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# "Describing our whole experience": The statistical philosophies of W. F. R. Weldon and Karl Pearson

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ABSTRACT

There are two motivations commonly ascribed to historical actors for taking up statistics: to reduce complicated data to a mean value (e.g., Quetelet), and to take account of diversity (e.g., Galton). Different motivations will, it is assumed, lead to different methodological decisions in the practice of the statistical sciences. Karl Pearson and W. F. R. Weldon are generally seen as following directly in Galton's footsteps. I argue for two related theses in light of this standard interpretation, based on a reading of several sources in which Weldon, independently of Pearson, reflects on his own motivations. First, while Pearson does approach statistics from this "Galtonian" perspective, he is, consistent with his positivist philosophy of science, utilizing statistics to simplify the highly variable data of biology. Weldon, on the other hand, is brought to statistics by a rich empiricism and a desire to preserve the diversity of biological data. Secondly, we have here a counterexample to the claim that divergence in motivation will lead to a corresponding separation in methodology. Pearson and Weldon, despite embracing biometry for different reasons, settled on precisely the same set of statistical tools for the investigation of evolution.


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What are the various motivations for taking up the tools of statistics? Put differently, what is it that draws historical actors toward viewing their subjects in a statistical manner? Two answers traditionally present themselves.[1] First, one might use statistics to simplify vastly complicated data, reducing it to the mean in order to construct a picture of "the average man." This position is all but synonymous with the name of Adolphe Quetelet (1796–1874), who coined the very phrase *l'homme moyen* (Porter, 1986, p. 52). As Ian Hacking describes it, Quetelet began with the normal curve, previously derived as either an error curve or the limit-distribution of the result of games like coin-tossing, and he "applied the same curve to biological and social phenomena where the mean is not a real quantity at all, or rather: *he transformed the mean into a real quantity*" (1990, p. 107, original emphasis). This shift created not a real individual, but rather "a 'real' feature of a population" (1990, p. 108). Quetelet then uses his average man to "represent this [population] by height [or some other character], and in relation to which all other men of the same nation must be considered as offering deviations that are more or less large" (Quetelet in 1844, quoted in Hacking, 1990, p. 105).

On the other hand, one might use statistics to attempt to model diversity, to study a statistical distribution with the intent of capturing outliers. Francis Galton (1822–1911), as Hacking tells the story, is a paradigm of this motivation for statistical study. Galton concerns himself, again on Hacking's picture, with "those who deviate widely from the mean, either in excess or deficiency" (Galton in 1877, quoted in Hacking, 1990, p. 180).[2] Hacking calls this a "fundamental transition in the conception of statistical laws," a shift toward Galton's "fascination with the exceptional, the very opposite of Quetelet's preoccupation with mediocre averages" (1990, p. 181).[3]

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[1] For example, in Porter (1986), Hacking (1990) or even Igo (2007).

[2] It is notable that this picture of Galton is up for debate—I thank an anonymous reviewer for pointing out that Galton's work on composite portraits (e.g., Galton, 1879) looks much like Quetelet's use of "averaging."

[3] A more metaphysical and less "historicized" version of this thesis is discussed in illuminating detail by Sober (1980).

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A larger conclusion is usually drawn here. The precise statistical methods one uses will, it is said, greatly diverge depending on which motivation one has for taking up statistical practice, a fact which should perhaps not at all surprise us. After all, as Larry Laudan famously argued, our methods "exhibit the realizability" of our aims, and those aims in turn justify our methods (1984, p. 63). In the specific case of statistics, Victor Hilts goes so far as to claim that this is a fair explanation of the fact that Galton, rather than Quetelet, made the first steps toward the theories of regression and correlation (Hilts, 1973). Hacking summarizes this position: "Thus where Quetelet was thinking of a central tendency, and hence of the mean, Galton, always preoccupied by the exception, was thinking of the tails of the distribution, and of the dispersion" (1990, p. 185). This differing emphasis led to Galton's focus on correlation coefficients, and hence his derivation of the theories of regression and correlation. In another example from biology, Nils Roll-Hansen describes Wilhelm Johannsen's use of statistics in terms similar to Quetelet's: quoting Johannsen writing in 1896, he notes that the normal curve "described how 'the various properties of individuals belonging to a species or race vary around an average' expressing a 'type'" (2005, p. 44). Further, this motivation led to his rejection of "'German dogmas' like the law of correlation claiming that certain traits were linked and could not be separated" (Roll-Hansen, 2005, p. 44). Again, we have Quetelet-inspired aims precluding the use of Galtonian methods.

Let us move a bit farther ahead, to the most influential and important disciple of Francis Galton: Karl Pearson (1857–1936). The work of the biometrical school of Karl Pearson and W. F. R. Weldon (1860–1906) around the turn of the twentieth century provided one of the most significant contributions to the debate surrounding heredity and variation in the period between the death of Darwin and what has been called the "eclipse of Darwinism" created by the advancement of Mendelian genetics and other non-Darwinian theories of variation (Bowler, 1992; Huxley, 1942). Pearson was a pioneer in statistics, and his work on evolutionary theory was regularly interspersed with studies in statistical theory, the latter often being derived as needed to solve the problems of the former.

With respect to the motivational dichotomy with which we began, Pearson is generally remembered as a Galtonian, having taken over leadership of Galton's Eugenics Laboratory (Magnello, 1999a, 1999b) and written a laudatory, three-volume biography of Galton (Pearson, 1914, 1924, 1930). Weldon, to the extent that he is ever considered independently of Pearson, is squarely placed in the same camp, having published his first statistical-biological article under the direct mathematical guidance of Galton (Weldon, 1890). This gives them both, and the biometrical school in turn, a very obvious place within the history of statistics.

I wish to argue for two related theses in light of this traditional view. First, if we look at Weldon's philosophy and motivation on its own, independent from that of Pearson, we can, despite their mutual connection to Galton, see an important and subtle difference between the two men with respect to their motivation for engaging in statistical practice. Pearson views statistics as part of a project consistent with his broader positivist philosophy of science—statistics is an appropriate tool to bring to biological data in order to simplify them and reduce them to their underlying mathematical laws. Weldon, on the other hand, appears more focused on the preservation of diversity, arguing that only statistics allows us to take account of the real variability present in the biological world.

Having made such a distinction, however, we can see an immediate and related problem in this common narrative in the history of statistical practice. For while Pearson and Weldon significantly differ in their motivation for engaging in statistics, they use precisely the same statistical methods—the highly rigorous mathematical tools of biometry. In other words, it is entirely possible to enter the practice of statistics with differing motivations and subsequently converge upon the same statistical methodology. Pearson and Weldon provide us with a spectacular, as well as unusual, example of such a case.

I will begin by attempting to lay out a new view of Pearson's motivation for engaging in statistics, consonant with his philosophy of science, his prescriptions on methodology, and the conclusions of recent biographical work. I will then consider a much-neglected debate between Weldon, Pearson, and a few of their opponents. We find here our first evidence of the distance between Weldon and Pearson—a philosophical disagreement that one would not expect on the traditional view of their relationship. I will then turn to developing a new conception of Weldon's motivation for engaging in statistics, grounded in a broader reading of Weldon's own philosophy of science, reconstructed in particular from the few sources in which Weldon self-consciously reflects on questions of philosophy and motivation. Weldon's view of science brought him to statistics by a profoundly different route than the positivism of Pearson.

There is a substantial body of literature on the history of biometry, particularly on the contentious debates between the biometricians and various proponents of discontinuous (and later, Mendelian) evolution, including William Bateson.[4] Weldon's work, however, has generally been seen only within Pearson's shadow.[5] I hope, in the end, to demonstrate that the lack of study of his thought is much to be regretted: Weldon's philosophy of science, and his reasons for adopting the biometrical method, are far more interesting than the usual stories would lead us to believe, and can direct us to insights not just about Weldon himself, but also about Pearson and even the general history of the development of statistics.

