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**Article:**

Radick, G (2016) Presidential address: Experimenting with the scientific past. *British Journal for the History of Science*, 49 (2). pp. 153-172. ISSN 0007-0874

<https://doi.org/10.1017/S0007087416000339>

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# Presidential address

## Experimenting with the scientific past†

GREGORY RADICK\*

**Abstract:** When it comes to knowing about the scientific pasts that might have been – the so-called ‘counterfactual’ history of science – historians can either debate its possibility or get on with the job. The latter course offers opportunities for engaging with some of the most general questions about the nature of science, history and knowledge. It can also yield fresh insights into why particular episodes in the history of science unfolded as they did and not otherwise. Drawing on recent research into the controversy over Mendelism in the early twentieth century, this address reports and reflects on a novel teaching experiment conducted in order to find out what biology and its students might be like now had the controversy gone differently. The results suggest a number of new options: for the collection of evidence about the counterfactual scientific past; for the development of collaborations between historians of science and scientific educators; for the cultivation of more productive relationships between scientists and their forebears; and for a new seriousness and self-awareness about the curiously counterfactual business of being historical.

To live through a scientific anniversary is, for the historian of science, to have a chance not merely to observe but to participate, maybe even to affect what happens next. 2015–16 saw a number of Gregor Mendel anniversary meetings. These marked 150 years from when Mendel, in February and March 1865, gave two lectures on a series of experimental crosses he had made using pea varieties in the garden of his monastery in Brunn – lectures that he published the following year in the annual proceedings of the Brunn Natural Sciences Society, under the title ‘Versuche über Pflanzen-Hybriden’ (‘Experiments on Plant Hybrids’). Our biological science has long looked back on Mendel’s extraordinary paper as the foundation statement for genetics: the study of inheritance understood as the study of the behaviour of discrete trait-determining entities, genes.<sup>1</sup>

Could it have been otherwise? Might there have been a flourishing science of inheritance that did not venerate the discovery of the Mendelian gene as its starting point? I want in what follows to explore the potential of such ‘counterfactual’ questions to enable new kinds of historical observation as well as new kinds of scientific participation by historians of science, whether anniversary-minded or not. I shall dwell especially on the case of Mendelism, and the light thrown on that case by a recent experiment in the teaching of genetics. But I will range widely, across periods and sciences, through high and low culture, in order to suggest something of the general interest of the scientific pasts that might have been, the challenge of learning more about them, and the rewards for historians of science who trouble themselves to do so.

## **An alternative to Mendelism: W. F. R. Weldon's emphases on variation, ancestry and the environment**

In the spring of 1900 three European botanists published papers reporting not merely the patterns Mendel had found in his hybrid pea lineages but his explanation for those patterns. That striking coincidence brought Mendel's name – hitherto known only to specialists in hybrid plants – into general biological discussion, where two English biologists, William Bateson FRS and Walter Frank Raphael Weldon FRS, took notice.<sup>2</sup> Bateson, based at Cambridge (where he had studied biology alongside Weldon in the 1870s and 80s), rapidly persuaded himself that Mendel's work was fundamentally important and deserved pride of place in a new, quantitative, experimental science of inheritance. Under Bateson's energetic leadership, a growing corps of self-identified 'Mendelians' made it their mission to extend Mendelian analyses ever further, developing its theoretical principles and its practical applications, especially in agriculture but also in medicine. Bateson's coinage of 1905-6, 'genetics', eventually displaced 'Mendelism' as the name for the new science.<sup>3</sup>

While Bateson was transforming himself into Mendel's greatest disciple, Weldon, who held a chair at Oxford, was transforming himself into Mendel's (and Bateson's) greatest critic. Early in 1902, in a journal Weldon had helped to found, *Biometrika*, devoted to 'the statistical study of biological problems', he published an article now best remembered as the first to show that Mendel's data examined statistically are too good to be true. But for Weldon, as has become clearer thanks to recent studies of his unpublished writings, that empirical problem was but a symptom of a much larger conceptual one. Everyone will recall from school biology that Mendel dealt with binary characters – with, say, the colour of the pea seed as either green or

yellow, the seed shape as either round or wrinkled, and so forth. According to Weldon, no other breeder's peas looked like that. In the article he included a coloured photographic plate showing pea seeds he had collected from commercial pea breeders in Britain and France. Yes, some peas were unambiguously green, and some unambiguously yellow. But between those extremes were varying shades of greeny yellow to yellowy green: a colour continuum.<sup>4</sup> The lessons he drew for inheritance and its study were threefold:

(1) Variation matters. It matters that real peas are not just yellow or green, and are not just round or wrinkled. Actual variability should not be idealized away, by the use of simple categories such as 'yellow' or 'round' treated as descriptions of reality. Statistics is the descriptive language appropriate to biology because biological populations are always variable and statistics captures that variability. (Hence 'biometricians', the name by which Weldon's side of the debate over Mendel's work became known.) And because that variability might itself turn out to hold indispensable clues to how inheritance really works, a science of inheritance that deliberately disguised variability, that encouraged the forgetting of it, was not moving in the right direction. Or so Weldon thought.<sup>5</sup>

(2) Ancestry matters. The particular ancestry, the particular lineage that these peas come from, as distinguished from those peas, can matter. Again, from Weldon's perspective, the trouble with Mendelism – Mendel's paper unjustifiably blown up to biological world view – was that it encouraged ignorance and incuriosity, in this case about the deep ancestry behind any particular individual's characters. According to the Mendelian, as long as true breeding green-seeded peas are producing green-seeded peas, that was all

one needed to know about them. Weldon, impressed by Francis Galton's teachings about the long reach of ancestry, thought that was a mistake, and one connected to the Mendelian mistake about variability. Behind all of that variability the Mendelians ignored might lie the different kinds of ancestry the Mendelians ignored.<sup>6</sup>

