How Not to Fight About Theory: The Debate Between Biometry and Mendelism in *Nature,* 1890–1915

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Abstract

Evolutionary theory in the late nineteenth century was marked by controversy over the way in which Darwin's insights could be reconciled with theories of heredity, particularly following the rediscovery of the work of Mendel. Darwin's proposal of common descent was quickly accepted, but natural selection's adoption was delayed as a result of this debate. Two schools of thought rapidly developed: one (the "biometricians") using statistics to describe patterns of inheritance, and the other (the "Batesonians," later the "Mendelians") using morphological study and searching for discontinuous variation. The traditional history of this debate has focused on its key players (most notably William Bateson, Karl Pearson, and W.F.R. Weldon). Sociologists of science have helpfully expanded this picture to include a wider array of biologists and emphasize the role of the "conversion" of lesser-known biologists from biometry to Mendelism.

In this chapter, I will expand our picture further, by considering the structure of the debate as it developed in the articles and correspondence of *Nature.* The "network of discourse" in this literature, as revealed by digital tools, shows us surprising features of the way this argument unfolded. Far from being a central topic in the biology literature of the day, as historians often have assumed, those arguing over theories of heredity recede into the background, engaging with fewer other members of their fields. While this digital analysis is not the end of the story, it offers us a variety of suggestive questions for future historical, philosophical, and sociological work on this crucial period in the development of evolution.

Keywords: Thomas Kuhn, theoretical crisis, theory change, genetics, Mendelism, biometry, W. F. R. Weldon, Karl Pearson, William Bateson

Thomas Kuhn's massively influential *Structure of Scientific Revolutions* (1996) proposed a pattern for the development of a scientific theory now intimately familiar to philosophers and historians of science. After the initial struggles to formulate a unified theory, a *paradigm* emerges in an area of study for the first time. This paradigm consists not just of theoretical content, but also metaphysical and epistemological commitments, textbooks and patterns of training, and a set of problems that, it is expected, the paradigm will be able to solve, along with the kinds of solutions for those problems that would be deemed acceptable. The solving of those problems constitutes the everyday work of most scientists, Kuhn's *normal science.* Should one of those problems persistently resist solution (or dissolution), a field might enter a state of *crisis,* where active replacements for the dominant paradigm are investigated and their future prospects evaluated. If a new paradigm is found to outperform the ruling orthodoxy, we have a *revolution,* and the new world view becomes dominant in its place.[1]

This is the standard picture that we present to our undergraduates. Despite its nearly trite status, however, it still provides us with an interesting point of departure for analysis of any instance of theory change. For one, Kuhn leaves the period of crisis tantalizingly under-developed. Why do crises really emerge? How do they disappear? What are the varying contributions of social factors like persuasion, empirical results provided by "nature," and novel theoretical developments? What of the bogeyman of Kuhn's "incommensurability," the idea that scientists operating in different paradigms in some sense "practice their trades in different worlds" (Kuhn 1996, 150), necessarily talking past one another's internalized ways of not just interpreting, but of *seeing* scientific data?

Here, it seems, is a place where the intervention of digital methods might genuinely be

1 All of this sounds a bit evolutionary, of course, a fact which was not lost on either Kuhn himself (who draws the analogy at the end of *Structure*), nor on David Hull, who significantly expanded on the analogy between scientific theory change and evolution in his *Science as a Process* (1988).

able to shed light on the subject. For crises do not only take place in the heads of scientists, in their notebooks, or the experiments performed in their laboratories – they are also played out in the literature that these scientists produce. Especially since the middle of the nineteenth century (with the proliferation of frequently published scientific magazines like *Science, Nature,* or *The American Naturalist*), the journal literature is perhaps even the primary venue in which such debates have taken place.

We should thus be able to explore the signature of crises as they happen in these publications, and precisely such an exploration is my aim in this chapter. I will examine a particularly divisive and public debate in the late-nineteenth century study of inheritance, a significant portion of which took place in the correspondence pages of *Nature,* and endeavor to draw some speculative conclusions for the structure of scientific communities during theoretical crises. By analyzing the network of author-mentions in *Nature* in this time period, we reveal new and interesting relationships between the relevant players – different from both the basic networks of paradigm membership and the more complex networks of training and education – and one that supports (albeit tentatively) some conclusions about how community members react to sustained theoretical disagreement.

Selecting a Case Study

Before I lay out the history of the episode that I'll be analyzing, however, I should offer a few words about why I selected it. Some of my reasons were purely selfish – I have already written on this debate rather extensively, and so am personally quite familiar with it (Pence 2011, 2015). But it also has a number of significant, independent advantages. First of all, we have a standard, "potted" history of the crisis – limited and problematic though it may be (about which more in the next section) – offered by William Provine's often-cited book on the history of population genetics (1971), as well as a detailed sociological account, thanks to Kyung-Man Kim (1994). Second, the debate takes place over a fairly delimited time period (roughly 1890–1910), and a significant part of it occurs in a single journal, *Nature.*[2] Finally, that time period lies fully in the public domain, somewhat ameliorating copyright concerns. Each of these features makes a digital-humanities analysis of this case significantly easier, more likely to yield fruitful results, and better contextualized.