## 1. Pearson and statistics

In addition to being a pioneer in statistics, Pearson was a profound philosopher of science in his own right, and was intensely reflective about his methodology and motivations. His philosophy of the physical sciences in particular, as expressed in his completion of W. K. Clifford's *Common Sense of the Exact Sciences* and his own *Grammar of Science,* was extensively developed, and, while formulated independently from the views of Ernst Mach (with whom Pearson corresponded only late in his career),[6] bears much resemblance to Mach's positivism.[7]

Jean Gayon offers us a helpful place to begin by condensing Pearson's philosophy of science into three broadly positivist tenets: (1) science rests ultimately only on *phenomena;* (2) scientific laws *economize* our thought regarding these phenomena (by reducing them to mathematical formulae); and (3) science must not engage

[4] Notably, I am forced to pass over the importance of this debate to the sociology of science; see Mackenzie (1978, 1979, 1981) and Norton (1978) for the sociological perspective, as well as Roll-Hansen (1980) and Olby (1989) for qualifications. See also Kim (1994) for a helpful dissection of the various classes of participants in these debates. Finally, see Cock & Forsdyke (2008) for a biography of William Bateson.

[5] For Weldon, the best biographical source is still Pearson's obituary (1906), though see, for example, Radick (2011), with significant insight into Weldon's work. Pearson's life is extensively detailed in Porter (2004).

[6] See Thiele (1969) for their correspondence.

[7] While several authors, such as Alexander (1964), Kevles (1985), and Plutynski (2006) argue that Pearson is best seen as a "Machian," Porter's recent biography places such a causal connection between Mach and Pearson in substantial doubt. See Porter (2004).

in *metaphysical speculation* (Gayon, 2007). Biometry can be readily seen to exemplify all three of these basic principles.

First, we have the phenomenological basis of science. Biometry consists crucially in the search for *empirical* trends in *observed data.* The extent to which this was adopted as a central claim in biometrical methodology can be seen as early as 1893, in the first paper produced from the collaboration of Pearson and Weldon. In it, Weldon claims that statistical investigation is "the only legitimate basis for speculations" regarding evolutionary theory: the study of phenomena is the only appropriate method in biology (Weldon, 1893, p. 329).

Second, we may turn to the economization of thought by mathematics. Pearson seems to adopt this unequivocally, equating the concepts of formula, law, and cause—all natural laws are merely mathematical formulas, and to describe the causes at work in a system *just is* to describe the laws (or formulas) governing it. Most directly, he says in the *Grammar of Science* that the last step of the scientific method is "the discovery by aid of the disciplined imagination of a brief statement or *formula,* which in a few words resumes the whole range of facts. Such a formula...is termed a *scientific law.* The object served by the discovery of such laws is the economy of thought" (1892, p. 93). Further evidence for this view may be found throughout his other work on biometry. In one of Pearson's many "Mathematical Contributions" articles, he mentions, regarding fertility, that "if it be correlated with any inherited character ... then we have a source of progressive change, a *vera causa* of evolution" (Pearson, Lee, & Bramley-Moore, 1899, p. 258). This cause is to be investigated, not merely by asserting the existence of a correlation, but by determining the precise mathematical law which relates the quantities at issue (Pearson et al., 1899, p. 267). Pearson is noticeably silent about what would constitute the appropriate mathematical laws for biology, but it might be inferred, on the basis of his enthusiasm for his version of Galton's Law of Ancestral Heredity, that this was the sort of thing he had in mind: a law which could tell us the expected deviation of an offspring from the generation mean based on the characteristics of its parents, grandparents, and so on.[8]

Such claims abound in Pearson's *Grammar of Science.* Commenting on the concept of "laws of nature," he says that

> law in the scientific sense only describes in mental shorthand the sequences of our perceptions. It does not explain *why* those perceptions have a certain order, nor *why* that order repeats itself; the law discovered by science introduces no element of necessity into the sequence of our sense-impressions; it merely gives a concise statement of *how* changes are taking place. (Pearson, 1892, p. 136)

This view of laws supports the understanding of science as economizing our thought from, as it were, another direction—by claiming that natural law, the supposedly basic explanation for the necessary connections holding within nature, cannot perform the role demanded of it by traditional ideas of causality.

Importantly, Pearson's view of causation creates a high bar for science—we must know quite a bit about the system under investigation in order to construct relationships of the sort that he demanded. In a paper read at the end of 1895 and published in the *Transactions of the Royal Society* for 1896, Pearson seems skeptical that biological causes can be found, given the current level of knowledge: "The causes in any individual case of inheritance are far too complex to admit of exact treatment; and up to the present the classification of the circumstances under which greater or less degrees of correlation ... may be expected has made but little progress" (Pearson, 1896b, p. 255). That is, the complexity of biological systems makes the project of delineating their formal structure with precision incredibly difficult, and the completion of such a project has, in Pearson's view, been far from successful.

One more example may be cited. In the second edition of the *Grammar of Science,* published in 1900, Pearson adds the following (my emphasis):

> In the last chapter we freely used the words 'evolution' and 'selection' as if they had current common values. Now this is very far from being the case, and it is accordingly desirable to give to these terms and to other subsidiary terms definite and consistent meanings. It is only within the last few years, however, *with the growth of a quantitative theory of evolution,* that precise definition of fundamental biological concepts *has become possible.* (Pearson, 1900, p. 372, emphasis added)

It is worthy of note that in the intervening years between 1895 and 1900, Pearson seems to have become substantially more optimistic about the odds for success of a "quantitative theory of evolution." Pearson sees the introduction of biometrical methods as the only way by which we can expose the true scientific, lawlike, or causal (all three identical for Pearson) foundations of biological concepts. This position might seem odd, until we consider that such a grounding for biology consists of a description of the mathematical dependence of phenomena on one another. In this light, Pearson's philosophy of science appears broadly unified.

This focus on statistical/causal laws was also noticed by Pearson's son, who, in his two-part obituary for his father, mentions that, given the tenor of the nascent biometrical method as espoused in the first (1892) edition of the *Grammar of Science,* this process was all but inevitable:

> Looking back it is easy to follow where these trends of thought led, almost at once, in action: to an interest in Galton's Law of Ancestral Heredity; to a more accurate statement of this Law, involving the development of the theory of multiple correlation; to the testing of its adequacy as a descriptive formula by an extensive collection and analysis of data on inheritance .... (Pearson, 1936, pp. 216–217)

In other words, the very essence of the biometrical school, for Pearson, led almost inexorably to the utilization of an entirely functional notion of cause—the attempt to flesh out descriptive, mathematical laws which can summarize extensive amounts of data.

Finally, we may turn to the third positivist tenet underlying Pearson's philosophy of science, the avoidance of "metaphysical speculation." Arid theorizing about the material basis of heredity or the precise physiological or causal significance of observational results, Pearson argues, will do nothing but damage the progress of the science. Empirical grounding is the way to avoid mere blind guessing, as Weldon, collaborating with Pearson, insisted in 1895:

> These [statistical results] are all the data which are necessary, in order to determine the direction and rate of evolution; and they may be obtained without introducing any theory of the physiological function of the organs investigated. The advantage of eliminating from the problem of evolution ideas which must often, from the nature of the case, rest chiefly upon guess-work, need hardly be insisted upon. (Weldon, 1895a, p. 379)

This claim rings strongly of both a grounding in phenomena and a reticence to engage in metaphysical speculation unwarranted by available data. Even more striking is Pearson's complaint, expressed in his extended 1896 article on panmixia (i.e., random mating, or, for Pearson, the effect of completely random interbreeding *without*

8 See Froggatt & Nevin (1971) for more information on the form and development of the law of ancestral heredity.

the influence of natural selection), that the current lack of progress in biology is

> largely owing to a certain prevalence of almost metaphysical speculation as to the causes of heredity, which have usurped the place of that careful collection and elaborate experiment by which alone sufficient data might have been accumulated, with a view to ultimately narrowing and specialising the circumstances under which correlation was measured. (Pearson, 1896b, p. 255)

When we look at Pearson's considered philosophy of science, then, it is no wonder that he found himself attracted to the biometrical methodology. Kevles describes Pearson as being drawn to biology because it was "rife with speculative concepts ... that purported to explain vital phenomena yet were beyond operational test. He found [the biometrical] program appealing because of its positivist determination to deal only with directly observable quantities" (Kevles, 1985, p. 29). And a further conclusion can be drawn. Pearson's work, throughout his revisions of the *Grammar of Science,* remained emphatic about the usefulness of science for the *economy of thought.* The complexity of organisms is undeniable, as is our relative inability to specify with any true precision their internal workings. Biological data is thus a vast, tangled web of observations—on various characteristics, of different organisms, at different times, in different environments. We need the statistical method in biology so that we can *simplify our way out of this tangle:* only through statistics can we hope to offer economized laws of nature, which can encapsulate this data in a comprehensible manner. E. S. Pearson, writing about his father's reasons for leaving the study of evolution, claimed that "in the growing complexity of the Mendelian hypothesis," Pearson "could not see those simple descriptive formulae which held so important a place in his conception of scientific law" (Pearson, 1936, p. 241).

