and specific kinds of elements in it are chosen (for instance fever, or delirium) which are given peculiar roles and significance, that is, turned into symptoms that point beyond themselves, singular narratives become something else—case studies, clinical histories in which there is at least an aspiration to exemplarity. A well chosen and presented case story, usually a striking enough one, can become thus a bearer of facticity, and furthermore of causal typicality.

This feature is a general one of narratives, and not exclusive of medical case stories. Jerome Bruner, writing about 'the narrative construction of reality', maintained that although narratives 'cannot be realized save through particular embodiment' there is often an emblematic nature to their particulars which brings 'suggestiveness' to some stories. This is what allows us to situate the case stories within wider more inclusive narrative types.[12] In her recent work aimed at furthering our understanding of case studies as 'explanatory narratives engendered in clinical medical practice', Julia Epstein defines the physician as 'a historian (*histor*) in a very basic sense: a chronicler of bodily events and systematic narrator of particular phenomena in a particular context'.[13] She adeptly makes

[10] Laín-Entralgo (1950). Recently, for instance, Julia Epstein mistakenly wrote that 'no comprehensive history has yet addressed the evolution and motivation of record-keeping in medicine from it Hippocratic origins to the present' (Epstein, 1995, p. 23).
[11] Ginzburg (1989), Temkin (1977).
[12] See Bruner (1991), Goodman (1980).
[13] Epstein (1995), p. 25.

use of some recent proposals in narratology to suggest that 'case histories participate in producing as well as recording what they observe' and in fact contain 'a narrative epistemology in its efforts encapsulate particular kinds of knowledge about the body. The case history's purpose, she continues:

> is to narrow the possibilities for disorder by a rigidly structured account that moves from first impressions to hypotheses to firm diagnosis.[14]

In his exhaustive historical revision of the use of medical case stories, Laín-Entralgo found five important tensions throughout the different stages and developments of this representational tool in medicine through the ages:

1. A tension between the individuality or singularity of the story and the generality to which knowledge aspires.
2. A tension between necessity and contingency in the succesion of the stages that constitute the course of each disease.
3. A tension between sticking to detailed evidence and wild speculation when judging the consequences of descriptions.
4. A tension between selecting only the meaningful symptoms and caring about the singularity and integrity of each individual existence.
5. A tension between a theoretical tendency and the operative or practical use of cases and stories.[15]

Basically, using case stories allowed the medical tradition to serve two masters simultaneously, which led to the tensions: first, the master of generality in knowledge; and second, the master of personal, singular attention to afflicted individuals and their maladies. On the one hand, story collecting could help organize the search for adequate nosological categories for useful classifications of disease and its typical morbid sequences.[16] On the other hand, it could respond creatively to singular, bizarre, unheard-of situations. Stories could range (and be classified) from the most common to the most extraordinary.[17] The tensions that Laín-Entralgo signals are linked to the epistemologically crucial issue of credibility. Thomas Laqueur has argued that a key feature of early medical reporting was that it 'needed to make its truth claims credible by dissociating itself from the popular tradition of sensationalist tall tales concerning prodigies, monsters'.[18] As Daston and Park have shown, this was only partially true, as the insistence in exceptional and singular occurrences allowed for the preservation and representation, in the form of catalogs and collections, of factual knowledge through dramatic 'coupures epistemologiques'.[19]

[14] Epstein (1995), p. 31.
[15] Laín-Entralgo (1950).
[16] Or as Julia Epstein puts it: 'Nosology relies on the accumulation of particular case histories that determine, in aggregate, a ''natural history of disease''' (Epstein 1995, p. 29).
[17] See López-Beltrán (1994), Ritvo (1998), Waller (2001).
[18] Laqueur (1990), quoted in Epstein (1995), p. 30.
[19] Daston and Park (1998). This is a point that both Bachelard (1938) and Canguilhem (1966) have also put across forcefully.