The Biometry-Mendelism Debate

The publication of Charles Darwin's *On the Origin of Species* (1859) did not precipitate the kind of immediate, wholesale conversion to Darwin's world-view which that work's lofty stature in the contemporary scientific canon might suggest. The *Origin* was, to be sure, amazingly successful, and quickly convinced the vast majority of working naturalists of the truth of one of Darwin's two main points – *common descent,* the claim that all life is descended, in a tree-like structure, from some small number of distant ancestors. But this was only part of Darwin's argument (for more on the development of Darwin's views, see Murdock et al., chapter 3, this volume). He also offered

2 Access to the *Nature* corpus is provided courtesy of Nature Publishing Group to the evoText Project (Ramsey and Pence 2016), with thanks.

a mechanism for diversification and the eventual production of novel species – *natural selection* – and it was much less accepted in the short term. In a period often, though problematically, called the "eclipse of Darwinism" (Huxley 1942; Bowler 1992; but see Largent 2009), the fate of selection waxed and (mostly) waned, as it confronted a variety of interesting criticisms, until the development of early population genetics in the 1920s and 1930s kicked off the Modern Synthesis that still grounds much of evolutionary biology today.[3]

One of the most significant issues that plagued natural selection was this: it seemed to many impossible to believe that selection was powerful enough to produce new species, in the absence of an understanding of the mechanism of variation. Only when we know how variations are generated within organisms and then passed from parents on to their offspring can we go on to learn how selection could bias that transmission to cause speciation. And this approach, in turn, only works on the assumption of Darwin's hypothesis that speciation would be the result of gradual, piecemeal changes from one generation to another – some scientists, such as William Bateson, remained convinced that these small, gradual variations would *never* suffice for producing species-level differences.

Blending inheritance provides one example of the kind of difficulties that could, at least potentially, arise within the process of heredity. If all characters of offspring are merely blends of the characters of their parents, for any favorable variation to "stick" in a population requires that some mechanism prevent it from being blended back into, and thus swamped by, the ancestral

3 This standard story is, of course, a simplification of the real facts of the case – in particular, there is more continuity between the biology that immediately follows Darwin (and will be discussed here) and the Modern Synthesis than is accounted for by this view. My full argument for this claim awaits a forthcoming book, though the discussion here anticipates those claims in a number of ways.

character. Darwin was, to be sure, aware of the trouble for his theory raised by blending inheritance (Vorzimmer 1963).[4] As a result, Darwin appeals to geographic isolation, the power of the struggle for existence (Vorzimmer 1963; Gould 1985), machinery from his (long-held) theory of pangenesis (Hodge 1985), and he also increases the bulk amount of variation available to natural selection (Depew and Weber 1995, 196). But the general problem festered.

It is also important to note that this plays into a preoccupation across the biological sciences of this period with a number of different "problems of variation." Beyond the questions of blending and mere quantity of variation, this era also saw the debates over August Weismann's theory of the germ plasm, the role of chromosomes as possible bearers of variations, Herbert Spencer's support for the necessity of the directedness of organic variation to the explanation of complex adaptations, and debate over the role of the environment in generating variation, among others. Worries about variation were therefore "in the air" in the period, and intimately tied to the nature of selection and whether it sufficed to explain the features of the natural world (see Pearce 2014, 18–20; Beatty 2016 for excellent surveys of the broader landscape).

One of the biologists most keen on solving the twin problems of variation and selection was the aforementioned William Bateson, who as early as the 1890s was already, as Radick has put it, "well on his way to a developing a vigorously dissenting saltationist perspective on evolution" (Radick 2012, 718), aiming to solve the question by finding evidence of large, discontinuous mutations responsible for speciation. Bateson published his *Materials for the Study of Variation* in

4 Vorzimmer (1963) also convincingly argues that this is not merely due to Darwin's reading of Fleeming Jenkin's review of the *Origin,* as is often supposed. For more on the details of the substance of Jenkin's argument and its relationship to the critical environment around the *Origin,* see Bulmer (2004).

1894 (the subtitle of which, not often quoted, is *Treated with Especial Regard to Discontinuity in the Origin of Species*), calling on those interested in Darwinism to return to the descriptive work of a morphological understanding of variation itself:

> As the first step towards the systematic study of Variation we need a compact catalogue of the known facts, a list which shall contain as far as possible all cases of Variation observed. To carry out such a project in any completeness may be impossible; but were the plan to find favour, there is I think no reason why in time a considerable approach to completeness should not be made. (Bateson 1894, vi)

Meanwhile, as Bateson rallied a group of researchers to his cause, the biologist W. F. R. Weldon began pursuing statistical research into natural selection under the tutelage of Francis Galton (Weldon 1890). This work (and a fortuitous move from Cambridge to University College London) brought him into contact with the statistician Karl Pearson, who had long been interested in biological problems, both for their own sake (as an aid to eugenics), and as a potentially fruitful field of application for statistical methods (Pearson 1892). These two men would form a fast friendship and fruitful partnership that would last some twenty-five years, until Weldon's untimely death from pneumonia at the age of 46 (Pearson 1906). In the interim, they would form the antithesis to Bateson's research program. Known as the "biometricians" or the "biometrical school," they were staunch Darwinian gradualists, committed to the use of statistical methods to demonstrate the ability of natural selection to generate new species from small, individual variations.

Relations between the sides began to sour after Weldon (who had been Bateson's mentor at Cambridge) published a harshly negative review of Bateson's *Materials* (1894). As the debate became progressively more heated, the work of Gregor Mendel (1866) was "rediscovered" in 1900 (published as Druery and Bateson 1901). While the biometricians initially believed this might provide an interesting statistical case study for their cause, Bateson and his allies (soon to be known as the "Mendelians") saw in Mendel's peas precisely the sort of discontinuous variation for which they had long been searching. For a variety of reasons – commonly cited are, at least, an increase in the amount of Mendelian data available (Cock and Forsdyke 2008), a well-run popular campaign of lectures by Bateson (Radick 2012), and the death of Weldon (see Vicedo 1995 for a helpful review) – the Mendelians had carried the day as of around 1906 (Sloan 2000). The combination of statistics with genetics would await the work of biologists like Fisher, Haldane, and Wright, during the development of the Modern Synthesis (Provine 1971).

So much for the standard historical tale we tell about this period. As can be seen, it focuses on key players (Bateson, Weldon, Pearson, Mendel), and it also attempts to explain the start of a crisis across an entire sub-discipline in terms of a small number of "key events" without much detail as to how those events precisely influenced the actors involved (e.g., it ignores the kinds of fine-grained details studied in Darbishire's case by Ankeny 2000; or for Yule by Tabery 2004). Finally, while the fact of the debate's conclusion by 1910 is acknowledged by all involved, there is a bewildering lack of clarity about why it might have ended. It is clear that more detail would be enlightening. To pursue it, I turn now to resources from the sociology of science.