Statistics is thus useful for establishing the *vera causa* of evolution, as Pearson describes it, not only because it lets us capture the outliers in some particular statistical distribution, but more importantly because we can *then* continue onward, simplifying and economizing our thought regarding biological systems, distilling those biological processes into mathematical, functional laws—the only things which Pearson would recognize as "causes." This, then, is Pearson's motivation for engaging in the biometrical program—the reduction of biological complexity to simple, functional laws, phrased in terms of statistics.

Two clarifications of Pearson's philosophy should be raised here. First, we have substantial evidence that Pearson's philosophy of science was not *merely* positivist. It was, as detailed especially by Porter (2004), a strange amalgam of influences, some of which are positivist, some based in German idealism, and some grounded in Pearson's political views. None of these other philosophical inclinations, however, do a better job than positivism of explaining Pearson's motivation for engaging in statistics. Fundamentally, positivism is clearly a *sufficient* explanation for Pearson's use of statistics—statistics clearly does economize our thought in the way Pearson valued. Moreover, we have much direct documentary evidence that Pearson viewed statistics in a profoundly positivist manner—it is positivism to which he returns time and again throughout the very papers in which he elaborates the biometrical methodology, and it is positivism to which his son ascribes his reluctance to engage in Mendelian genetics. Such evidence is simply lacking with respect to any other explanation for the motivation behind Pearson's statistical project.

Second, it is certainly true that Pearson is often very concerned with the preservation of outliers, complex distributions, and so forth: Pearson can hardly be said to want to simplify or reduce all statistical distributions to mean values, as, for example, did Quetelet.[9] Pearson worked extensively with non-normal distributions, in what could be argued was an attempt to preserve their structure, or their variability (e.g., Pearson, 1894a, 1895). In a review harshly titled "Dilettantism in Statistics," Pearson rails against an investigator who reduces his data to a simple normal curve, discarding the distribution's important skewness, which Pearson claims constitutes the data's only important characteristic (Pearson, 1894b). Further, Stephen Stigler even reports that Weldon was angry with Pearson during the writing of the paper on crabs because he refused to take Weldon's suggestion to remove the outliers from his curves (Stigler, 1990, pp. 337–338).[10]

In evaluating this aspect of Pearson's work, we must be careful to separate two distinct features of Pearson's thought. On the one hand, we have his methodological prescription that we must always preserve outlying points, skew distributions, and so forth. On the other hand, we have the issue of Pearson's motivation for engaging in the statistical method in the first place. On the first point, I have no wish to argue that Pearson's statistical methodology was as simple as Quetelet's—such a claim is obviously ludicrous when applied to a mathematician as sophisticated as Pearson. But importantly, his technical methodology is consistent with several *motivations* for statistical practice—positivism foremost among them.

Turning to Pearson's harsh criticism of reduction to a normal curve, the "simple descriptive formulae" that Pearson's son described as so central to his father's view of scientific law should, to borrow an old cliché, be as simple as possible, but no simpler. I think we see in Pearson's critical review an instance of Pearson attacking oversimplification—a perfectly acceptable critique even on positivist grounds. At the same time, the reason that we engage in statistical work in the first place is because of its exceptional ability to provide us with the descriptive formulae that positivism places at the center of scientific research. Pearson and Weldon may have in fact disagreed about whether a given data point was (biologically) legitimate or not, but this disagreement fails to speak to the two men's fundamental motivation behind the practice of statistics.

It is to Weldon's motivation that we should now turn, beginning by attempting to separate Weldon's view of science from that of Pearson—pulling Weldon out from under Pearson's gargantuan shadow.

## 2. Weldon and the *Nature* debate

On the traditional reading of the relationship between Pearson and Weldon, we would expect the two men to view statistics in precisely the same way. As the story usually goes, Weldon is the empirically-minded biologist who approaches Pearson when he feels his experimental problems might be helped by statistical methods. Beyond this point, Pearson and Weldon are deemed to be all but philosophically, methodologically, and motivationally identical.[11]

It is understandable that this is the accepted reading of their relationship. Extracting a distinct view of Weldon's thought is a difficult enterprise for several reasons. No comprehensive biography of Weldon has yet been prepared, and he was a strict naturalist of the highest order—his published articles rarely stray from

[9] I thank Theodore Porter for encouraging me to review this side of Pearson's thought.

[10] In fact, Weldon is lamenting in this letter that Pearson is often more concerned with applying complicated statistical analyses than adhering to biological accuracy.

[11] Froggatt & Nevin (1971, pp. 3–4), describe them both as drawn to the same problems by the same reading of Galton's *Natural Inheritance.* Sloan (2000, p. 1071), and Norton (1978, p. 4), have a similar reading of their early relationship, though Sloan complicates Weldon's later development.

relatively straightforward reporting of the biological data which he devoted his entire (and, sadly, too-short) career to collecting. Weldon, unlike Pearson, very rarely stopped to consider the philosophical and motivational grounding of his own methods.

I will explore three sources in order to separate Weldon's motivation for engaging in the biometrical program from Pearson's. First, in this section, I will consider a debate which occurred in the correspondence pages of *Nature* between Weldon, Pearson, Joseph T. Cunningham, and E. Ray Lankester.[12] In the next section, I will examine two other sources—a lecture which Weldon wrote for a volume on the methods of science and the first paper Weldon wrote with Galton, before his collaboration with Pearson began.

The *Nature* debate is yet another chapter in the long and storied argument between the biometricians and their Batesonian (and later Mendelian) opponents. We should begin by setting the stage.

### 2.1. The opposition

The level of acrimony between the opponents and supporters of biometry around 1900 is indeed legendary. It would take a monograph to describe this conflict in detail, but a little context is useful here. We begin with the publication of Bateson's *Materials for the Study of Evolution* in 1894 (Provine, 1971; Sloan, 2000, p. 1074). Bateson worried, as Cock argues, about the twin problems of the usefulness of small variations and the difficulty of preserving variation over time (Cock, 1973, p. 8). These issues coalesced a community of scientists concerned with, first and foremost, describing the mechanism of heredity.

We can see many levels of disagreement between, on one side, Bateson, his allies, and even, as we will see below, scientists as diverse as E. Ray Lankester (a British Haeckelian and dyed-in-the-wool defender of Darwin) and J. T. Cunningham (a highly influential British neo-Lamarckian)—and, on the other, the early biometrical school of Pearson and Weldon. First, the biometrical method was highly technical. The life sciences had engaged in research for centuries without the aid of complex mathematics, and many practitioners saw no need for it now.

Secondly, the Batesonian group was convinced that the sort of variation that would resolve their problems would be *discontinuous*. Cock and Forsdyke (2008, part V) argue that if one issue can be said to have motivated William Bateson throughout his career, it is the conviction that there was something qualitatively different about the discontinuous variation responsible for the generation of new species. The biometricians, in contrast, were committed gradualists—staunch defenders of an orthodox Darwinism (Froggatt & Nevin, 1971, p. 10).

Further, Pearson and Weldon, as I noted in the first section, explicitly deemphasized in their biometrical methodology the discovery of the physical mechanism of heredity. The Batesonians, therefore, failed to see how Pearson and Weldon's statistical methods could even be *relevant* to the study of evolution. Observational work ought to intend, as that in Bateson's *Materials* did, to test and explain theories of heredity like theirs. These theories are not the sorts of things *even subject to* investigation using Pearson's unnecessarily complicated tools. A later, though representative, statement of the objection can be found in a critique by Bateson of one of Pearson's later works:

> ... much of the statistical work produced by Professor Pearson and his followers has, I believe, gone wide of its mark, if that aim is the elucidation of Evolution. More fitly might this work be described as "Mathematical Contributions to a Theory of Normality." [...] By the one word *Variation* we are attempting to express a great diversity of phenomena in their essence distinct though merging insensibly with each other. The attempt to treat or study [these phenomena] as similar [i.e., by using advanced statistics like Pearson's] is leading to utter confusion in the study of evolution. (Bateson, 1901, pp. 203–204)[13]

Looking at the statistics which so interested Pearson, Bateson claims, smoothes over precisely the sorts of differences we are concerned with capturing in the study of evolutionary variation.