3. The hereditary and case histories

A domain of events as unpredictable and irregular as 'the hereditary' benefited greatly from this story collecting approach by medics and other naturalists. Its malleability allowed for the exploration and analysis of different descriptive and explanatory schemes. The impact of singular occurrences (and their accumulation) was not demonstrative but, as I aim to show, played a crucial evidential and rhetorical role in the eventual creation of the phenomenological domain of heredity. Although the complete organization and evaluation of the complexities and vagaries of hereditary transmission were beyond the reach of such epistemological strategies alone, the achievements it made possible have not been properly recognized.

I believe that the rationale that supported the use of story telling and case collecting as a way of constructing some kind of general scheme, at least in the case of hereditary transmission, can be described as the combination of 'the principle of coincidence' with the articulation of genealogies. During the nineteenth century the advance of new criteria for objectivity embodied in the statistical approach challenged and undermined the use of case collecting as a useful cognitive device in many fields of knowledge. The newly demarcated domain of heredity was an important battleground for this.

The first generation of western savants to adopt the noun heredity (*hérédité*) to refer to the phenomenology of peculiar character (trait) transmission from parents to offspring were the post-revolutionary French physicians who had been educated under the aegis of Philip Pinel and Etienne Esquirol in the 1810s and 1820s.[20] Heredity was then devised as a causal explanatory resource particularly useful for alienists and other public hygiene physicians, which were then emerging as a politically relevant group of experts.

Laure Cartron has looked recently into the details of this process, mainly conducted by Esquirol and his disciples, and has confirmed the basic role of the transition from case stories to statistical table construction. She writes that:

> The notion of pathological hereditary predisposition emerges at the point where clinical teaching converged with the use of medical statistics. Clinical observation of numerous cases ... could lead to certainty only if they were presented as statistics from which doctors could extract adequate probabilities.[21]

As both Cartron and myself have insisted, in his pioneering work Philipe Pinel only indicated (perhaps inspired by Maupertuis' successful use of probability arguments in analyzing hereditary causal links, as we will see below) that careful record-keeping would clarify the confusion between alternative causal contributions to mental diseases, and specifically would help sort out the importance of heredity. It was Etienne Esquirol, with access to an enormous amount of information from important French asylums and hospitals, who produced a series of statistical tables that seem to confirm the presence of a hereditary predispositional cause in many cases of insanity. It must be recalled that precisely during that period, there was an important debate within the French medical community about the utility or idleness of the 'numerical method' in the organization of empirical data originally gathered as singular case stories. In that context Esquirol wrote:

[20] López-Beltrán (1992, 2004), Cartron (2003).
[21] Cartron (2003).

> What is experience if not observation of facts? But sometimes memory is unreliable. Statistics record and don't forget. Before a physician puts forward a prognosis, he has done a mental calculus of probability and solved a statistical problem. Notice that he has observed the same symptoms ten, thirty, a hundred times in the same circumstances, and from this he draws his conclusions.[22]

Tabular arrangement of facts from individual cases would—if the correct probable causes were pinpointed—render the right balance for causal attribution. Hygienists could then know how to distribute their prevention and therapeutic strategies. Heredity, as has been discussed, was an ambiguous causal presence in this scheme as it could be used both to justify incurability and help substantiate the physical bases for the treatment of moral ailments.[23]

In any case, in looking into this early period of the establishment of heredity as a main constitutional influence, the transit for genealogical case stories to tabular handling of data makes evident the movement of previous facticity (and causal connectedness) from genealogical storytelling to statistical probabilism.[24]

In my view, in this area the transition from one approach to the other was facilitated by a similarity in the rationale behind use of some peculiar stories and the deployment of striking statistical results. In both cases the thrust of their rhetorical effect was in the idea that some coincidences are simply too improbable to be ascribed to the action of chance. In the case of hereditary transmission of peculiarities, coupling this probabilistic argument with the transformation of case histories into genealogical sequences made the inferential bases of the singular case to causal links even more robust. Eventually, this was the basic structure that was later used by hereditarian pioneers of statistical analysis to shift the emphasis from the narrative, case-based practice to the more idealized, abstract statistical representations. I will try to show this in the last (and longer) historical part of this work.