The Sociological Picture

To offer a first expansion of our lens, we may turn to the work of Kyung-Man Kim, whose book *Explaining Scientific Consensus: The Case of Mendelian Genetics* (1994) extends the network of players to include a variety of what he calls "paradigm articulators." These are a number of writers who "articulated the still inchoate paradigms" of biometry and Mendelism "by extending and elaborating the theory," but who did *not,* in general, "evaluate their mentor's theory" (Kim 1994, 35). Importantly, five of the central paradigm articulators, Kim argues, "converted" from biometry to Mendelism between 1903 and 1910. These biologists – A. D. Darbishire, Edgar Schuster, George Udny Yule, Raymond Pearl, and George Shull – were instrumental in producing the consensus around Mendelism, as they brought a significant shift in resources and interest (momentum, one might say) toward the Mendelian side.

I lack the space here to evaluate Kim's claim about the influence of paradigm articulators on its own merits.[5] But what makes Kim so interesting for my purposes is the expansion in scope: rather than a narrow view only of Pearson, Bateson, and occasionally Weldon, Kim turns our attention to structures of education, training, and theory transmission as they actually appear on the ground (the full network of actors considered by Kim in detail is found in figure 1). It is precisely this same impulse toward casting a wider, more comprehensive net that underlies my turn toward digital methods. To really understand the nature of a scientific community during a period of crisis, it clearly won't suffice to consider only the "elite" players.

5 For skeptical takes on Kim's major thesis, see Barnes (1996) and Vicedo (1995). Vicedo also helpfully questions the general utility of the categories "Mendelian" and "biometrician," an important task which I also am forced to avoid here. Ankeny also (2000) disputes the usefulness of the "conversion" metaphor, at least for the case of Darbishire, another important issue with Kim's view, and the reason that I persistently place "conversion" in scare-quotes.

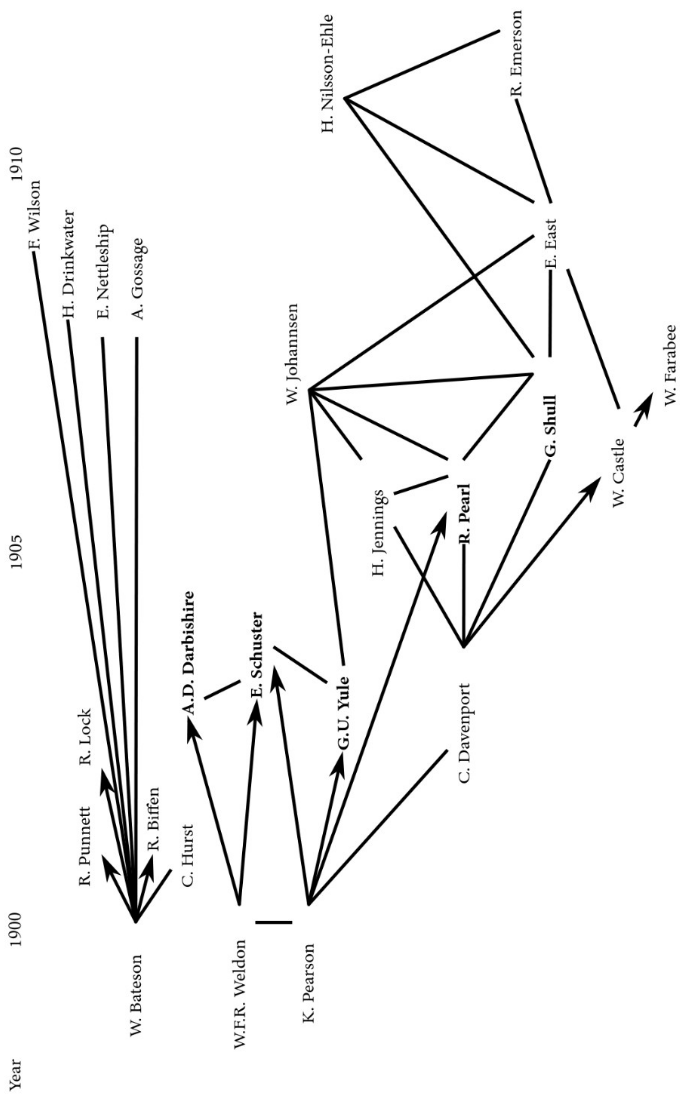

Figure 1: The network of training and influence in the biometry-Mendelism debate, between 1900 and 1910. Redrawn after figure 2 of Kim (1994). Arrows indicate mentorship, lines indicate collaboration. The five paradigm articulators whose "conversion" Kim singles out are in boldface.

Building a Digital Analysis

If, as the traditional history has it, the biometry-Mendelism debate is a crisis in the nascent field of genetics, and, as Kim's sociological story has it, it involves a wide array of players from across the field, then we should be able to detect its signature in the journal literature of the period. As I mentioned above, a number of important contributions to the discussion occur in the pages of *Nature.*[6] The first step in analyzing the crisis digitally, then, was to build a corpus of relevant articles, using evoText (Ramsey and Pence 2016), an analysis platform which includes the entire print run of *Nature*. I began by compiling a list of articles authored by central individuals in Kim's network (figure 1) – these included Bateson, Pearson, Weldon, the five "converted" paradigm articulators, and Wilhelm Johannsen (who is also central to Kim's analysis).[7] This resulted in a "seed set" of 144 articles.