This dispute was further complicated by some preexisting bitterness between Weldon and Bateson—Weldon had written an unfavorable review of Bateson's *Materials*, and Weldon and Bateson had argued at length in the correspondence pages of *Nature* in 1895 about an issue concerning the *Cineraria*, a genus of small, shrub-like flowering plants (Cock, 1973, p. 8). In the same year, Weldon had spearheaded the organization of the Evolution Committee of the Royal Society as a haven for biometrical work. After Bateson and his allies roundly criticized Weldon's article on the evolution of crabs (about which more later), Galton pressured Weldon to place Bateson on the committee. Bateson promptly took over and stacked the committee, causing Pearson and Weldon to finally resign in 1900 (Froggatt & Nevin, 1971, p. 9; Pearson, 1936, p. 228). Shortly thereafter, the hostile climate for the biometricians spurred the founding of the journal *Biometrika*, intended to be a place for them to publish their works without interference ([Weldon, Pearson, and Davenport], 1901). At this point, the dispute between Bateson and Weldon had become so bitter that Weldon called it "paltry and dirty beyond measure" (quoted in Magnello, 1998, p. 72).

Lastly, we have the role of Mendel. After the "rediscovery" of Mendel's paper and the publication of its translation in the *Journal of the Royal Horticultural Society* (Druery & Bateson, 1901), the Batesonians eagerly picked up Mendel's banner, in large part because they felt his theory would be a highly useful way to approach their concerns in both heredity and breeding (Darden, 1977; Olby, 1987).[14] Despite attempts by some at the time to synthesize the work of the biometricians and the Mendelians, Mendelian genetics rapidly became the front line in this controversy—and the Mendelians rapidly won converts.[15] From 1900 until 1906, the story for the biometricians is one of a steady loss of allies, as attempts were made to discover how the Law of Ancestral Heredity, a central biometrical principle which Pearson had extended from Galton's original formulation, might be related to Mendelian inheritance (Pearson, 1898, 1904; Weldon, 1902). Weldon's death in 1906 precipitated Pearson's retirement from the study of evolution, and he would attend only one meeting of the British Association after 1904 (Pearson, 1936, p. 231). Until the early synthetic work of Fisher (1918, 1922), leading to the later contributions of authors like Wright and Dobzhansky, the Mendelians carried the day.

### 2.2. The debate begins

I want to narrow the focus, however, to one particular debate between Pearson, Weldon, and two opponents that took place in the letters to *Nature* in 1895 and 1896. This exchange has been discussed before: Bowler (1992, p. 4) cites it as evidence that the Darwinians during the "eclipse of Darwinism" weren't able "to

[12] In general, these "paradigm articulators" (to use the phrase of Kim (1994)) are not well known, excepting Lankester (see Lester & Bowler, 1995). See Ankeny (2000) and Tabery (2004) for further study of two other important, smaller players in these debates.

[13] "Mathematical Contributions to the Theory of Evolution" was the title of a series of more than a dozen papers Pearson wrote during his collaboration with Weldon.

[14] See, however, Pearson (1908), Magnello (2004), and Porter (2005) for the complexity of the biometricians' response to Mendel. For the impact of Mendel in other fields, see Roll-Hansen (2000).

[15] On these near-syntheses, see Tabery (2004) and Morrison (2002).

maintain a unified front" against their opponents.[16] But this (admittedly accurate) portrayal conceals a very interesting aspect of the exchange: the insight it brings into the relationship between Pearson and Weldon on deep, philosophical points. First, a brief discussion of the context in which the discussion took place.

In March of 1895, Weldon published a summary of his seminal paper on the statistical analysis of measurements of the crab *Carcinus maenas*.[17] Weldon had collected extensive data on several morphological quantities of interest—one of which, "frontal breadth" (a relatively unimpressive morphological characteristic of these crabs), he claimed could be shown to be under selective pressure. Pearson's influence on the paper was extensive, as the amount of statistical work required to demonstrate the influence of selection was massive. First, one had to normalize for the simple *growth* of the crabs over their lifespan (a profoundly difficult statistical feat), and discard data for obviously wounded or malformed crabs. Once this was done, Pearson believed he had arrived at a *slightly* non-normal distribution for frontal breadth. This distribution curve could be factorized (using a new method which Pearson had just developed) into the superposition of *two* normal curves. The crucial claim was that this superposition provided evidence that the population itself was bimodal—that is, that natural selection had *split* the population into two sub-groups which were evolving away from one another. Weldon hypothesized that the selective pressure at work was due to the turbidity of the water at various places in the crabs' environment.

It is not surprising, given the tenuous nature of these inferences, that controversy soon developed. The first encounter on the *Nature* correspondence pages occurred when the botanist William T. Thiselton-Dyer submitted a letter commenting on Weldon's paper (Thiselton-Dyer, 1895).[18] Thiselton-Dyer believed that the statistical method could be used to shed light on the "stability problem"—the tendency of a "mean specific form" to be preserved in a population. This was quite a live question in the biological community in 1895—Galton would present a request to the Entomological Society just three weeks after Thiselton-Dyer's letter was published, asking "those who have had experience in breeding" for data bearing on "a theoretical question of much importance; namely, the part played in Evolution by 'organic stability'" (Galton, 1895, p. 155). Galton's proposal of the Law of Regression had attempted to formalize the observation that, as he put it, "offspring [plants] did *not* tend to resemble their parent seeds in size, but to be always more mediocre than they" (Galton, 1886, p. 246). It was a solution to this problem which Thiselton-Dyer believed he had spotted in Weldon's work.

But despite his optimism that statistical methods might be used to solve problems of regression, Thiselton-Dyer was more skeptical when it came to Weldon's methodological claims:

> I am not sure that I quite understand Prof. Weldon when he says that "the statistical method is the only one at present obvious by which [the Darwinian] hypothesis can be experimentally checked." In the first place, I should myself hardly call it experimental at all. In the next place ... in the important cases where evolution is actually taking place, the mathematical analysis appears to me to be beset with very great difficulties. (Thiselton-Dyer, 1895, p. 461)

Thiselton-Dyer, it seems, entirely missed the point—confirmed by the fact that *both* the biometricians and their opposition cite him as an opponent. He managed to read into Weldon a concern—namely, the demonstration of the law of regression to the mean—in which Weldon had little interest, and flatly dismissed as unintelligible the point that Weldon was actually trying to make. (As it turns out, the statistical method *is* highly amenable to the explication of regression to the mean—Pearson would publish a paper doing precisely that the following year (Pearson, 1896b, p. 306ff).)

Nonetheless, the floodgates had opened—spurred, no doubt, by Thiselton-Dyer citing one of the most controversial statements in Weldon's paper—and the parties to the debate quickly formed: J. T. Cunningham and E. Ray Lankester on one side, and Weldon on the other.[19]

Joseph T. Cunningham was a marine biologist and zoologist at the Marine Research Station at Granton, whom Bowler has called "an important but by no means typical British Lamarckian" (Bowler, 1992, p. 89). He was deeply engaged in the battle against Weismann; his obituary read that "he remained to the last one of the most eminent of the neo-Lamarckians" (Mudge, 1935, p. 42). E. Ray Lankester, on the other hand, "was one of the giants of late-nineteenth-century British science," and had positioned himself as "a champion of the Darwinian selection theory against Lamarckism" (Lester and Bowler, 1995, pp. 1, 87). But he was very interested in the German or Haeckelian version of the "problem of variation," espousing a theory of "correlated variation," according to which variation "is limited by the already selected and emphasized characteristics of the group. Every part ... varies in accordance with the constitutional tendency of the organism, which may be called its ancestral bias, or group bias."[20] With figures as influential as these lined up against Pearson and Weldon, and given the confusing content of the string of *Nature* letters, a reconstruction of the play-by-play is bound to be useful here.