(3) Environment matters. As Weldon saw it, from within a lineage, an individual inherits not just this or that 'factor' or 'determinant element' or 'gemmule' or whatever but a whole suite of them, and – crucially – these constitute a context, an environment, conditioning the visible effects of any one of them. Change the environment, and you can change the effect. In a book-length Theory of Inheritance manuscript that Weldon worked on throughout 1904-5, the conditioning role of environments – not just cellular but more generally physiological and physico-chemical – was a major theme. Indeed, Weldon took that theme to be one of the main conclusions of experimental embryology in its glory period towards the end of the nineteenth century, and so, surely, something that any science of inheritance worth having had to take into account. In trying to articulate that science himself he had avoided the language of 'acquired characters', as he explained in the manuscript, because no character is either all-acquired or all-inherited, but all are always the joint product of what was inherited and what was around it.<sup>7</sup> Furthermore, and again, he took this contextualist message – and the related ones about variability and ancestry – to be thoroughly Galtonian. Our culture remembers Galton, coiner of 'eugenics', as a doctrinaire hereditarian, adamant that nature beats nurture across the board. But that was not at all Weldon's reading of Galton. And Weldon was probably the most careful reader of Galton there ever was.<sup>8</sup>

The upshot for Weldon was that the Mendelian picture of ‘dominance’ as absolute— so, for example, of yellowness as dominant to greenness, no matter the context – was deeply misleading. In Weldon’s view, to run a Mendelian experiment was deliberately to exclude all of the variability that would otherwise produce different kinds of pattern. If an experimentalist were so minded, he thought, a race of peas could be established in which greenness was dominant to yellowness. It all depended on the choices made, the contexts built. And to declare one pattern the natural one, and other patterns as somehow deviant, was just arbitrary. No, what biologists needed was a concept of dominance which treated it as context-dependent.<sup>9</sup>

Weldon arrived at this perspective in the course of an increasingly bad-tempered debate with Bateson and his allies. Now known as the ‘biometrician-Mendelian controversy’, it ended only when, in the spring of 1906, Weldon – the leading biologist on the biometrician side, though his friend and collaborator, the London-based mathematician Karl Pearson, has got more historical attention – died unexpectedly after a short illness.<sup>10</sup> Mendelism went on to become one of the great success stories of twentieth-century science. In 1910 in Brünn, Bateson watched the dedication of a statue of Mendel installed in a square next to his monastery.<sup>11</sup> More than a hundred years later, in classrooms around the world where the subject is genetics, students early on are taught about Mendel and his experiments crossing peas. In a well-constructed introductory genetics course at university level, students are inducted by degrees into ever more complex variations on the basic Mendelian example, in all the ways that Thomas Kuhn has made familiar to generations of historians of science. Genetics has moved on in all sorts of ways since the early twentieth century; but instruction in genetics, once organisms are in the picture, keeps

faith with its beginnings, making room for later innovations in a structure whose core remains forever Mendel's.<sup>12</sup>

That is how it happened. But was it bound to happen that way? Was it a foregone conclusion that our biology would, somehow or other, at some time or other, recognize and enthrone something like the Mendelian gene? Or, on the contrary, might Weldon's alternative biology of inheritance, so insistent on the conditioning role of context, been the success story? Could Weldon's account have been at least more successful than it actually was, had circumstances somehow been different, more favourable – if, say, Weldon had been more adept than he was at recruiting talented and zealous students, or had simply lived to finish and publish his *Theory of Inheritance*? Supposing history had taken a different, Weldonian turn in the early twentieth century, might we now have a Weldonian biology of inheritance, just as successful as the Mendelian one became, but with a different kind of intellectual complexion? And whether it might have or might not, why should we care?

### **Counterfactuals and the history of science**

I come now to my title, 'Experimenting with the scientific past'. What I have just wondered about is what has come to be known as 'counterfactual history': what did not happen but might have. We can describe such wonderings, if we want, as experiments – experiments of the imagination or, if 'imagination' talk seems somehow un-serious, conceptual experiments, thought experiments. In the laboratory of the mind, we change something in the actual past and reason through the consequences. 'Altered pasts' is how the eminent historian of modern Germany Richard J. Evans labels the results. If that phrase sounds a touch sinister, affiliating the

counterfactual historian with totalitarian rewriters of history, that is in keeping with Evans' disapproving attitude towards the genre. His 2014 book *Altered Pasts: Counterfactuals in History* declares counterfactual history fit for examination mainly as a historical phenomenon in its own pathological right – a sign of intellectually and politically unhinged times, when people who cannot face reality console themselves with wish-fulfilling fantasy.<sup>13</sup>

We need a less polemical guide in thinking about counterfactuals-as-experiments. I find Ian Hacking's *Representing and Intervening* (1983) endlessly stimulating on the subject of experiment, and Hacking's writings overall salutary in the care he takes to be attentive to when words are being used literally and when not. When we theorize, we represent; when we experiment, we intervene. It is not literally the case that we can experiment with the scientific past in the way that we can experiment with, say, water. Water is around, accessible, available for scientifically motivated interventions (removing dissolved impurities, running an electric current through, and so on). The scientific past is not like that, not even remotely. It is completely inaccessible, un-intervenable upon. So, if we are being experimental when we go counterfactual about the past in the laboratory of the mind, those experiments are not literal experiments in the way that watery interventions are.<sup>14</sup>