To expand this set, I used the Named Entity Recognizer provided by the Stanford NLP project (Manning et al. 2014) to construct a list of every proper name referenced in any of the articles within the seed set. This roster was manually culled to include only biologists (taken broadly to include authors of natural histories, explorers, etc.). This increased the number of biologists to 98, of whom 52 had published in *Nature.*[8] These authors, in turn, published 1,622

6 I have analyzed a portion of this content in detail elsewhere (Pence 2011).
7 Schuster, Pearl, and Shull contributed no articles to *Nature.* All of the data discussed here, along with a detailed discussion of the methodology used, is available under the CC-BY license at <https://github.com/cpence/biometry-mendelism>. An animated network visualization of the data discussed below is also available.
8 One iteration of this "snowball sampling" proves to be the sweet spot – it provides a large enough corpus for analysis, while running a second iteration of snowball sampling would require the manual inspection of some 16,427 proper names which appear in the current dataset of 1,622 articles, a clearly impractical undertaking. The Stanford NER is too inaccurate to use its output without at least some sort of manual filtering. Edward Murray East and Richard South also had to be removed from analysis, as searching for instances of their last names (for obvious reasons) returns much more noise than signal.

articles in *Nature,* between 1872 and 1940. This constituted the dataset which is analyzed in the following.[9]

Citations, Mentions, and the Network of Discourse

If the goal is to offer a new lens into community structure, then a classic methodology would be to utilize citation networks, an approach now more than fifty years old (de Solla Price 1965). Unfortunately, this methodology is not applicable to the articles in the biometry-Mendelism dataset. Citation practices had yet to be standardized in *Nature* in the late nineteenth century. To take just one example, a letter from J. T. Cunningham to *Nature* on July 30, 1896 makes a reference to a prior letter from July 16 as follows: "It appears to me that Prof. Weldon's argument, referred to in *Nature* of July 16 (p. 245), is accurately represented in the following illustration" (Cunningham 1896). What's more, the letter to which Cunningham is referring is not even authored by Weldon himself – it is a recounting of one of Weldon's arguments by E. Ray Lankester (1896). It is clear that no automated system would be able to extract the actual citations as they occur, and in many cases it isn't clear that the modern notion of "citation" is even applicable.

We thus need some way to determine how authors are related to one another, without invoking citations. The simplest such method, as it turns out, works perfectly well for our purposes. Rather than looking for formal citation, we simply search each of the articles in the dataset, and determine when each mentions the name of one of the other authors in our set. For

9 I performed this "seed set with snowball sampling" method for two reasons: (1) to focus, to the extent that I could, on biological discussions in *Nature*, as opposed to those from other disciplines, and (2) to minimize confusion regarding common last names, so that, when a last-name is mentioned, it is likely to refer to the relevant biologist.

example, if an article by Pearson mentions Punnett (or vice-versa), then we add a connection between Pearson and Punnett (or increase the weight of that connection if it already exists). This produces a network which I have dubbed the *network of discourse.* While less precise than a citation network, as it turns out, the network of discourse is still powerful enough to draw interesting conclusions.

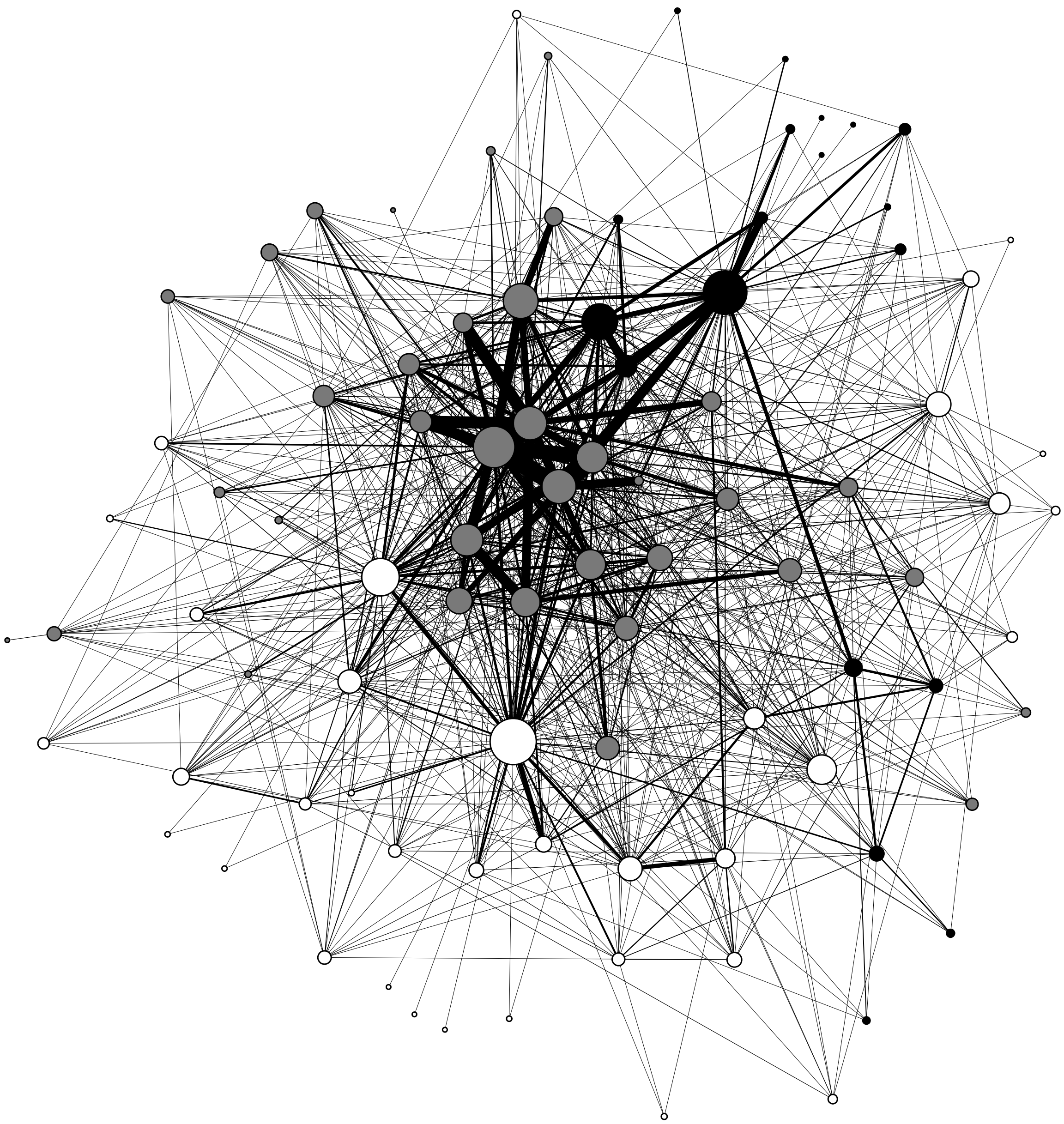

Figure 2: The network of discourse for the entire 1,622-article dataset. Colors of nodes indicate membership in one of three modularity classes, size of nodes indicates number of occurrences in the corpus, and thickness of edges indicates number of connections between nodes. Bateson is the largest solid-black node, upper-right. Weldon is the small black node to its lower left. Pearson is the largest white node, bottom-center. Network visualizations throughout created using Gephi (Bastian, Heymann, and Jacomy 2009).