The first letter following Thiselton-Dyer's was from Cunningham (1895), who brought with him an exceptional dose of methodological vitriol. After claiming that all Weldon had done was to show that some *future* demonstration of natural selection *might* be possible using statistical methods, he railed that:

> Prof. Weldon says that if we know that a given deviation from the mean is associated with a greater or less percentage of death-rate, we do not require to know how the increase or decrease of death-rate is brought about, and all ideas of functional adaptation become unnecessary. This may be his own state of mind on the subject, but I venture to state that it is not Darwinism, and that he cannot shut others out from the most interesting and most important fields of biology in this way. (Cunningham, 1895, p. 510)

Not only, then, are statistical methods good for delivering us little more than a promissory note on future results, Weldon's methodological prescriptions actually hinder the advancement of biological science.

After this outburst, the debate fell silent for a little more than a year. In June of 1896, Alfred Russel Wallace presented a paper to the Linnean Society regarding the existence and utility of the "specific character," or the set of characteristics that separate a species from the other members of its genus. Lankester wrote to *Nature*, ostensibly to comment on the views of Wallace. But his intent was clearly otherwise: he said outright that his "chief object in writing this letter is to draw attention to the views of Prof. Weldon" (Lankester, 1896a, p. 245).

As Lankester tells the story in his letter, during the discussion at the Linnean Society after the reading of Wallace's paper, he had argued for the importance of his "correlation of variation." Weldon declared Lankester's theory entirely irrelevant, because (again, as

[16] It is also briefly mentioned by Olby (1989) and Plutynski (2006).
[17] The original is Weldon (1895a); the summary printed in *Nature* is Weldon (1895b).
[18] This letter is also known for sparking the debate over the origin of the cultivated *Cineraria*, mentioned earlier (Froggatt & Nevin, 1971, p. 9).
[19] Some of the more interesting letters include Cunningham (1895), Lankester (1896a, 1896b), Pearson (1896a), and Weldon (1896a, 1896b).
[20] From the Lankester papers, privately held, quoted in Lester (1995), p. 89.

reported by Lankester) given a case of two characters, both of which are positively correlated with favorable selection, it is "absolutely impossible to separate these two correlated phenomena. The coloured skin is as much a cause of the survival of the dark man as is the germ-destroying property of his blood" (Lankester, 1896a, p. 245).

Lankester was dumbfounded. "I was not prepared," he laments, "for an empty wrangle in regard to the proper uses or improper uses of the word 'cause'" (Lankester, 1896a, pp. 245–246). Lankester immediately proceeded to a philosophical debate over Weldon's use of causation. He claimed that Weldon

> has deliberately departed from the simple statement which his observations warranted, viz., that such-and-such a proportion of frontal measurement accompanies survival, and has unwarrantably (that is to say unreasonably) proceeded to speak of the "effect" of this frontal proportion, to declare it to be a *cause* of survival, to estimate the "advantage" and "disadvantage" of this same proportion, and finally to maintain that its "*importance*" may be estimated without troubling ourselves to inquire how it operates, *or whether indeed it operates at all.* (Lankester, 1896a, p. 246, original emphasis)

I have quoted this passage at some length to give an idea of the philosophical level on which this debate took place. Importantly, we see a shift from the position of Cunningham to that of Lankester. For Cunningham, the problem is merely about the methodological claims of biometry: statistical investigation of correlation is fine, he seems to say, but it cannot constitute a replacement for comparative physiology and the investigation of functional adaptations (a point with which, we will see, Weldon actually agrees). Lankester, on the other hand, seems to indict Weldon for *philosophical* mistakes. Weldon's *underlying philosophy of science* is inadequate if it leads him to think that the discovery of correlation is sufficient to determine causal influence. Lankester's letter closes by asserting that biometrical methods "appear to me not merely inadequate, but in so far as they involve perversion of the meaning of accepted terms and a deliberate rejection of the method of inquiry by hypothesis and verification, injurious to the progress of knowledge" (Lankester, 1896a, p. 246).

Weldon responded to Lankester's letter with one of his own, and if there were any doubt that the argument had become genuinely philosophical by this point, his response should remove it. Weldon quoted, at length, Hume's definition, from the *Enquiry,* of cause as constant conjunction, and challenged Lankester as to whether he had the audacity to disagree with Hume (Weldon, 1896a, p. 294). Weldon stated he only ever intended to discuss cause under Hume's definition "or in Kant's extension of it [!]; but Prof. Lankester seems to go beyond it" (Weldon, 1896a, p. 294).

Lankester rushed to the defense of the Humean acceptability of his method, claiming that he merely desired, given the existence of two features correlated with some positive outcome, to engage in a process of hypothesis and experiment in order to determine the "true order and relation" of "a complex group of related phenomena" (Lankester, 1896b, p. 366).

Weldon, perhaps finally realizing in full detail the proposition with which he was disagreeing, beat a hasty retreat in August of 1896 (Weldon, 1896b). He explained that he was "far from rejecting the method of imaginative hypothesis and subsequent experiment and observation." "A complete knowledge," he wrote, "of the processes associated with this relation between frontal breadth and death-rate is a thing of very great interest, and I believe, as firmly as Prof. Lankester, that every effort should be made to attain to it" (Weldon, 1896b, p. 413). Though such a theory is quite hard to obtain (even, perhaps, impossible) due to the complexity of the interrelations of the organs of any organism, it must nonetheless be sought. Weldon would indeed undertake a series of experiments attempting to determine the influence on the crabs of the amount of silt in their water in 1897 and 1898, concluding that "a narrow frontal breadth renders one part of the process of filtration of water more efficient than it is in crabs of greater frontal breadth" (Weldon, 1898, p. 901; Pearson, 1906, pp. 26–27).

It is worth pausing here to note that this letter and Weldon's subsequent experiments, despite their appearances, do not necessarily constitute a *motivational* retreat on Weldon's part. For both the claims that hypothesis and experiment are a good way to guide our future statistical research, and that more detailed knowledge of the correlations at issue in a given biological system is a desirable thing, are fully consistent with Weldon's belief that the *correlations* are the complete and sufficient endpoint of biological research. But more on this in the next section.

Surprisingly, Pearson entered the debate at this point, with a response *countering* Weldon (Pearson, 1896a). Pearson began by arguing that the statistical analysis in Weldon's 1895 paper was far too simplistic to constitute a genuine verification of natural selection. Given the complex nature of the assumptions regarding the general growth of the crabs that had to be made, Pearson declares it "very improbable" that the true growth curve was found. Further, he writes that "when the law of the growth of crabs has been accurately ascertained ... I am convinced that it will require much more complex analysis than that of the *Report* to ascertain whether a selective death-rate does or does not exist" (Pearson, 1896a, p. 460). Then, Pearson turned to the defense of a concept of causation as *more* than mere Humean event-correlation. Even if the data in Weldon's 1895 paper were perfect, Pearson claimed, it would still not be enough to show that frontal-breadth *is the cause of* death-rate. "Very probably it may be, but the demonstration is not logically complete, or at any rate a definition of cause has been adopted which does not appear of much utility to science" (Pearson, 1896a, pp. 460–461). As an example, he turns to a discussion of artificial selection in cows, describing what contemporary philosophy of biology would recognize as the difference between selection *of* and selection *for*—both establish correlations, only one (selection *for*) involves causation. Without much more research, he claims, "it seems to me that a link is really missing in the chain of demonstration" (Pearson, 1896a, p. 461). After Pearson's input, the discussion tapered off.

On the traditional reading of the relationship between Pearson and Weldon, we should by no means expect a disagreement here, nor should we expect a disagreement of this kind. Pearson and Weldon, it is alleged, shared precisely the same view of science and the role of statistics. Yet it is on these fundamental issues that Pearson and Weldon disagree here. Let us consider a few more sources.

## 3. Weldon on statistics

In 1906, the year of his death, Weldon contributed a piece on "Inheritance in Animals and Plants" to a collection of lectures on scientific method (Weldon, 1906). Its breadth and central concerns are quite exciting—Weldon offers a sustained defense of the use of statistical methods in science, both generally and with particular emphasis on the biological sciences.