4. From storytelling to statistical tabulation

When Charles Darwin, in the chapter dedicated to inheritance in his *Variation of animals and plants under domestication* (1868), wanted to convince his readers of the pervasive and impressive action of hereditary transmission among living organisms he wrote:

> It is hardly possible, within a moderate compass, to impress on the mind of those who have not attended to the subject, the full conviction of the force of inheritance.

Darwin's solution to remedy this is to:

> select a few facts of the kind which, as far as I can judge, have most influenced my own mind.[25]

To produce well-selected, striking, authenticated cases of hereditary communication, and to collect and list them when persuasion about the reality of the phenomena was needed,

[22] quoted and translated in Cartron (2003).
[23] See Dowbiggin (1991), Cartron (2003), Ackerknecht (1967), Waller (2003).
[24] There is actually an intermediate stage between case history and tabular representation of disease, which is the tabular presentation of single case histories as formalized for instance by Francis Clifton (1732). See Epstein (1995), p. 41.
[25] Darwin (1868), Vol. 2, p. 4.

was an old procedure. Darwin felt it was a valid one, and as his first example he chose one of the more striking and much-mentioned cases: the English Lambert family of 'porcupine men' (first reported in the *Philosophical Transactions* in 1731[26]), 'whose skin was thickly covered with warty projections which were periodically moulted'. Deployed by many authors before Darwin,[27] the 'porcupine' story can be said to have been a 'standard case' that shows the transmission within a family of a dramatic variation, of a peculiarity that 'had appeared only once or twice in the history of the world' in an individual, 'but have reappeared in several of the children and grandchildren'. Some authors could not fail to make the inference that a constitutional alteration like this, transmitted repeatedly within an isolated (inbreeding) family, could easily be the origin of a new variety, a new race, or eventually, for some, a different species. Whatever the origin of these astonishing constitutional transformations, the fact that they could pass through generation into the offspring seemed proven. Explanations other than heredity of the coincidence of such deviations from type seemed highly implausible, so the singular case militates strongly for the hereditarian position. There are other typical cases that tend to reappear in the literature, dealing both with human beings and animals (mostly domestic). As Lucas' research so thoroughly proved, by the mid-nineteenth century there were cases, observed in the main by reliable authors, that could 'prove' in this way the (potential) hereditary transmission of variations of virtually any kind of character. And as Darwin also wrote, since 'the evidence of inheritance is more striking when we consider the reappearence of trifling peculiarities',[28] the proliferation of candidate cases for hereditary transmission knew practically no bounds. The existence of ancient collections of strange and sometimes unreliable ocurrences, were added to and transmitted as collections of unexplained curiosities by such recent authorities as Blumenbach, Haller and Meckel.[29]The sceptics, on the other hand, tended to point to the multiple exceptions, the unreliability of some descriptions, or tried to produce alternative explanations for the recurrences. Some, like the sadly notorious Dr. Knox, tended to react drastically. In reference to another multiply quoted example of dramatic hereditary variation, the Ancona Sheep, he flatly replied:

> When I am told that there is a short-legged race of sheep somewhere in America, the product of accident, my reply is simply, I do not believe it.[30]

The degree of scepticism that a case would generate varied according to the kind of feature that a case was supposed to illustrate—it depended on how well the feature was defined and how believable its dependance on heredity (on transmitted constitutional dispositions) was. As we have seen before, the case-collecting approach left room for all sorts

[26] Darwin is mistaken when he gives the date as 1755. The reference is: Baker (1731). The family was followed through several generations, and updates of the pedigree of the mutation were given later by William Lawrence, in his (1819), pp. 449–451, and by James Paget (1857), p. 192.

[27] Like Blumenbach (1786), Hunter (1786), Prichard (1813), Adams (1814), Lawrence (1819), Lucas (1848–1850).