The network of discourse for the entire debate may be found in figure 2. In general, this network is too dense and too interconnected to offer us any significant information. One feature, however, stands out. A number of methods are available to differentiate clustering in the graph, most common among them the *modularity* statistic. Modularity measures the distinctness of communities within a network by determining whether the number of edges that fall into a hypothesized set of groups is greater or less than that expected if edges were distributed according to chance. Algorithms for detecting modularity traditionally accept a parameter that lets their level of "grain" be selected by the user, and will then attempt to break the network up into clusters of the requested size (Blondel et al. 2008; for more on modularity detection in general, see Rivelli 2019). Robust modularity (i.e., a pattern of groups that remains roughly the same through a range of values of the fine- vs. coarse-grain parameter) indicates genuine community structure or clustering within the network – that is, it indicates that the nodes of the network really do separate into "sub-groups," and these groups are not simply random results of the algorithm. In the full network as shown in figure 2, we do indeed get robust clustering, into more or less three groups. Most interestingly, we see that the network here does not simply recapitulate the network of paradigm membership (clusters do not simply sort biometricians from Mendelians), nor the network of training as identified by Kim. In fact, Bateson and Weldon fall into the same cluster, and a different cluster from Pearson – the first signal of the controversy in the literature as found in the data.

The Network of Discourse Over Time

To amplify the signal, and to capture the ebb and flow of the debate over the time period between 1890 and 1910, I segmented the network by date, visualizing the networks consisting only of articles published between 1885 and 1889, 1890–94, 1895–99, 1900–04, 1905–09, and 1910–1914. These five-year windows were selected such that, in essence, the first window occurs entirely before the debate between biometricians and Mendelians breaks out (Bateson's *Materials,* recall, is not published until 1894, and Weldon's first statistical work dates from 1890) and the last window occurs entirely after the debate has cooled (Weldon's death is in 1906, and Kim's analysis has it that the last of the paradigm articulators to "convert," Raymond Pearl, does so around 1909).

Let's begin by looking at the topology of the network over these time slices, and then turn to what this case study might tell us more broadly. When we analyze this network in detail (in particular, attempting to detect communities within each time slice of the network), we find a variety of interesting features. Prior to 1884 and after 1910, we see no robust clustering – i.e., the clusters which the modularity algorithm provides change dramatically as the coarse- vs. fine-grain parameter is adjusted, indicating that the clusters produced by the algorithm are merely artifacts. The network has a very standard center-periphery structure, with the center occupied by the expected players – prolific writers from the correspondence pages such as E. Ray Lankester (Lester 1995), Thomas Henry Huxley, and George J. Romanes. As the controversy really begins to heat up, between 1895 and 1899, the impact on the network of discourse becomes more significant (figure 3). The most commonly appearing names, and the strongest links, are precisely those between the authors engaged in this debate. Right at the heart of the conversation we find

Weldon (1), Pearson (5), and Bateson (3), along with Lankester, Cunningham, and W. T. Thiselton-Dyer, all of whom had been exchanging salvos of letters and articles on the controversy. Only a few other edges in the network even come close to producing the volume of cross-mentions that these figures do. This signal of the debate itself thus dominates other discussions about and among these biologists in this period.