Why would statistical methods require a defense? According to Weldon, they inherently require a compromise—a methodological value judgment by a group of scientific practitioners. "Men measure a certain thing," he writes, "and find that up to a certain point their measurements agree with each other, and their experience is uniform; but beyond that point [i.e., in the very fine details], their experience is contradictory" (Weldon, 1906, p. 88). We can use statistics to smooth over these contradictory results (e.g., by averaging), but at a price—we must decide "how far the variability of the actual experience depends on imperfect observation, and how far it is a true record of differences in the thing measured" (Weldon, 1906, p. 88). What we truly desire—in Weldon's words, the "ideal description of every experience, the description which alone makes further progress possible" (Weldon, 1906, p. 93)—is the correct description of all the observed results, without having neglected any inconsistencies whatsoever. Physics and chemistry possess successful, general, formal methods because they have "succeeded in confining the limits within which these inconsistencies occur, so that the proportion of the whole experience affected by them is very small. But biologists have not yet advanced so far as this: the margin of uncertainty in their experience is so large that they are obliged to take account of it in every statement that they make" (Weldon, 1906, p. 93). That is, physics and chemistry have advanced theoretically to the point that these disciplines are confident that they can sort any discrepancies in their standard processes of measurement into clear, well-marked types: either they are a result of individual failings of experimenter or apparatus, or they are an indication that the fundamental theory needs to be refined.

Biology, however, is a different story. We do not yet know enough about the underlying structure of biological systems to know what constitutes important or unimportant variation. Weldon offers an example:

> If we want to make a statement about the stature of Englishmen, we must find a way of describing our whole experience; we must find some simple way of describing our whole experience, so that we can easily remember and communicate to others how many men of any given height we find among a thousand Englishmen. We must give up the attempt to replace our experiences by a simple average value and try to describe the whole series of results our observation has yielded. (Weldon, 1906, p. 94)

The gist is this: physicists have more knowledge regarding their experimental systems, and work tirelessly at searching for sources of error. All in all, they are in a much better position than biologists to render themselves confident that any variation within a set of measurements is due to operator error. They can then take the mean, and (given their precautions) assume that, for all practical purposes, they have arrived at the correct answer. Such a claim is consistent with Weldon's paradigm for statistics in physics being its use to correct for errors in observational astronomy (Porter, 1986, pp. 95–96). In biology, we cannot yet do this—due to the peculiarities of biological systems (especially their complexity), and our lack of knowledge, we can almost never simplify our observations in this way. We must therefore strive to preserve *all their details,* in a manner which still may be communicated—and, as you may have guessed, statistics is how we accomplish such a feat.

We should now have enough of a grip on Weldon's thought to try to articulate his view of biological method. Statistics is essential to the investigation of biological systems, due to their vast complexity and our lack of comprehension of their fine-structure. However, statistics is *not* essential for the reason we ascribed to Pearson—the primary motivation for using statistics is not to simplify or economize data for limited knowers, though this is certainly part of its merit. Its main purpose is rather *to permit us to retain* (as much as is possible) *the complexity of the biological world.* Any overzealous act of simplification (in Weldon's example, by doing things like substituting averages for statistical distributions) is equivalent to claiming that any remaining variation in our data is an artifact, not a feature of the outside world. We must use statistics (and keep the entirety of the statistical distribution intact) in order to hold on to the range of observational data which nature gives us.

We can see this same statistical philosophy in Weldon's earlier works. In his first publication to use statistical methodology, Weldon, with the help of Francis Galton, endeavored to draw conclusions from some data on shrimp (Weldon, 1890). By the standards of his later work with Pearson, his methods are horribly primitive—they simply consist in measuring the data's absolute deviation from a normal curve provided by Galton. The stated goal of the paper is to "determine the degree of accuracy with which [the] adjustment [of a local variety to its environment] is effected, and the law which governs the occurrence of deviations from the average" (Weldon, 1890, p. 445). In other words, since no character of any organism is *perfectly* adapted to its environment, and assuming that the mean value constitutes the (selective) optimum, to what extent do organisms deviate from this optimum? Galton had proposed a curve (the "law of error," known today as the normal distribution) as an answer to this question, extracted from his empirical studies, and Weldon aimed to test this proposed solution in his shrimp.

Interestingly, however, the conclusions of the paper are broader than this. Weldon collected his measurements, and noted that, indeed, all the characters he measured were normally distributed—with, however, different means and standard deviations in the three environments from which he collected the shrimp. But he also considers as confirmed a stronger proposal of Galton's, namely, that even though selective pressures "must vary in intensity in different places," "the frequency with which the observed deviations from the average occur is in all three cases expressed by a curve of error" (Weldon, 1890, p. 451). That is, though natural selection can alter the mean and standard deviation of a given character, the characters in *any* environment, under *any* selective pressure, will remain normally distributed.

We cannot infer from this paper anything like the statistical and methodological sophistication which would later come to the biometrical school after many years of developing its tools and techniques. But we can see, sixteen years before publication of the "Inheritance" lecture, the first glimmers of Weldon's statistical philosophy of science. The variations present in his shrimp, he writes, "depend not only on the variability of the individuals themselves (which is possibly nearly alike in all races), but also on the selective action of the surrounding conditions" (Weldon, 1890, p. 451). Clearly, these causes are too complex to allow our exact treatment. And we cannot rely on the mean values either, "for I am aware of no case in which the individuals composing any race of animals—however small and isolated the area in which they live, however uniform the conditions which obtain throughout that area—have been shown to resemble one another *exactly* in any character" (Weldon, 1890, p. 445). To accept the mean value is to discard important information about the population. In order to preserve necessary variation in the data, then, we must focus on the *normal distribution itself*—it is only at this level that we are permitted to draw conclusions about the population. Weldon's view here may lack the precision of his later formulations, but we see all of its important features.

Finally, we should return to Weldon's contribution to the *Nature* debate in 1895 and 1896. With this understanding of Weldon's statistical philosophy, we can easily see why he would adopt a "Humean" definition of cause. It is not an empiricist worry about the legitimacy of the imposition of a necessary structure on nature (*à la* Hume) that drives Weldon to such a position—rather, it is his recognition that the structure of biological systems is far too intricate to make such a determination possible. Correlation (which Weldon seems to think is similar to, or at least acceptable on the standards of, Humean "constant conjunction") is the only type of connection that can be drawn between biological systems of the kind Weldon was interested in investigating *without discarding some of the essential features of these systems.* To single out one cause and one effect is to commit precisely the same fallacy as substituting the mean for the statistical distribution—it is to unjustifiably decide that some aspect of one's data (whether variability or a consistent correlation) is *unimportant*.

It is also easy to understand Weldon's response to Lankester's persistent probing. For Weldon clearly thought that investigation into particular facets of biological systems was worth undertaking—he did so himself. And such investigation is fully compatible with his philosophy as I have laid it out here: there is nothing dishonest about attempting to understand more fully the detailed nature of organisms—as long as such knowledge does not come with the implication that some features of organisms are to be privileged at the expense of others. To use Lankester's example, "the coloured skin" and "the germ-destroying property of his blood" both *cause* "the survival of the dark man" on Weldon's view because they are both elements of the broad, complex picture of such a human being (Lankester, 1896a, p. 245). Any attempt to choose one of these *over* the other must necessarily discard vitally important information, unless and until the correlation itself breaks down (e.g., when we detect "coloured" individuals *without* the property of exceptional survival "in malarial regions").

Recall from the first section that Pearson's engagement with biometry is best interpreted in the light of his positivist philosophy of science. Statistical correlation, for Pearson, is one step in the process of determining the precise mathematical laws or causes underlying a given biological system. We can now see clear evidence that Weldon, on the contrary, enters statistical practice out of a much more broadly empiricist concern for the preservation of variation in biological systems. For this is precisely what Weldon means when he speaks of "describing our whole experience"—statistics is the enterprise that lets us preserve and study the full range of biological phenomena. It is a grave methodological error to attempt to simplify away—to attempt to *economize*—this data, even when we might think we have good reasons for ignoring certain correlations or variability. Furthermore, why would we need to do so when we have the tools of biometry available to us, which allow us to study biological systems in the full array of their natural variety?

## 4. Conclusions

With a novel conception of Weldon's philosophy of science in hand, we can turn to reevaluating the relationship between Pearson and Weldon. While their overall methodologies were all but identical, as were their ideas of valuable data and good experimental process, this agreement masks the fact that their motivations for engaging in statistics were interestingly divergent. Pearson, I have argued, views statistics as a tool for positivist simplification, while Weldon sees it as essential for the preservation of variation.