Fussiness at this stage will pay dividends later, when I will recount a literal experiment that does bear, I contend, on our knowledge of the counterfactual scientific past. But even that will fall short of the standard of a bona fide, literal-experiment intervention in the scientific past. As for not-even-remotely-literal experiments with the past, of the conjectural kind that exercise Richard Evans – what if Britain had not entered into the European war in 1914, and so on – we can describe them, if we like, as thought experiments in the laboratory of the mind, or 'virtual history' (Niall Ferguson), or 'replays of the tape' (Stephen Jay Gould). And there may well be

some intellectual mileage to be gained in regarding the operations that these label as continuous rather than discontinuous with what happens in a literal experiment. But counterfactual conjectures are not in themselves experimental interventions.<sup>15</sup>

What about counterfactuals and the history of science in particular? I think the most important points come down to three. First, counterfactuals are pervasive, like it or not, because historical explanation is a counterfactual business. Second, counterfactuals can express basic visions and divisions about science (and its connections to history, knowledge and reality). Third, the epistemology of counterfactuals – why we so often fictionalize in order to get closer to the facts – is mysterious. Let me expand on each of these in turn, before coming to what I take to be their overall thrust.

(1) Counterfactuals are pervasive, like it or not, because historical explanation is a counterfactual business.

Why do people go counterfactual? They do it when they are probing the past to figure out what mattered, what made the difference, how much of a difference something made. My first point embeds a thesis about the logic of historical explanation that I am not going to defend outright. I want instead to illustrate pervasiveness with some examples.<sup>16</sup>

To begin, I go back to Weldon. You might be inclined to think it was misdirected nit-picking on his part to point out that peas are not all either green or yellow. I think Weldon was sensitive to being understood in that way. It is the more striking that when he lectured students at Oxford in the summer of 1905 on good scientific method in the study of inheritance, he offered them a lesson wrapped in a historical counterfactual. The year before, the Nobel Prize

had gone to Ramsay and Rayleigh for the discovery of argon in the mid-1890s: work that inaugurated the discovery of a whole new column of the periodical table, the noble gases. On Weldon's telling, 'The discovery of argon was ... directly due to a refusal to replace the variable and discordant experience of the weight of nitrogen by an ideal uniformity based on the mean of actual experiences.' The difference in two datasets that they were working with was trifling. Other, lesser scientists would have overlooked it. But these great ones understood that the tiniest of differences can be a clue. Weldon went on with a pair of counterfactuals. First: 'If Lord Rayleigh had replaced all the results in his table by a single compromise between them, and had been content to stop there, we should not know the existence of argon to-day.' A fundamental discovery depended on not allowing oneself off the hook with a discrepant datum. Second (making the same point but, as it were, from the other end): if previous investigators 'had not been content to replace their experience and that of Cavendish by a compromise which neglected his residual bubble, argon must have been discovered long ago.'<sup>17</sup>

Nearer to our own day, we can find some distinguished examples of professional historians and philosophers of science pursuing explicitly counterfactual inquiries, notably James Cushing's *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony* (1994), on how physics might have gone Bohmian but for the accidents of history, and Peter Bowler's *Darwin Deleted: Imagining a World without Darwin* (2013), on how biology might have developed if Darwin hadn't published the *Origin of Species* in 1859.<sup>18</sup> The example I want to consider here, by way of illustrating counterfactual pervasiveness, does not come from Bowler's book, but is to do with Darwin, indeed with one of the most vexed and complex questions surrounding his achievement and its legacies. How much of a difference did Darwinism make to what happened in Nazi Germany? Here is what I regard as the most insightful thing anyone has

written on this topic. Because I want to bring the reasoning into focus, let us take it in two parts. The first part, is, as historical writing, quite ordinary, though what it recounts is anything but:

Selectionist Social Darwinism – the idea that human society was governed by the struggle, whether between races or between individuals or families, for the survival of the fittest – and the language and concepts which it inserted into welfarist and criminological discourse before the First World War, had a widespread and growing influence in the discussion of social problems at this time.

So far, so standard. Something happened. The language of ‘survival of the fittest’ permeated welfarist and criminological discourse. So what? To be clear about how much that language mattered, our author goes counterfactual:

.... [W]ithout the emergence of this language, Nazi ideology would not have been able to develop as it did. And its spread during the Weimar Republic helped reconcile those who used it, and for whom it had become an almost automatic way of thinking about society, to accept the policies which the Nazis advocated and in many cases to collaborate willingly in putting them into effect.

That is a very strong statement. Not only did Darwinism matter to what happened in Nazi Germany, but it mattered in a big way. No Darwinism, and nothing like the lethal potency of Nazi ideology as we actually had it.

Who is our author? Richard Evans.<sup>19</sup> So, when he is being a (brilliant) historian, he helps himself to counterfactual language in order to clarify why past events happened in the way that they did. Yet when he reflects on his practice as a historian, he regards counterfactualism as bunk. Cognitive dissonance has its place, but not in the theory and practice of history.

(2) Counterfactuals can express basic visions and divisions about science (history, knowledge, reality)

A splendid new collection, *Science as It Could Have Been: Discussing the Contingency/Inevitability Problem* (2015), shows that, in taking up the theory and practice of counterfactual history of science, I am far from alone in owing a debt to another of Ian Hacking's books. In *The Social Construction of What?* (1999), Hacking proposed that, for all the silliness of the 'science wars' of the 1990s, there was a serious and interesting question at issue about the nature of science, and it was most perspicuously expressed in counterfactual terms.<sup>20</sup>

On the one side was a traditionally reverential view of how science works, articulated as a kind of inevitabilism about science. By way of examples, consider two Englishmen who, I wager, I am the first person ever to put together: the politician, historian, and essayist Thomas Babington Macaulay (1800-1859) and the TV presenter and car enthusiast Jeremy Clarkson (1960-). In 1828, in a typically magisterial essay on the poet John Dryden, Macaulay argued that, in understanding Dryden's achievement, one must not forget that 'it is the age that forms the man, not the man that forms the age'. When the age calls something forth, circumstances determine which man will be its instrument. This has the result that, however meritorious, any individual ends up playing a celebrated or notorious role in history as a matter of happenstance.