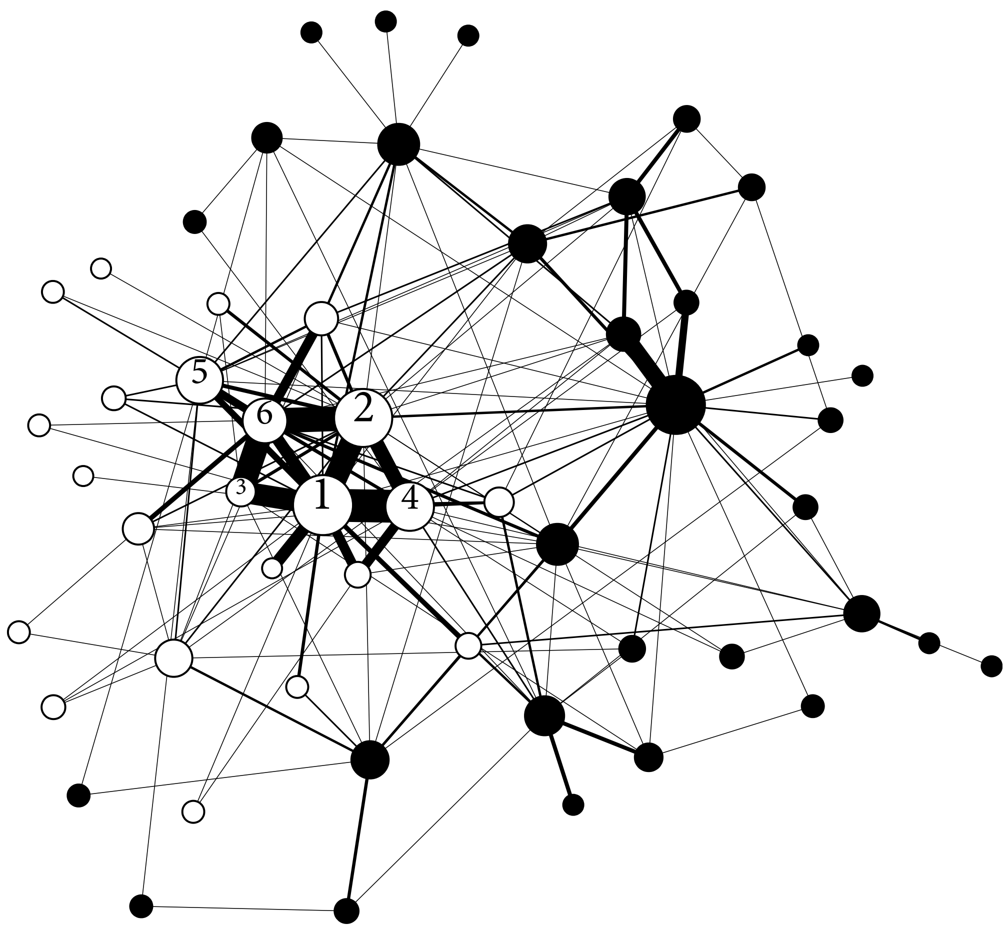

Figure 3: The network of discourse in *Nature,* from 1895 to 1899. The following nodes are labelled: W. F. R. Weldon: 1; E. Ray Lankester: 2; William Bateson: 3; J. T. Cunningham: 4; Karl Pearson: 5; W. T. Thiselton-Dyer: 6. Node colors indicate membership in one of two modularity classes, node sizes indicate number of appearances in the corpus, and thickness of edges indicates strength of connection between nodes.

When we move just five years forward, however, to the period between 1900 and 1904, the network shifts dramatically (figure 4). Pearson (5), Lankester (2), Cunningham (4), and Thiselton-Dyer (6) no longer even appear *in the same cluster* as Bateson (3) and Weldon (1). While the other authors here seem to have moved on to other concerns, Bateson and Weldon have, essentially, become consumed by this debate. Not only are they talking only to and about one another, few other members of the network mention their works at all during these five years. The network visualization itself is even reminiscent of Weldon and Bateson standing in a corner, arguing with one another, being primarily ignored by the rest of the biologists publishing in *Nature*.

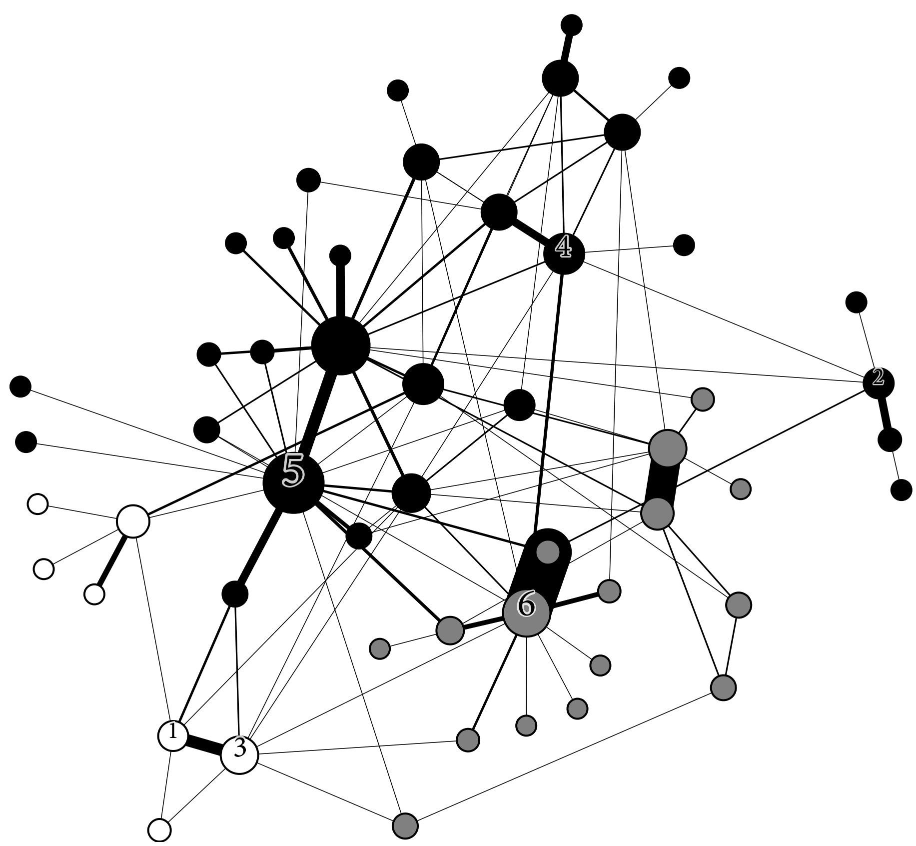

Figure 4: The network of discourse in *Nature,* from 1900 to 1904. Node numbers are the same as in figure 3. Node colors indicate membership in one of four membership classes, with the two smallest classes collapsed into gray. Node sizes indicate number of appearances in the corpus, and thickness of edges indicates strength of connection between nodes.

After 1904, we see a brief signal of the heavy collaboration between Pearson and Raymond Pearl between 1905 and 1909, and the network returns to the structure it had before the debate took off in the first place.

Conclusions

I want to offer conclusions at two levels here – first, for what this analysis can offer to scholars of the debate between biometry and Mendelism, and second, for what we may be able to infer about scientific communities and Kuhnian crises.

Biometry and Mendelism

First and foremost, we can see from these results that the method used here, focusing on networks of discourse rather than citation networks, does in fact offer us reasonable results in the case of the biometry-Mendelism debate in *Nature*. This is an important finding in and of itself, as it was entirely unclear prior to running the analysis whether mere mention would suffice for indicating significant connection between authors in the debate. As we have seen, however, the network of discourse does indeed separate clusters of those individuals contributing to the biometry-Mendelism debate from those not. I anticipate that this method will be useful in a variety of other contexts where citation data are unavailable or prohibitive to extract. Network analysis without citations is thus a promising area for future research.