[28] Darwin (1868), p. 5

[29] Blumenbach's collection can be found in his (1786), and Haller's in 'Similitudo parentum', Vol. I, Ch. IV, of his (1754). The tradition continued well into the nineteenth century: see for intance Gould and Pyle (1956).

[30] Knox (1850), p. 67. The original story of the Ancon sheep appeared in *Humphrey's Philosophical Transactions*, London, 1813, p. 85.

of scepticism. What turned out to be the strongest mid-nineteenth century challenge to hereditary inferences was precisely focused on the unreliability of such evidential procedures. It was raised not by a naturalist or a medic, but by a historian who was particularly weary of any talk of hereditary transmission of moral or mental characters: H. T. Buckle. Recognized by Darwin, Lewes, Galton and De Candolle as a well-articulated (though exaggerated) challenge to normal procedures, and by recent historians[31] as an important impulse for the incorporation of statistical method in hereditary inferences, the following paragraph appeared as a footnote in Buckle's renowned *History of civilization in England* (1859):

> We often hear of hereditary talents, hereditary vices, and hereditary virtues; but whoever will critically examine the evidence will find that we have no proof of their existence. The way in which they are commonly proved is in the highest degree illogical; the usual course being for writers to collect instances of some mental peculiarity found in a parent and in his child, and then to infer that the peculiarity was bequeathed. By this mode of reasoning we might demonstrate any proposition, since in all large fields of inquiry there are a sufficient number of empirical coincidences to make a plausible case in favour of whatever a view a man chooses to advocate. But this is not the way in which truth is discovered; and we ought to inquire not only how many instances there are of hereditary talent, &c, but how many instances there are of such qualities not being hereditary. Until something of this sort is attempted, we can know nothing about the matter inductively; while, until physiology and chemistry are not much advanced, we can know nothing about it deductively.[32]

Fascination with exceptional cases, and scepticism about them, or their use as empirical support for this or that theory, is perhaps one of the central characteristics of the story of heredity. Many recent historians have condemned that attitude as belonging to a pre-scientific period where midwife tales dominated over sound observational or experimental empiricism. Darwin himself has been accused of being too credulous. The emergence of statistical analysis as a sound base for inductive inferences (together with the increase in the use of experimental crossings pioneered by hybridists) has been rightly declaimed as a major breakthrough that broke the vicious circle of claim and counterclaim.[33] However, in this approval there is a lack of appreciation of the extent to which the case-quoting, storytelling approach to evidence has some continutity, both in its rhetorical workings and in it probabilistic rationale, with the statistical approach, specifically with the early statistics as they began to be employed by alienists, naturalists and other theoreticians in the mid nineteenth century and thereafter.

Buckle was actually being very partial and unfair when he described the procedure of case studies as irrational. As Darwin poignantly reminded his readers, many of the reocurrences in the same family of peculiar characters 'cannot be due . . . to a coincidence, but must be consequent on the members of the same family inheriting something in common

[31] Olby (1985), Hilts (1981).

[32] Buckle (1857), Vol. 1, p. 161. Quoted also by Lewes (1859), p. 376, and by Olby (1985) in an appendix.

[33] See for instance the accounts of Francis Galton's work in Cowan's several articles (1968–1977), and those chapters dedicated to the subject in Stigler (1987) and Porter (1988). A recent assessment can be found in Waller (2001).

in their constitution'. As he wrote in *The origin of species* (before he knew of Buckle's objections), this was because once the truth or reliability of the story is established, 'the mere doctrine of chances almost compels us to attribute its reappearance to inheritance'.[34] Darwin, as I expect to show, was not alone in feeling 'compelled' to believe that hereditary transmission rather than external influences or mere accidental recurrences were responsible for the observed events,[35] and a part of the seduction of the adequate case, anecdote, or story was certainly based on a rational probability assesment. Darwin, I will show below, followed eighteenth-century French physicist Maupertuis in adopting and adapting a probabilistic argument that could answer the sceptics.