Thus, in the sciences, the remarkable regularity of what would later be dubbed ‘multiples’: in mathematics (Newton versus Leibniz on the calculus); in political economy (Malthus and less well-remembered figures on the theory of rent); and beyond. ‘We are inclined’, wrote Macaulay, ‘to think that, with respect to every great addition which has been made to the stock of human knowledge, the case has been similar; that without Copernicus we should have been Copernicans, that without Columbus America would have been discovered, that without Locke we should have possessed a just theory of the origin of human ideas.’<sup>21</sup>

To a certain sensibility, the pairing of Macaulay and Jeremy Clarkson joins the sublime to the ridiculous. But in 2003 Clarkson received an honorary degree from Brunel University in London for his services to the university’s namesake – and Macaulay’s exact contemporary – the Victorian engineering impresario Isambard Kingdom Brunel. The services were rendered on a BBC television debate over ‘the greatest Briton’. Clarkson so ably put the case for Brunel that, in the vote following the debate, Brunel came in at number two, behind Churchill, but ahead of Shakespeare, Darwin and the very distinguished rest of the list. On my reckoning, at least, a key moment in Clarkson’s triumphant campaign came during an exchange with Darwin’s defender, when Clarkson put it that whereas no one but Brunel could have pulled off those stupendous bridges, ships and railways, someone else would have come up with the theory of natural selection eventually if Darwin had not done so. Indeed, as he pointed out (multiples again), someone else did, and at more or less the same time, Alfred Russel Wallace. As a debating move, this one could hardly have been bettered: a kind of rhetorical checkmate. If Darwin’s defender agreed, then Brunel would trump Darwin. And if Darwin’s defender disagreed, then – for all that Darwin’s and Wallace’s formulations of natural selection theory might legitimately be considered non-equivalent – natural selection would end up looking not as worthy of esteem as

previously thought, because too dependent for comfort on the workings of an individual mind. Scientific truths are not supposed to be like that; they are supposed to be out there, waiting, discoverable, independent of the merely human. In some hard-to-shake way, all of this is part of what we mean by 'true'. There was no good answer to Clarkson, and none given. Brunel wins (almost).<sup>22</sup>

Above I described the inevitabilist view of science as reverential, but its reverence towards science translates into irreverence towards individual scientists, who turn out not to matter all that much really.<sup>23</sup> For genetics, here is an early statement of that same sensibility at work. It comes from an article by the American anthropologist Alfred Kroeber, writing a little bit more than fifteen years after the triple rediscovery of Mendel's paper. Already it was a canonical multiples episode. Here is Kroeber on the events of the spring of 1900:

There may be those who see in these pulsing events only a meaningless play of capricious fortuitousness; but there will be others to whom they reveal a glimpse of a great and inspiring inevitability which rises as far above the accidents of personality as the march of the heavens transcends the wavering contacts of random footprints on clods of earth. Wipe out the perception of [the rediscoverers] De Vries, Correns, and Tschermak, and it is yet clear that before another year had rolled around, the principles of Mendelian heredity would have been proclaimed to an according world, and by six rather than three discerning minds. That Mendel lived in the nineteenth century instead of the twentieth, and published in 1865 [sic], is a fact that proved of the greatest and perhaps regrettable influence on his personal fortunes. As a matter of history, his life and discovery are of no more moment, except as a foreshadowing anticipation, than the

billions of woes and gratifications, of peaceful citizen lives or bloody deaths, that have been the fate of men. Mendelian heredity does not date from 1865. It was discovered in 1900 because it could have been discovered only then, and because it infallibly must have been discovered then.<sup>24</sup>

That is the inevitabilist tradition. It helps to keep in view both the inevitabilism and the traditionalism when asking what, exactly, can be so challenging about the challenger -ism, social constructionism. Against inevitability, social constructionism posits contingency, and on its basis upends traditional pieties about science and scientists alike. Examples? In certain respects, contingentism and associated attitudes have gone so deep among several generations of historians of science as to have passed into the scarcely noticed methodological background. To write as though everything in the past was up for grabs all the time in every possible way is to ensure that one is never, ever accused of 'Whiggism'. For Hacking's part, when he gave examples, he dwelt on works by Bruno Latour and Andrew Pickering. For a BJHS readership, I prefer a familiar passage from Steven Shapin and Simon Schaffer's *Leviathan and the Air-Pump* (1984): '[W]e want to show that there was nothing self-evident or inevitable about the series of historical judgments in that context [of the Hobbes-Boyle debate] which yielded a natural philosophical consensus in favour of the experimental programme. Given other circumstances bearing upon that philosophical community, Hobbes's views might well have found a different reception.' So, there was nothing intrinsically doomed-to-failure in Hobbes' critique of Boyle's apparatus-driven, laboratory-bound experimental methods. Our science now might not esteem the experimental method in the way that it does had things gone differently back in the late

seventeenth century. That is an almighty counterfactual assertion if ever there was one, and it can still set pulses racing – well, my pulse anyway.<sup>25</sup>

(3) The epistemology of counterfactuals is mysterious (we fictionalize in order to get closer to the facts).