Further, in this particular case, the network of discourse does not simply recapitulate features of the community of which we were already well aware. For example, it would be only of trivial interest if the network of discourse simply sorted the biometricians into one cluster and the Mendelians into another, or if it looked precisely like Kim's network of training and collaboration (indicating that collaborators, mentors, and mentees made references to one another far more often than to others). Rather, the network of discourse seems to give us a novel way of approaching the structure of this debate, one that both carries features of intrinsic interest and inspires future research questions.

For example, the mobility of Pearson in the network throughout this controversy is notable. The idea that Weldon and Pearson were importantly different figures with different philosophical backgrounds and biological commitments has recently gathered some steam (Radick 2005; Pence 2011), and the role of Pearson in Weldon's extended disagreement with Bateson would be a fruitful locus to explore this question further. Similarly, in the network between 1890 and 1894, a tight cluster of authors forms which includes Romanes, Lankester, Herbert Spencer, and August Weismann. When I initially generated the network, I was unable to account for this clustering, but later study points to a debate over Weismann's results occurring precisely during this time period.[10] While I lack the space to pursue it here, we see yet again the digital analysis serving as a way to generate interesting research questions. Had there not been a ready explanation for this second set of clusters at hand, the digital methods would themselves

10 Thanks to Trevor Pearce, in particular, for bringing this debate to my attention.

have generated a novel object for historical investigation.

Of course, this analysis is not without its problems. Most significantly, the work so far only analyzes *Nature,* which at this point is still more or less centered in the United Kingdom (for more on the history of the journal, see Baldwin 2015). This harms the analysis in a variety of ways. Perhaps the most significant is that an entire school around Charles B. Davenport, operating in the United States (and hence less likely to publish in *Nature*), is nearly invisible in this analysis. A significant number of the authors picked out in Kim's work are thus nowhere to be found here. Further, in the middle of this period – the first issue is published in 1901 (Weldon, Pearson, and Davenport 1901) – the biometricians launch their own journal, *Biometrika,* as a response to perceived difficulties publishing in the journals of the Royal Society, which the biometricians believe are being taken over by Bateson and his allies (Pearson 1906, 34–35). Integrating an analysis of *Biometrika* would present a variety of technical challenges. It is not immediately clear how the analysis here could be expanded to include multiple journals. For one, the fact that the rate of publication in *Nature* is relatively constant over this period means that no "normalization" was required to control for the number of papers published in each of the time slices. It is likely that the entire publication output of *Biometrika* would be much smaller, and thus some sort of weighting would be needed for the data from *Nature* not to completely overwhelm that from *Biometrika.* If, on the other hand, a separate network was created just for the *Biometrika* contributions, we run into the problem of how to compare and draw conclusions from the various networks that result. Again, this is a promising area for future work.

I should also take some time here to consider the place of these results in a broader picture of the history of the biometry-Mendelism debate. I have noted already that these results offer us an interestingly different perspective from either that of the standard history or Kim's sociological take. Such perspectives, then, are always at least somewhat intrinsically valuable. But does this view tell us anything original about the history of the case? And to the extent that it does, is it actually useful?

It would take much more space than I have here to make the case in full, but I believe that we do find a facet of the controversy that is genuinely novel in this analysis, and one that aligns with what we know to have been going on in the same period in the other work of figures like Weldon, Bateson, and Pearson. To perhaps over-extend a military metaphor, the biometry-Mendelism controversy was fought and won (or lost) on a number of fronts. These biologists were engaged in, at the very least, theoretical work within their own traditions aimed at expanding and solidifying their accounts of the biological world, debates within their own traditions about the most effective ways to move forward, debates between the two approaches of biometry and Mendelism, and various kinds of positioning with respect both to other fields of science and to the public.

The analysis of the correspondence pages of *Nature* gives us insight into the campaign on at least two of these many fronts. Short articles in *Nature* in this period, as Melinda Baldwin has detailed, served as a fast-moving, discursive outlet for an entire generation of young, British men of science (Baldwin 2015, 63–67). Weldon himself notes its peculiar place in correspondence with

Pearson over the future direction of their journal, *Biometrika*. He laments that, despite the fact that "people now want very technical journals, *plus* Rudyard Kipling and the evening papers," nonetheless "*Nature* survives, somehow. Do you think you would be proud to run another such?" (Weldon 1901a). Since *Nature* is not technical enough to publish "real" results, nor as loose as the "evening papers," discussion there must "be more or less gaseous, and quote a lot of details, – otherwise one will have to fight the quotations out, letter by letter & comma by comma" (Weldon 1901b). Why, then, continue to publish there? First, such contributions served the important function of positioning projects with respect to the broader scientific community as a whole, an angle on the biometry-Mendelism debate that is not often enough considered by our more internalist approaches to the issue and the period. Second, the peculiar role of *Nature* meant that it would serve as one of the only possible places for the inter-camp debate between the biometricians and the Mendelians to take place. The technical work of both sides – though clearly often written with an attack on the opposing camp in mind – was too focused to really permit this kind of discussion, nor was such discussion suitable enough for the general populace to make the evening papers. We should thus expect, I think, that *Nature* would be a place where debates like these are over-represented.[11]

Scientific Communities in Crisis

If we take the biometry-Mendelism case as an example of a community in the middle of a

11 Writing for the official history of the journal, Ruth Barton even suggests that such controversies were actively courted by *Nature*'s early editorial team (unfortunately, this web resource now appears without author and citation information; see <https://www.nature.com/nature/about/history-of-nature>).

Kuhnian crisis, what lessons might we be able to draw about scientific theory change as a whole? First and most obviously, we should be hesitant to think that the structure of such communities will have many common features, or even many temporally stable features during the course of the crisis. Kuhn, for his part, describes crises as particularly all-consuming and significant. They constitute, he says, "a period of pronounced professional insecurity" (1996, 67–68), or occur as a result of the "breakdown of the normal technical puzzle-solving activity" (1996, 69). The data support this picture in part – this is precisely what we see from 1894–99 (figure 3), with the crisis figures forming a well-connected network at the center of biological discussion in one of the most significant journals in the field. But we see precisely the opposite of this from 1900–04. The crisis, which is taken by many historians to remain the central concern in the field in this later period, only finds itself on the fringe of the network of discourse. Normal science proceeds apace for the majority of the researchers that I have analyzed here.