The other, complementary aspect of the 'compulsion' to attribute a causal link is the rhetorical role of the stories themselves; their capacity to strike a chord in the imagination, and the preconceptions of the reader about narrations; they tend to associate them with a causal vertebration that links the succesive episodes. The intimate working of these two elements in the arrangement and presentation of the evidence was what gave such method of induction (so poorly regarded under other lights) the effectiveness it had for centuries, especially in the medical sciences.

The rhetorical, evidential power of narratives has elicited much interest among historians and philosophers of science recently, particularly after some literary scholars began to apply their analytical tools to scientific discourses like Darwin's.[36] Literary scholars have fortunately also paid attention, even before historians of science, to the early and mid-nineteenth century hereditarians in France and Britain. They could not fail to detect the powerful rhetorical and explanatory work that the storytelling and case quoting emanating from the medical tradition was playing in the hereditarians' works. In his insightful *Mythologies de l'hérédité* (1981), French literary critic Jean Borie, analysing Lucas' *Traité*, correctly decribes it being constituted of two well-defined parts. The first volume is dedicated to the aprioristic theorizing in which he distinguishes the specific and the individual types, and gives an account of his laws of heredity and inneity. The second volume is dedicated to the practical side, where proofs, arguments and examples are accumulated. In this part of Lucas' work (the one Darwin perused more thoroughly), Borie sees the emergence of:

> cent anecdotes, exposés des cas, rappels des faits historiques, en un mot, des personnages. C'est même le charme de tous les ouvrages entarsés par les archivistes de l'hérédité et de la déviance d'accumuler pour nous les pièces d'un immense et fabuleux musée: illustrations, atlas, arbres généalogiques, et surtout, exposés de cas, par dizaines. Un exposé de cas est toujours un recit biographique, un petit roman.[37]

[34] Darwin (1859), p. 13.

[35] Though elements like the 'distinctiveness' of the characters (so they could be clear enough to facilitate their identification), and the reliability and care with which the genealogical research had been undertaken, as well as the elimination of other explanations (like contagions in case of disease) were all important to Darwin, he relied entirely on the accumulation of facts, from all sort of sources, that took the existence of transmission beyond reasonable doubt.

[36] Beer (1983) is a leading figure in this area. See also Morton (1984), Borie (1981), López-Beltrán (1998a, 1998b), Malinas (1985).

[37] Borie (1981), p. 87.

Borie is interested in the narrative structure these hereditarian tales provided for nineteenth century writers like Zola,[38] but against Ginzburg he is aware of the inductive role they are playing in the medical men's quest for a general theory. 'Le médecin ne s'interesse, dans le cas qu'il étudie à rien d'individuel, il ne retient que les éléments typiques de la situation'.[39] This inductive performance has a part of its leverage in the fact that a striking, well knitted narrative will always have a causal backbone that can lead the reader through it in an assentive way. Whatever mechanism drives a story or an anecdote, there has to be a causal link between the succesive stages. A story, it can be argued, is always a way to exemplify a causal connection, general or not, in such a way as to impress the spectator. The individuality and details of the cases add life and poignancy to the experience of assimilating the message, but if there is not some kind of cause–effect thread, however fanciful or contorted, in relation between the initial and the final states, the efficacy of the tale diminishes. In the case of heredity stories, the connection between the parents' constitutional features (usually exceptional) and those of the children is is the narrative backbone: the casual implication is reinforced by the sense that no other account can compete with it (and here the probabilities enter).