How can it be possible to know about what did not happen but might have happened? What kind of knowledge is counterfactual knowledge? Are there new kinds of consideration that come into play as we scale up in complexity, from the simple cases favoured by philosophers to the very unsimple ones that the likes of Richard Evans (and historians of science) find themselves asserting? Why are some counterfactual claims convincing and others less so? And, more generally, why is counterfactual thinking and language as pervasively a part of human knowledge and discourse as it is? What is it about fictionalizing, and about human understanding, that seems to take us closer to the facts? Nobody has good answers yet to any of the above, nor is any discipline especially well set up to make headway. There is a great deal of undoubtedly interesting and potentially valuable work to do.<sup>26</sup>

What to do? We can either debate the possibility of counterfactual history of science or get on with the challenge of doing the job better. It will be plain which of these options I prefer.<sup>27</sup> My own improvement efforts in counterfactualist method have concentrated on problems of evidence, most recently the prospects for acquiring, in post-Boylean fashion, experimental evidence about a scientific past that never was but might have been.

## **Experimenting with the Weldonian biology that might have been**

Here we come to what, in methodological terms, seems to mark a difference, and for the better, between counterfactual history of science and counterfactual history of pretty much everything else. In expressing the contrast, we can do worse than return to Hacking's great dualism, representing and intervening. If I am curious about what might have happened had, say, the losing side in a battle or election been the winner, the methods at my disposal for satisfying that curiosity are all, in one form or another, representational. I can think about it, write about it, maybe even program a computer to run a simulation. I cannot actually go back and meddle. But if I am interested in what might have happened had the losing side in a scientific debate been the winner, or even just not as big a loser as it actually was, there is nothing in principle preventing me from undertaking an intervention: the bringing back into being, now, of that losing-side science.

Of course, a host of abstract difficulties and puzzles immediately present themselves. But let us, in the spirit of my scholasticism-bashing manifesto above, put those aside for now and see how far we can get with a little concrete action.<sup>28</sup> My own efforts along these lines have concerned the losing-side science I introduced earlier: Weldonian biology. I take its intellectual essence to be well expressed in the following sentence, referred to earlier, from Weldon's Theory of Inheritance manuscript:

Since the character of any organ depends, not only upon a specific something transmitted to it through the germ-cells out of which it was developed, that is to say upon something inherited, but also on two sets of conditions external to the organ itself, namely its

relation to the parts of the body to which it belongs and its relation to the environment in which that body exists, we may say that every character of every animal is both ‘inherited’ and ‘acquired’.<sup>29</sup>

I cannot, anytime soon, usher into existence the Weldonian world of my counterfactualist dreams, full of laboratories, fields and clinics that are themselves full of biologically and biomedically trained people who approach inheritance in such a thoroughly interactionist, inextricably nature-and-nurture manner. But the materials that might educate such people are another matter. Why not try and develop a Weldonian curriculum in introductory genetics and then – passing from representation to intervention – teach it to students? To get them pointed in a Weldonian direction from the start, the first meeting would dwell not on Mendel’s pea experiments – the traditional ‘genes for’ curriculum – but on an example where the effects of genes have to be understood alongside the interacting effects of many other causes – as with, say, the healthiness of otherwise of a human heart. From that beginning, the teacher, the readings and associated activities would seek to press home, at every possible opportunity, the message that genes have the effects that they do on bodies only in developmental and, more broadly, environmental contexts. Where the traditional curriculum typically treats the Weldonian emphases on variation, ancestry, and environments as largely ignorable luxury items, the Weldonian curriculum would treat them as primary. It would likewise dispense, so far as possible, with ‘dominance’. And when the time came, eventually, to introduce Mendelian patterns, the students would learn to regard these not as showing inheritance at its most basic – as the great generalizations – but as special cases, exceptions to the rule, interesting precisely

because, for explicable reasons, they can be understood as if genes acted independently of their contexts.

Suppose one could actually teach this curriculum, and get it, as pedagogy, working well enough that the students would not simply rush for the exits in dismay and confusion after the first meeting, but would instead stay the course, right through to the end. What would these students be like? In particular, how different would they be when it comes to holding that exaggerated view of the power of genes that our academic culture calls ‘genetic determinism’? The most exciting possibility, of course, is that these students would be interestingly different, and in a particular way. They would be less prone than students coming out of a traditional Mendelian course to accept unquestioningly the latest news reports about ‘genes for’ autism or schizophrenia and so on. They would be more sensitive to the ways in which individual differences can really matter in whether a BRCA mutation or other ‘disease gene’ mutation will lead to illness in any particular individual. They would be more willing to demand more information from the experts, not just about genes, but about the genetic backgrounds and range of environments in play in the research warranting claims to know what genes are ‘for’. They would, in other words, be less determinist.

From early days, Mendelism and a kind of simple-minded determinism travelled hand in hand. I was pleased, in preparing this address, to find an example relevant to my Welsh setting, in Ted Porter’s marvellous biography of Karl Pearson. During the Great War, Pearson kept a diary in which he recorded an encounter with a Mendelian. On Pearson’s exasperated account, the latest hypothesis from this permanently misguided fellow was that ‘music in the Welshman was a “Mendelian recessive”’. That kind of reductive determinism incensed Pearson.<sup>30</sup> After the war, the Mendelian in chief, Bateson, recalled the response of a Scottish soldier after listening to

a lecture from Bateson's: 'Sir, what ye're telling us is nothing but Scientific Calvinism'.