On such a view, the tension between Pearson and Weldon concerning the notion of causation (as reflected in the *Nature* debate) becomes manifest—and fully explicable. Pearson has adopted a positivist view of causation—we want to examine biological systems until we can reduce their behavior into a series of simple mathematical laws. When we know these laws, we will have the only thing which might pass for "causal" knowledge in biology. Weldon, on the other hand, has adopted what we might call a statistical view of causation—the only way in which we may accurately claim "causal" knowledge of a system, without destructive simplification, is to point to correlations within the system as a whole. Such correlations, on Pearson's philosophy, would constitute a very weak sort of causation—they would be clearly *necessary* for a causal link between two features, but far from *sufficient*. That is, a correlation is a form of functional relationship of the variety Pearson recognized, but an unacceptably *weak* one. The sharpening of these simple correlations into true *laws* must be one of the projects of a positivist biometry. It is obvious, then, that Pearson would decry Weldon's view as "a definition of cause . . . which does not appear of much utility to science" (Pearson, 1896a, pp. 460–461).

We might reconstruct the traditional view of the biometrical school as follows. Pearson and Weldon both engage in the biometrical project motivated by the need to preserve the wide ranging variation found in biological systems, and grounded in a philosophy of science that is broadly positivist. There is, on this view, no real philosophical or motivational difference between the two scientists.

This view must be substantially revised. Pearson, indeed, does seem to engage in the biometrical program on broadly positivist philosophical grounds. But his motivation for working in statistics does not focus on diversity, but rather simplification and economization of thought, a central positivist tenet, best interpreted in light of his *Grammar of Science* and other positivist writings. Weldon, on the other hand, holds a profoundly different motivation, a more traditionally empiricist reliance on the diversity of biological phenomena.

At a minimum, this much is interesting to the study of the history of biology. Weldon is a profoundly intriguing character in the life sciences around the turn of the twentieth century, though his work and thought are vastly under-studied. His rich form of empiricism impelled him to approach the great diversity of biological observation as a necessary and even beautiful feature of life.[21] Far from it being our duty to take this diversity and simplify it by using statistical tools, Weldon claimed that "it is the first business of a scientific man to describe some portion of human experience as exactly as possible. It does not matter in the least what kind of experience he chooses to collect; his first business is to describe it" (Weldon, 1906, p. 81).

We can, however, say more. Pearson and Weldon, on this reading, came to the biometrical school with interestingly different motivations. In spite of this, though, they converged on precisely the same methodological tools, and became the closest of collaborators. This case study thus makes trouble for our intuitions regarding the connection between statistical motivations and methods. It is not necessarily the case that the presence of identical methodologies is an indicator of identical motivations.

The general thesis that motivations are necessarily tied to methods is, therefore, false. The biometrical case study, however, shows us precisely why the question of the relationship between methods and motivations is interesting in the first place. For Pearson and Weldon, teasing out their reasons for taking up the tools of statistics reveals those reasons to be carefully nuanced and philosophically sophisticated. The history of statistics, particularly in the formative years of the late nineteenth century, proves to be an impressive locus for historical actors engaging in relatively overt theoretical and philosophical writing (certainly more overt than was usual for a naturalist like Weldon). As these figures attempted to decide whether or not to apply statistical reasoning to their areas of research, and then went on to justify to themselves and others the roles that these new tools were to play, they found an entirely novel and challenging set of problems yet to be solved. More research remains to be done on cases such as these, but the biometrical school offers us a profoundly interesting example.

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[21] Radick comes to effectively the same conclusion regarding Weldon in his (2005) and (2011).

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## References


Alexander, P. (1964). The philosophy of science, 1850–1910. In D. J. O'Connor (Ed.), *A critical history of Western philosophy* (pp. 402–425). London: The Free Press of Glencoe.

Ankeny, R. A. (2000). Marvelling at the marvel: The supposed conversion of A. D. Darbishire to Mendelism. *Journal of the History of Biology, 33*, 315–347.

Bateson, W. (1901). Heredity, differentiation, and other conceptions of biology: A consideration of Professor Karl Pearson's paper "On the principle of homotyposis". *Proceedings of the Royal Society of London, 69*, 193–205.

Bowler, P. J. (1992). *The eclipse of Darwinism: Anti-Darwinian evolution theories in the decades around 1900*. Baltimore, MA: JHU Press.

Cock, A. G. (1973). William Bateson, Mendelism and biometry. *Journal of the History of Biology, 6*(1), 1–36.

Cock, A. G., & Forsdyke, D. R. (2008). *Treasure your exceptions: the science and life of William Bateson*. Springer.

Cunningham, J. T. (1895). The statistical investigation of evolution [letter of Mar. 28, 1895]. *Nature, 51*(1326), 510.

Darden, L. (1977). William Bateson and the promise of Mendelism. *Journal of the History of Biology, 10*(1), 87–106.

Druery, C. T., & Bateson, W. (1901). Experiments in plant hybridization. *Journal of the Royal Horticultural Society, 26*, 1–32. (Translation of Mendel, J. G., Versuche über Pflanzenhybriden. *Verhandlungen des naturforschenden Vereines in Brünn*, 1865).

Fisher, R. A. (1918). The correlation between relatives on the supposition of Mendelian inheritance. *Philosophical Transactions of the Royal Society of Edinburgh, 52*, 399–433.

Fisher, R. A. (1922). Darwinian evolution by mutations. *Eugenics Review, 14*, 31–34.

Froggatt, P., & Nevin, N. C. (1971). The "Law of Ancestral Heredity" and the Mendelian-Ancestrian controversy in England, 1889–1906. *Journal of Medical Genetics, 8*(1), 1–36.

Galton, F. (1879). Composite portraits, made by combining those of many different persons into a single resultant figure. *Journal of the Anthropological Institute of Great Britain and Ireland, 8*, 132–144.

Galton, F. (1886). Regression towards mediocrity in hereditary stature. *Journal of the Anthropological Institute, 15*, 246–263.

Galton, F. (1895). Questions bearing on specific stability. *Transactions of the Entomological Society of London, 43*(1), 155–157.

Gayon, J. (2007). Karl Pearson: Les enjeux du phénoménalisme dans les sciences biologiques. In J. Gayon & R. Burian (Eds.), *Conceptions de la science, hier, aujourd'hui, demain* (pp. 305–324). Brussels: Ousia.

Hacking, I. (1990). *The taming of chance*. Cambridge: Cambridge Univ. Press.

Hilts, V. L. (1973). Statistics and social science. In R. N. Giere & R. S. Westfall (Eds.), *Foundations of scientific method: the nineteenth century* (pp. 206–233). Bloomington, IN: Indiana Univ. Press.

Huxley, J. (1942). *Evolution: The modern synthesis*. London: Allen and Unwin.

Igo, S. E. (2007). *The averaged American: Surveys, citizens, and the making of a mass public*. Cambridge, MA: Harvard Univ. Press.

Kevles, D. J. (1985). *In the name of eugenics: Genetics and the uses of human heredity*. New York: Alfred A. Knopf.

Kim, K.-M. (1994). *Explaining scientific consensus: The case of Mendelian genetics*. New York and London: The Guilford Press.

Lankester, E. R. (1896a). Are specific characters useful? [letter of Jul. 16, 1896]. *Nature, 54*(1394), 245–246.

Lankester, E. R. (1896b). The utility of specific characters [letter of Aug. 20, 1896]. *Nature, 54*(1399), 365–366.

Laudan, L. (1984). *Science and values: the aims of science and their role in scientific debate*. Berkeley: Univ. of California Press.

Lester, J., & Bowler, P. (Eds.). (1995). *E. Ray Lankester and the making of modern British biology*. Oxford: British Society for the History of Science.

MacKenzie, D. A. (1978). Statistical theory and social interests: A case-study. *Social Studies of Science, 8*(1), 35–83.

MacKenzie, D. A. (1979). Eugenics and the rise of mathematical statistics in Britain. In J. Irvine, I. Miles, & J. Evans (Eds.), *Demystifying social statistics* (pp. 39–50). London: Pluto Press.

MacKenzie, D. A. (1981). *Statistics in Britain, 1865–1930: The social construction of scientific knowledge*. Edinburgh: Edinburgh Univ. Press.

Magnello, M. E. (1998). Karl Pearson's mathematization of inheritance: From ancestral heredity to Mendelian genetics (1895–1909). *Annals of Science, 55*(1), 35–94.