The accumulatiom of heredity stories, although not always altogether reliable, had a probabilistic backbone that sustained the inferential movement from the storylines to probable causal links, and thence towards a more general and explanatory concept of heredity. Henry Lewes, for one, answered Bukle's challenge in the following way:

> It must be admitted that many of the cases collected to prove hereditary transmission have been allowed to pass unchallenged by criticism, and many of them are worthless as evidence; but is Mr Buckle prepared to deny that the tendencies and peculiarities of men depend on their organisations? If he is not prepared to deny this, his scepticism is illogical, since there can be no shadow of doubt that organisations are inherited. He will not say that it is mere coincidence which preserves intact the various 'breeds' of animals . . . Unless parents transmitted to offspring their organisations, their peculiarities and excellencies, their would be no such thing as a breed, or a race. The cur would run the same chance as the best-bred dog of turning out valuable.[40]

Two elements coincide here to undermine Buckle's scepticism towards storytelling: one is the strong belief that in the case of humans, and of domesticated breeds, there is a purpose in the increased variability that is observed: the perfectibilty (or its complement the capacity for degeneration) of the genealogical line. The other element, and perhaps in the end the stronger one, is the conscious or not application of the doctrine of chances to the the use of cases. The only way to explain the familial pattern of distribution of characters within given species, the argument goes, is an uneven distribution of chances due to the causal link provided by heredity between the members of the same genealogical line. The first time this chance analysis was explicitly made was in Maupertuis' well-known study of six-digitism in the Ruhe family in Berlin. In it the French eighteenth century savant wanted to demonstrate that hereditary transmission of a striking constitutional variation could take both the mother's or the father's route towards the offspring (thus

[38] A similar study for the British case has been done by Morton (1984) where he traces the influence of hereditarianism on writers like Thomas Hardy and Samuel Butler.

[39] Borie (1981), p. 87.

[40] Lewes (1859), especially Vol. 2, Ch. 12, 'The qualities we inherit from our parents', pp. 376–377.

undermining preformationism). Polydactily, having first appeared in a woman of the family (Elizabeth), had reappeared in three generations of Ruhes. Jacob Ruhe was the intermediary between Elizabeth and her grandchildren. Maupertuis stressed then that chance could have been responsible for the first, spontaneous, variation:

> But if one wished to regard the continuation of polydactily as an effect of pure chance, it would be necessary to see what the probability is that this accidental variation in a first parent would be repeated in his descendants.

He proceeded:

> After a search which I have made in a city which has one thousand inhabitants, I have found two men who had this singularity. Let us suppose, which is difficult, that that three other have escaped me; and that in 20,000 one can reckon on one six-digited: the probability that his son or daughter will not be born with polydactily at all is 20,000 to 1; and that his son and his grandson will not be six-digited is $20{,}000 \times 20{,}000$ or 400,000,000 to 1; finally the probability that this singularity will not continue during three generations would be 8,000,000,000,000 to 1; a number so great that the certainty of the best demonstrated things of physics does not approach these probabilities.[41]

In a recent analysis of Maupertuis views on hereditary transmission, Isabel Sandler correctly points out that in the above reasoning, he was using the probabilistic principle of coincidence, with an epistemological interpretation of probability.[42] What is interesting from our point of view is that such principle of coincidence means simply that two events, if they are not probabilistically independent, must be, directly or indirectly, causally connected (one must cause the other, or they must share a common cause). Sandler makes a point of insisting that Maupertuis understands chance (or probability) epistemologically, as a reflexion of ignorance. But it is the proof of causal connection that really matters for his argument. In the case that Maupertuis chose to explore, the explanatory alternative to hereditary transmission is that the reappearance of the same trait (polydactily) in different individuals of the same family is some external common cause. But even if such a cause existed, it seems highly implausible that it could really 'pick out' only the members of the family without them sharing at least an exceptional predisposition or tendency, which in its turn would have to be hereditary. Maupertuis' analysis makes evident what most believers in the hereditary transmission knew to be true: that their collection of cases, tales and anecdotes of familial patterns of reccurrence of peculiarities (both normal and pathological) warranted their inferences to hidden causes because there were no good alternative explanations. Many later authors were very impressed by Maupertuis' simple calculations (uncommon in biological problems) and Darwin himself made a similar, copy-cat argument in his *Variation*, exaggerating the proportions and leaving the pencil work to someone else:

> If the occurrence of the same unusual character in the child and parent cannot be attributed to both having been exposed to the same unusual conditions, then the following problem is worth consideration, as showing that the result cannot be due, as

[41] Maupertuis (1752), Lettre XIV, p. 308. The translation is taken from Glass (1959).