Bateson thought that was spot-on, even considering Scientific Calvinism to be an apt title for a contemplated, but never completed, collection of his popular writings and addresses on genetics.<sup>31</sup>

In our day, textbook Mendelism-determinism continues, entrenched. At the research frontier in biology and biomedicine, meanwhile, and in historical, philosophical and pedagogical studies of these fields too, interaction is where the action is. Consider the New York City-based Resilience Project, which encourages healthy people to have their whole genomes sequenced on the expectation that some will be found to carry genes which, according to the textbooks, should have been lethal long ago. Identifying such people is a first step towards figuring out what else is going in their genomes, bodies, environments to make those 'genes of doom' (in Steven Pinker's phrase) anything but. The hope is that medical science will be able to draw on that new knowledge of genes-in-context in order to develop a next generation of diagnostics and therapies.<sup>32</sup> In a similar vein, a recently published survey article on the genetics of neurodevelopmental disorders notes that 'while a mutation associated with hemochromatosis or breast cancer might have high expression in one particular pedigree or clan, that same mutation may have very low expression in another pedigree, clan or group of unrelated people'. Elsewhere in this paper we find an explicitly Weldonian conclusion drawn: maybe the time has come to 'just discuss the expression of each trait in the context of a phenotypic spectrum, which is of course what led Walter Frank Raphael Weldon to establish the field of biometry'. One of the co-authors of this paper – and the Weldon fan – is Gholson Lyon, an unusual combination of human geneticist and clinical psychiatrist. In other ways too he is an instructive figure, to whom I will return towards the end.<sup>33</sup>

Closer to home in disciplinary terms, the interactionist case has been put most influentially by Evelyn Fox Keller. The title of her book *The Century of the Gene*, published in 2000, was meant not to praise but to bury. One hundred years after the Mendelian rediscovery, and with the Human Genome Project entering its final phases, the time had come, in Keller's view, to relinquish the gene concept as no longer fit for scientific purpose. Up to a point, the twentieth century had managed well enough with the notion of genes as little atoms of heredity. They marched through the generations heedless of context, their structures and functions neatly individuated and stable over time, their autonomy in bringing about complex traits taken for granted. But by century's end, the gap between that notion and what was actually known had grown intolerably large. If the twenty-first century is to realize the potential of the genomic age, it needs to find new concepts and new language, better matched to biological reality (though quite what those concepts and that language were Keller did not say). Jenny Lewis, a specialist in genetics education, brought a similar message to those involved in teaching genetics in school. In 2011, Lewis warned that teachers who, not wanting to confuse students, rely on simple examples – where one gene produces one protein that in turn affects one character – risk pedagogical damage. For in so doing they may inculcate in their students 'a deterministic view of genetics in which every characteristic is determined by a single gene', thus making it harder for those students to absorb the interactionist message of genetics in the genomics age.<sup>34</sup>

By happy coincidence, Lewis turned out to be a colleague of mine at the University of Leeds; and together with a third Leeds colleague, Annie Jamieson, we got a chance to run a pilot-scale version of the Weldonian curriculum experiment. Our Genetics Pedagogies Project, as we came to call it, started in autumn 2012 and ended two years later.<sup>35</sup> The basic design was to work with two groups of students, one taking Weldonian genetics (the experimental group),

the other taking Mendelian genetics (the control group). Both groups were assessed beforehand and afterwards on their ‘belief in genetic determinism’, to use the phrase of the pioneer questionnaire-makers in this area. Under ideal conditions, the project would have recruited from a single homogeneous pool of students; randomly selected which individuals got put into which group; used the same teacher for both groups; offered those groups courses having the same number of lectures of the same length delivered in the same style; and so on. Alas, our project did not take place under these conditions. Our experimental group consisted of about thirty humanities (mostly philosophy) students at the start of their second year, while our control group consisted of about the same number of first-year biology students. Where the students in the experimental group met for nine extracurricular one-hour lectures taught by a member of the project team, and had associated reading and blog tasks to do, the students in the control group met for more than thirty credit-bearing lectures, in a large lecture hall with hundreds of other students, were taught by a member of the School of Biology, and had associated reading plus labs and problem sets. The incomparabilities introduced by all these differences are, to put it mildly, considerable. Our students entered their courses with more or less the same attitudes towards genes and their determinative power.

Annie Jamieson wrote the Weldonian course, taught it, and collected all of the relevant data. She knew Weldon’s work intimately, through collaborating with me on a scholarly edition of Weldon’s Theory of Inheritance which we expect soon to send to press. Better still, she is a developmental biologist turned historian of science, and a truly wonderful teacher. By the time this address is published, her teaching materials, including her lectures in the form of audio recordings coupled to slide presentations, will be freely available online at the project website.<sup>36</sup> For present purposes, a brief and imperfect overview of her first lecture, on the question ‘what is

genetics?', can serve to give at least a feeling for what it was like to be introduced in Annie's classroom to Weldonian genetics. She starts with inheritance as we observe it in the relations of parents and their offspring. Humans have human babies, horses have foals, tigers have baby tigers, and so on. There is always a sameness that gets preserved, and yet there is difference as well, and different degrees of difference. A baby tiger is much more like its parent than a foal seems to be like its parent. The children in a human family more closely resemble their parents than they will resemble the parents of other children, but are nevertheless different from their parents, and different from each other. To be interested in inheritance scientifically is to want to understand all of that: the sameness that gets preserved over time and the differences too.

From there we move to consider inheritance and development. In typical biological educations, genetics is handled in one course and development in another, separate course. In our Weldonian classroom, by contrast, students encountered genetics from the first as fundamentally tied up with development. Mature characters are not what are inherited. What are inherited are genes plus cytoplasm and, more generally, a developmental context. To think about how genes affect bodies is to think all the time about the developmental context in which genes function. With the next step that point gets affirmed and enlarged with a look at organism and environment, and an invitation to the students to think expansively about environments. These aren't, on reflection, just one thing, but many different kinds of thing. Your family is an environment of a sort. So is the climate. Your diet. What are your environments and how do they affect you? While the question lingers, we move on to two examples of genes and environment in interaction. In one the same genome with different environments produces very different results (queen and worker bees are genetically identical but the queen develops by being fed exclusively on royal jelly). In the other, different genomes in the same environment

give different results (mice given access to the same level of food get fat or not depending on whether they have a mutation in the OB gene, which, in its functional state, encodes a hormone called leptin, which regulates appetite and energy expenditure).