Magnello, M. E. (1999a). The non-correlation of biometrics and eugenics: Rival forms of laboratory work in Karl Pearson's career at University College London, part 1. *History of Science, 37*, 79–106.

Magnello, M. E. (1999b). The non-correlation of biometrics and eugenics: Rival forms of laboratory work in Karl Pearson's career at University College London, part 2. *History of Science, 37*, 123–150.

Magnello, M. E. (2004). The reception of Mendelism by the biometricians and early Mendelians (1899–1909). In M. Keynes, A. W. F. Edwards, & R. Peel (Eds.), *A century of Mendelism in human genetics* (pp. 19–32). Boca Raton, FL: CRC Press.

Morrison, M. (2002). Modelling populations: Pearson and Fisher on Mendelism and biometry. *British Journal for the Philosophy of Science, 53*, 39–68.

Mudge, G. P. (1935). J. T. Cunningham, M.A. *British Medical Journal, 2* (3887), 42.

Norton, B. J. (1978). Karl Pearson and statistics: The social origins of scientific innovation. *History of Science, 8*(1), 3–34.

Olby, R. (1987). William Bateson's introduction of Mendelism to England: A reassessment. *British Journal for the History of Science, 20*(4), 399–420.

Olby, R. (1989). The dimensions of scientific controversy: The biometric-Mendelian debate. *British Journal for the History of Science, 22*(3), 299–320.

Pearson, E. S. (1936). Karl Pearson: an appreciation of some aspects of his life and work: Part I: 1857–1906. *Biometrika, 28*(3/4), 193–257.

Pearson, K. (1892). *The grammar of science* (1st ed.). London: Walter Scott.

Pearson, K. (1894a). Contributions to the mathematical theory of evolution. *Philosophical Transactions of the Royal Society of London, A, 185*, 71–110.

Pearson, K. (1894b). Dilettantism and statistics: *The growth of St. Louis children* [review]. *Nature, 51*(1311), 145–146.

Pearson, K. (1895). Contributions to the mathematical theory of evolution: II. Skew variation in homogeneous material. *Philosophical Transactions of the Royal Society of London, A, 186*, 343–414.

Pearson, K. (1896a). The utility of specific characters [letter of Sep. 17, 1896]. *Nature, 54*(1403), 460–461.

Pearson, K. (1896b). Mathematical contributions to the theory of evolution: III. Regression, heredity, and panmixia. *Philosophical Transactions of the Royal Society of London, A, 187*, 253–318.

Pearson, K. (1898). Mathematical contributions to the theory of evolution: On the Law of Ancestral Heredity. *Proceedings of the Royal Society of London, 62*, 386–412.

Pearson, K. (1900). *The grammar of science* (2nd ed.). London: Adam and Charles Black.

Pearson, K. (1904). Mathematical contributions to the theory of evolution: XII. On a generalized theory of inheritance, with special reference to Mendel's laws. *Philosophical Transactions of the Royal Society of London, A, 203*, 53–86.

Pearson, K. (1906). Walter Frank Raphael Weldon. 1860–1906. *Biometrika, 5*(1/2), 1–52.

Pearson, K. (1908). On a mathematical theory of determinantal inheritance, from suggestions and notes of the late W.F.R. Weldon. *Biometrika, 6*(1), 80–93.

Pearson, K. (1914). *The life, letters, and labours of Francis Galton, vol. I: Birth 1822 to marriage 1853*. Cambridge: Cambridge Univ. Press.

Pearson, K. (1924). *The life, letters, and labours of Francis Galton, vol. II: Researches of middle life*. Cambridge: Cambridge Univ. Press.

Pearson, K. (1930). *The life, letters, and labours of Francis Galton, vol. IIIA: Correlation, personal identification, and eugenics*. Cambridge: Cambridge Univ. Press.

Pearson, K., Lee, A., & Bramley-Moore, L. (1899). Mathematical contributions to the theory of evolution: VI. Genetic (reproductive) selection: Inheritance of fertility in man, and of fecundity in thoroughbred racehorses. *Philosophical Transactions of the Royal Society of London, A, 192*, 257–300.

Plutynski, A. (2006). What was Fisher's fundamental theorem of natural selection and what was it for? *Studies in History and Philosophy of Biological and Biomedical Sciences, 37*, 59–82.

Porter, T. M. (1986). *The rise of statistical thinking, 1820–1900*. Princeton, NJ: Princeton Univ. Press.

Porter, T. M. (2004). *Karl Pearson: The scientific life in a statistical age*. Princeton, NJ: Princeton Univ. Press.

Porter, T. M. (2005). The biometric sense of heredity: Statistics, pangenesis, and positivism. In S. Müller-Wille & H.-J. Rheinberger (Eds.), *A cultural history of heredity III: 19th and early 20th centuries* (pp. 31–42). Berlin: Max Planck Institute for the History of Science.

Provine, W. B. (1971). *The origins of theoretical population genetics*. Chicago and London: Univ. of Chicago Press.

Radick, G. (2005). Other histories, other biologies. *Royal Institute of Philosophy Supplement, 80*(3–4), 21–47.

Radick, G. (2011). Physics in the Galtonian sciences of heredity. *Studies in History and Philosophy of Biological and Biomedical Sciences, 42*(2), 129–138.

Roll-Hansen, N. (1980). The controversy between biometricians and Mendelians: A test-case for the sociology of scientific knowledge. *Social Science Information, 19*(3), 501–517.

Roll-Hansen, N. (2000). Theory and practice: The impact of Mendelism on agriculture. *Sciences de la Vie, 323*, 1107–1116.

Roll-Hansen, N. (2005). Sources of Johannsen's genotype theory. In S. Müller-Wille & H.-J. Rheinberger (Eds.), *A cultural history of heredity III: 19th and early 20th centuries* (pp. 43–52). Berlin: Max Planck Institute for the History of Science.

Sloan, P. R. (2000). Mach's phenomenalism and the British reception of Mendelism. *Sciences de la Vie, 23*, 1069–1079.

Sober, E. (1980). Evolution, population thinking, and essentialism. *Philosophy of Science, 47*(3), 350–383.

Stigler, S. M. (1990). *The history of statistics: The measurement of uncertainty before 1900*. Cambridge, MA: Harvard Univ. Press.

Tabery, J. G. (2004). The "evolutionary synthesis" of George Udny Yule. *Journal of the History of Biology, 37*, 73–101.

Thiele, J. (1969). Karl Pearson, Ernst Mach, John B. Stallo: Briefe aus den Jahren 1897 bis 1904. *Isis, 60*(4), 535–542.

Thiselton-Dyer, W. T. (1895). Variation and specific stability [letter of Mar. 14, 1895]. *Nature, 51*(1324), 459–461.

Weldon, W. F. R. (1890). The variations occurring in certain decapod crustacea. I. *Cragnon vulgaris*. *Proceedings of the Royal Society of London, 47*, 445–453.

Weldon, W. F. R. (1893). On certain correlated variations in *Carcinus maenas*. *Proceedings of the Royal Society of London, 54*, 318–329.

Weldon, W. F. R. (1895a). An attempt to measure the death-rate due to the selective destruction of *Carcinus maenas* with respect to a particular dimension. *Proceedings of the Royal Society of London, 57*, 360–379.

Weldon, W. F. R. (1895b). Variation in animals and plants. *Nature, 51*(1323), 449–450.

Weldon, W. F. R. (1896a). The utility of specific characters [letter of Jul. 30, 1896]. *Nature, 54*(1396), 294–295.

Weldon, W. F. R. (1896b). Utility of specific characters [letter of Sep. 3, 1896]. *Nature, 54*(1401), 413.

Weldon, W. F. R. (1898). Address of the president of section D [Zoology]. *Report of the British Association for the Advancement of Science, 68*, 887–902.

Weldon, W. F. R. (1902). Mendel's laws of alternative inheritance in peas. *Biometrika, 1*(2), 228–254.

Weldon, W. F. R. (1906). Inheritance in animals and plants. In T. B. Strong (Ed.), *Lectures on the method of science* (pp. 81–109). Oxford: Clarendon Press.

[Weldon, W. F. R., Pearson K., Davenport, C.B.] (1901). Editorial: The scope of Biometrika. *Biometrika*, 1(1), 1-2.