[42] In which she argues that Maupertuis was no precursor of Mendel, but provides however an insightful analysis.

> some authors have supposed, to mere coincidence, but must be consequent of the members of the same family inheriting something in common in their constitution. Let it be assumed that, in a large population, a particular affection occurs on an average in one out of a million, so that the a priori chance that an individual taken at random will be so affected is one in a million. Let the population consist of sixty millions, composed, we will assume, of ten million families, each containing six members. On these data, Professor Stokes has calculated for me that the odds will be no less than 8333 millions to 1 that in the ten million families there will not be even a single family in which one parent and two children will be affected.[43]

Darwin felt very confident that this calculus actually allowed him to take as proof all the evidence he had gathered of cases of hereditary transmission of variation from the medical, agricultural, zoological, botanical, and other sources (provided other causes were eliminated and the cases were well-authenticated). The sceptical views of Buckle and the like could be decently ignored. The rationality of using isolated cases as a base for inductions towards causal links, and the establishment of certain patterns of regularity within the irregularity of hereditary links was assured by Maupertuis' example of the probabilistic 'principle of coincidence'. Of course, as Maupertuis showed, and as Darwin was well aware, in order to prove hereditary transmission by this 'external' evidence, two important problems remained to be solved. One was the elimination of all the other possible causal sources that could compete with heredity, which could only be achieved by having groups as homogeneous as possible in all other respects (so the prior probabilities assigned by Darwin and Maupertuis would be justified). The remaining problem is the oldest one: the explanation of the exceptions, the roots of difference and inhomogeneity. It is on these two counts that storytelling needs the underpining of the background physiological theories, theories of reproduction, and more general assesment of the importance of other, external, influences. But in this 'weakness' storytelling is no different from the statistical evidential procedures that superseded it. In fact, rather than signifying a radical rupture with outmoded, irrational ways of dealing with facts, statistical procedures can be seen as a completely natural development of them—a way of clarifying the manner in which an external pattern of recurrences can legitimately be said to justify a belief in internal transmission. It must be added, however, that marking a distance from the uncritical use of fact quoting that had given some medical hereditarian studies a shady reputation was a part of the motivation behind the move to statistical procedures.

To state it directly: when statistical tables began to be used within the realm of hereditary theorizing they were a new way of telling stories, of capturing both the probabilistic causal logic underlying case quoting, and the rhetorical power of the narrative.

In relation to heredity, statistical tables began as organized collections of transmission stories that progressively lost their individuality and whose narrative detail became gradually abstracted. This is evident, for instance, in the way alienists, mainly in France, constructed and handled their first tables (modern authors say they did it 'naively'), and also in the way Francis Galton organized his genealogical, biographical data when he decided he wanted to prove the inheritance of talent.

When Francis Galton began collecting genealogies in the 1860s he decided he would eliminate the arbritrariness of case selecting by using lists of talented individuals chosen

[43] Darwin (1868), Vol. 2, p. 448.

independently of whether they were related to other talented persons. If it turned out that they tended to have more talented relatives than what would be expected from the 'a priori' probability of the population, then a case could be made for an hereditary link. Similarly, the alienists after Esquirol had considered that if the registration of insanity cases to their establishments were unrelated to the origin of the disease then there shouldn't be any bias towards the hereditary cause in the selection process, and if a high percentage of them turned out to have affected parents or relatives, then the inference was warranted.