I admire, and would like to think Weldon would admire, that pair of examples as a vehicle for impressing upon students a sense of genes and environment not just as interactive but as symmetrical. Genes here are not presented as super causes, what inheritance is ‘really about’, with everything else involved playing a supplementary role. Genes are there in the mix, making things happen, but along with lots of other things. And so that first lecture carries on. If we skip ahead to the very end, we come – as in our anticipatory sketch– to the heart, specifically to cardiovascular disease and the question of the contribution that genes make. One by one, causal lines of influence are traced. Yes, genes can affect cardiovascular condition directly. But they can also affect other things which themselves affect cardiovascular condition, among them age, sex, alcohol, smoking, activity levels, obesity, diabetes, and blood pressure. The students then get an image representing all of those causal lines put together. It is as tangled a web of influence as could be. And where in there, the students are asked, is the ‘gene for’ cardiovascular disease? A hopelessly inapt question, as, we hope, they now see too.

That was the first lecture. To our delight, almost all the students who began the course stuck with it. So, by the end, did the genetic determinism of these students change or stay the same? And how did they compare with the control group? Of course, from the perspective of counterfactualist methodology, any data are interesting, whether they support inevitability (no change in determinist-belief levels) or contingency (the levels go down). Our data support contingency. Students coming out of the traditional Mendelian course were just as determinist about genes at the end as they were from the start, whereas students coming out of the

Weldonian course showed reduced belief in genetic determinism, and to a statistically significant degree. Mindful of the small numbers involved, the previously noted imperfections in the experimental design, and Mendel's own difficulties with exaggerated statistics (and Weldon's role in first publicizing them), I am reluctant to make too much of statistical significance here. But within the limitations of a proof-of-concept pilot study, these are encouraging results.

### **Making alternative pasts present: Science as complementary HPS (and vice versa)**

Too many unacceptable dualisms have come up in the above, most egregiously inevitability/contingency, but also representing/intervening, tradition/social construction, winners/losers, (f)actual/counterfactual, and science/history of science. All are ripe for reflection and revision.<sup>37</sup> The one I want to consider especially in closing is the last, science/history of science. My Genetics Pedagogies Project co-conspirators and I were in the midst of our project when we became aware that Rosie Redfield, a microbial genetics specialist based at the University of British Columbia, had published a manifesto calling for a new genetics curriculum. She called for dispensing with the accretions of history – Mendel's peas included – in order to give students more directly useful and relevant instruction. Yet, for all that Redfield's project and ours had broadly similar reformist ambitions, her curriculum was not in the least Weldonian, in that developmental context, and the Weldonian emphases on phenotypic variability, ancestry and environmental generally, were in no way prioritized in her 'Useful Genetics' course.<sup>38</sup> So a desire to update not-fit-for-purpose introductory teaching materials in genetics is probably a necessary but not a sufficient condition for revising those materials in a Weldonian direction. To reform with a view to diminishing support for genetic determinism, and to pursue that goal via

the uprooting of curricular Mendelism, it helps – maybe it is even indispensable – to have spent time in the intellectual company of someone like Weldon, who was educated before Mendelism took hold.

Be that as it may, our Weldonian curriculum does not, of course, represent anything like a total replacement of the existing curriculum. Rather, it seeks to take what is peripheral in the existing curriculum and make it central, and to take what is central and make it peripheral.<sup>39</sup> My inspiration for asking these sorts of questions at all comes from a work of counterfactual history of science: a 2008 essay by Peter Bowler on the history of science and society without Darwin's *Origin*. The essay was a first statement in miniature of what became Bowler's *Darwin Deleted*. But what perhaps came through more clearly in the essay was the refusal of the notion that, when conjecturing counterfactually, there are (those pesky dualisms again) only two options: either everything would have been more or less the same; or everything would have been more or less completely different. In the Darwinian case, Bowler reckoned that, without the *Origin*, biology would ultimately have come to embrace roughly the same elements; but that these elements would have come about in a different order. This different order has consequences for the relationships between and among its elements: for what came to be considered primary and what secondary, what mainstream and what marginal, what consensual and what controversial. Our Weldonian curriculum, and the experimental project elaborated around it, expresses that same willingness to contemplate a counterfactual past whose relevance, and maybe also its knowability, lie precisely in not being that different from what actually came to pass.<sup>40</sup>

Taking that possibility seriously may hold the key to extending what we have done to other areas of science and history of science. As a case in point, consider Hasok Chang's

Inventing Temperature (2004) and associated work, including experimental work, on boiling point. Back in the eighteenth century, it was taken for granted that water boils over a range of temperatures, depending on the condition of the water, the container and so on. Thermometers of the day thus represented boiling point as a spread around 100 degrees in Celsius' scale. Nowadays, of course, boiling point is thought of as constant.<sup>41</sup> That shift, from sensitivity about context-dependent variability to indifference to it, has obvious affinities with the shift that exercises me in the history of genetics. Unsurprisingly, I suspect that the best way to characterize the chemical shift is not in terms of a forgetting about variability – as Chang has sometimes been inclined to characterize it, prompting objections from chemists – but rather a shunting of knowledge about it to the margins. Here it lies accessible in principle without in practice being assimilated into everyday chemical thinking and doing. Likewise, I reckon that a perspicuous way to develop Chang's conviction that this shunting had consequences for chemistry and chemists (and that both are the poorer for it) is to develop and teach an experimental chemical curriculum in which boiling point and other chemical phenomena are presented as variable. One could then see whether the students thus taught are interestingly different from students taught the traditional, constancy-emphasizing curriculum. The results may be as interesting for chemistry educators as for historian-philosophers of chemistry. More generally, this kind of endeavour presents an opportunity for a new kind of mutually beneficial collaboration between science educators and historians and philosophers of science.<sup>42</sup>