Before I continue, I should pause to note that one might simply take this network data as evidence that, despite the emphasis placed upon this episode by historians like Provine, there quite simply *was no crisis* in the field at this point. And there are good reasons to think that some kind of more complex story is at work. As already mentioned, Vicedo has persuasively argued that we should be skeptical of "forcing many historical actors into the monolithic and static categories of Biometry and Mendelism" (1995, 374).[12] And the unpacking of the biometry-Mendelism controversy into a variety of separate, independent research debates, as performed for

12 Both this critique, as well as Vicedo's apt worry about the scope of Kim's analysis, particularly the justification of which scientists are included and which are left out, are simply inherited by my work here. Many of the concerns with the analysis which I mentioned above are identified precisely because they would help us address Vicedo's worries.

example by Olby (1989), could also be read as casting doubt on the idea that there is *the* biometry-Mendelism debate, as opposed to *a cluster* of related debates. On the one hand, I am sympathetic to the idea that we have likely over-simplified the kind of debates that were occurring in this period. Weldon's archival materials, for instance, indicate a wide array of concerns with the life sciences of his day, including chromosome theory, agricultural breeding, statistics, probability theory, measurement, and experiment. But, on the other hand, the data from 1895–99 are fairly compelling: there *was* a controversy here, significant enough to dominate the literature for several years. It resulted in the redistribution of academic resources, the founding of journals, and altered the creation and dispensation of professorships. By the final time period (and even to some extent from 1905), there is no detectable signal that the discussion is continuing in the literature. The debate thus, as a matter of empirical fact, disappeared. There is therefore a perfectly coherent way in which to read the data that supports the existence of some form of crisis – though perhaps only a crisis on a very small scale[13] – and resolution.

Returning to the broader morals that we might draw from this case, another notable feature is the ability (or lack thereof) of scientists involved in crisis to move back and forth between debates concerning the crisis itself and the work of "normal science," which continues to move forward during the period. To take just a few examples from the case study, Pearson seems to be particularly adept at moving into and out of the crisis debate. At least as measured by clustering in the network of discourse, he is involved in 1894–99, not involved from 1900–04, and

13 Kuhn himself argued that revolutions might happen on any scale, from those as broad as the Copernican to those as fine-grained as the development of a new piece of technical methodology by scientists studying the use of X-rays (Kuhn 1996, 92–93).

involved again from 1905–09. This forms a stark contrast with Weldon, who from 1890 onward can, it seems, do very little in the pages of *Nature* but contest Bateson. Bateson lies in the middle, involved in the debate for around fifteen years, but recognizing his victory after Weldon's death. The explanation for these responses, on the other hand, is less obvious, and another place for further fruitful work. Some of the relevant factors will likely be personal. In a letter to Pearson, after he had been invited to a public debate on Mendelism at the British Association, Weldon writes that "what I hate is that I want to get a definite result. I want the thing to be proved nonsense. That is a thoroughly unhealthy and immoral frame of mind, and I expect it will lead to a well deserved smash" (Weldon 1902). Bateson was similarly attracted to personal controversies (Cock and Forsdyke 2008). Other explanations, in turn, are likely to be institutional. While Weldon's Oxford position was quite comfortable, he regularly lamented his inability to obtain quality students, while Bateson had a revolving cadre of devotees at Cambridge. The interplay of these and other influences deserves further study.

A further feature of the temporal evolution of this network is evocative. Those most invested in the debate, in our example here – Weldon and Bateson – exhibit a tendency not to make themselves central players in the broader literature, but to marginalize themselves. This class of scientists – we might call them *paradigm debaters* or *paradigm warriors* – seems to sacrifice their connection to what remains of normal scientific practice, instead focusing singlemindedly on the active crisis-debate. If this is a recurring feature of scientific crises, it has gone entirely unremarked upon in the literature, and deserves to be examined in further case studies.

This leads us to one final question. Do any features of the digital analysis point toward general features of scientific crises that are exemplified in this case? In particular, the biometry-Mendelism controversy is often pointed to as an example of "fruitless" debate – had the interlocutors only been able to see past their petty differences, it is sometimes said, we could have potentially seen the development of the Modern Synthesis several decades earlier than it actually appeared. While I think this latter claim is both historically unfounded and does not do nearly enough justice to the legitimate problems with which the biologists in this period were wrestling, it is notable the extent to which the two sides were unable to come to anything like worthwhile exchange. Several of the features exposed by the digital analysis might help us understand why that would have been so. The presence of these "paradigm warriors," along with their lack of mobility in and around the networks of discourse, might indicate that the debate had "degraded" – that the partisans truly invested in it were no longer able to argue about the merits in the course of performing normal-scientific, technical work and were reduced to public fighting in a non-technical venue. We see this to some degree mirrored in the private work of the biometricians, who had in large part decided by the mid-'00s that the way to prevail was not to engage directly in what we might call "productive" debate with the Mendelians, but rather to produce a theory that could convince others of the validity of the biometrical program independently of its role as weapon in the debate (see, e.g., the work of Weldon described after his death in Pearson 1908). In that sense, this case offers us an example of how not to fight about theory – disengagement on a technical level combined with acerbic argument on a public level seems to have rendered it more acute and more personal.

In short, not only has the analysis of the network of discourse proven fruitful for the biometry-Mendelism debate, it has unearthed a number of features of it that have not been sufficiently studied. In turn, many of these point toward questions about crises and scientific revolutions more generally, a compelling set of problems for any account of theory change in the sciences, from Kuhn to today. Digital analyses can, indeed, make good on their two-fold promise: both to reveal interesting facts about the philosophy and history of science, and to generate novel research questions that we would have been likely to miss without digital aid.

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