Both in Galton's case and in the alienists' there were critics, in their own time, who noticed the unwarranted nature of their assumptions of having non-biased samples. It was also pointed out that they could not eliminate other possible explanations for the correlation between eminence (or madness) and parentage.[44] But Galton, like the alienists before him, was sure that with his procedure, he showed that the number of relationships between eminent persons was considerably much higher than what would be expected by pure chance, and so 'the over-whelming force of a statistical fact like this renders counter-arguments of no substantial avail'.[45]

Again, it is the probabilistic principle of coincidence which justifies the inference. But a 'statistical fact' seems more impressive than a simple occurrence. As Hilts has argued, Galton was well aware that some of the loopholes of his methodology left him exposed to obvious criticisms, such as failing to take adequate account of the social and class-related factors in the distribution of merit and honour, especially within certain professions which he chose as examples when he detailed his views in *Hereditary genius* (1869). As Jean Gayon has analysed in some detail, Galton's move after *Hereditary genius* was to distance himself from the (methodologically dubious) purely genealogical approach (in which the Maupertuis-style probabilistic rationale was explicit, as was quite evident in the narrative structuring of the argument about family histories) and developed a more abstract statistical scheme in which hereditary influences could be fragmented and divided up through the whole ancestral lineage. But Gayon himself detects a continuing tension in Galton's statistical maneuvers and his interpretations, which stems from the presence of old style hereditary and genealogical concerns.[46] Galton eventually surrenders when he recognizes that reversion is simply a statistical effect (and changes the name to regression), but it can be said that Galton's early intuitions were to underscore his whole statistical enterprise. His view of heredity always invoked the presence of ancestors in an actual body (in its parts) that had first to be proven beyond doubt and then quantified, and if possible manipulated. One could thus argue that to render the originally weak probabilistic argument more robust was then the aim of much of Galton's life work. This he had to do first by finding ways of showing as little bias in the selection of the samples as possible, and at the same time increase the definition and level of detail with which a given character could be followed through the generations. Second, by clarifying the kind of causation that could generate both the 'higher than expected by chance' link between the characters of

[44] As Olby (1985) has acutely pointed out, the reception of Galton's *Hereditary genius* was more positive than some believe, but not entirely uncritical. Though some of his critics, like H. Merivale, understood very well the logical structure of his attempts, and thus its flaws and shortcomings. This is clearly analysed in Olby's 'Galton's response to Buckle' (typescript). In case of the alienists' statistical analysis, the main critics were made by their many environmentalist critics. See Dowbiggin (1990), Williams (1994). See also Waller (2001a).

[45] Galton (1865), p. 160.

[46] Gayon (1998), Ch. 6.

parents and offspring, and the irregularities that plagued the field of heredity. The former aspect of his work was perhaps the most succesful. That success was acknowledged by Bateson when he wrote that it was Galton who had concentrated attention upon 'the outward facts of (hereditary) transmission'.[47]

With the increasing sophistication of statistical analysis that he introduced, as Baillarger had written many years before, it was no longer the fact itself of hereditary transmission of a given character that was at stake, but the details or structure of the relationship. Galton's invention of statistical correlation and regression, analysed and explained in different fashions by many historians due to their subsequent importance, can be described as the conclusion of a long process within the field of hereditary transmission in which the external evidence of certain patterns of recurrences was successively challenged by sceptics.[48]

5. Conclusions

The use of accumulating narratives as a cognitive and representational device aiming at constructing an explanatory causal concept like heredity was (historically) crucial in arriving at our modern view of hereditary transmission. There are different kinds of generality that can be attained through narratives. Some of them can be both explanatory and have a basic rational structure. In the case of the construction of heredity the underlying rationale seems to have been the probabilistic principle of coincidence. Such a principle provides a continuity between the use of narratives and stories and the use of statistical tabulations and causal analysis based on them. Early statistical developments found important support in the explanatory and intuitive expectations created by the long medical tradition of case-story collecting and storytelling.

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[47] Quoted in Olby (1993).

[48] Cowan's works (1968–1977) on this matter, though important, are now insufficient. Further accounts of this episode have been given by Olby (1985), Mackenzie (1981), Stigler (1986), Porter (1986), Gayon (1998), López-Beltrán (1992) and Waller (2001a) have given alternative and recent views. Baillarger's pioneering use of statistics in heredity is to be found in his (1844).

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