Inventing Temperature ended with a vision for History and Philosophy of Science (HPS) as 'Complementary Science'. I loved the idea from the first. For the sake of the advance of scientific knowledge, historians and philosophers of science should work in close proximity to scientists, not actually in the lab but right down the corridor. Here, their investigations into

neglected phenomena, debates shut down too soon, and so forth, might serve the cause of creative science by providing just the right measure of provocation.<sup>43</sup> However, if I had to give a programmatic name to our experimental curriculum project, it would be Chang's in reverse: 'Science as Complementary HPS'. In getting our experiment going, what I wanted to find out more about in the first instance was the history of science. How much did the biometrician-Mendelian debate really matter? Was a critical perspective like Weldon's bound to become marginal, whatever the historical circumstances that in fact made it so, because there is some intrinsic flaw in it, rendering it incapable of sustaining successful science? If so, then – recall Macaulay – the particular marginalizing circumstances are of relatively little interest, because, if not for them, there would have been others, and the end of the story would have been the same. But if, on the contrary, and as our experimental results suggest, there is no intrinsic flaw, then there is a far more compelling warrant for concentrating on the particulars of the biometrician/Mendelian moment in explaining why Weldon's perspective got marginalized as and when it did. Add in the apparently better match between twenty-first century biology and a Weldonian curriculum than a Mendelian one, and the mystery of the Mendelian triumph – and the need for a better explanation of it – only increases. Supplying that explanation will be one of the aims of my book in progress.<sup>44</sup>

I gladly fly the flag for HPS as complementary science too, and have been pleased that, through the magic of social media, my counterfactual inquiry into the Weldonian biology that might have been now shares an electronic corridor with Gholson Lyon's lab at Cold Spring Harbor Laboratory. Lyon found out about the Weldonian research a few years ago through a blog posting from just before we launched the Genetics Pedagogies Project, and has been cheering on the research ever since, even attending our end-of-project conference in Leeds. In

Lyon's article from which I quoted earlier, Weldon provides the first epigraph, from the 1902 paper that has the variable-peas photograph:

The fundamental mistake which vitiates all work based upon Mendel's method is the neglect of ancestry, and the attempt to regard the whole effect upon offspring, produced by a particular parent, as due to the existence in the parent of particular structural characters; while the contradictory results obtained by those who have observed the offspring of parents apparently identical in certain characters show clearly enough that not only the parents themselves, but their race, that is their ancestry, must be taken into account before the result of pairing them can be predicted.

Lyon's article closes with a broadside against 'the current paradigm of genetic determinism', cast as stretching back to the time of William Bateson, and castigated for inculcating indifference to variability, ancestry and environment. 'Categorical thinking misses complexity', say Lyon and his co-author, Jason O'Rawe.<sup>45</sup>

So Weldon is emerging for some critically minded scientists in our own day as an ally from across the seas of time. What about Mendel? Some might complain that it is a poor anniversary gift to jettison him from his place of honour in the genetics curriculum. Let me suggest that grumbling along these lines, though understandable, is misguided. If we want to honour Mendel, then let us read him, and read him seriously, which is to say, historically. Indeed, in the service of better readings of Mendel – and by way of a genuinely suitable anniversary present – the British Society for the History of Science will later this year be publishing online, in freely accessible, fully annotated form, a new, scholarly English translation

of Mendel's pea-hybrids paper by Kersten Hall and Staffan Müller-Wille. Read Mendel, but let him be part of his time. Likewise, let our biology students be part of their time, by giving them a genetics curriculum that is fit for purpose in the twenty-first century. If we ask them to read Mendel, we should do so not to fill them with slack-jawed wonder at Mendel's foundational achievement, but to help them appreciate how even the most creative and rigorous science – and Mendel's was first-rate on both counts – shows the stamp of the historical circumstances of its making. To learn that lesson well about past science is to bring a welcome level of self-awareness and critical self-reflection back to the present.

The Mendel anniversary of 2015-16 is among the circumstances stamped all over this address. I end with a tribute to a less conspicuous circumstance: the seaside city of Swansea where the address had its debut. The very possibility of a Weldonian biology was arguably born in Swansea, as it was there, at the annual meeting of the British Association for the Advancement of Science in August 1880, that Weldon – still a student at Cambridge at that moment – first saw Galton in action.<sup>46</sup> The meeting did not go down in the annals of the BAAS as one of their great ones, as it was a little under-attended. But Galton was everywhere. He described some new mathematical instruments he had invented. He reported the work of a committee which he had overseen, charged with gathering and analysing anthropometric data. And he gave an evening discourse during which he talked about his then-new technique of composite portraiture, whereby he took pictures of individuals and merged them in order to discover what kind of a type lay behind them.<sup>47</sup> Would Weldon have been as eager to read Galton's *Natural Inheritance* when it came out in spring 1889 had Galton not made the impression he did at Swansea? *Inheritance* wasn't what Weldon was working on, nor was his research up to that point even the slightest bit mathematical, let alone statistical. But he read it, was excited by it, and sought not

only to carry out research in its image but to correspond with Galton about it. The rest is history, actual and, now, counterfactual too.

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† Presented in modified form at the 2015 British Society for the History of Science Annual Conference, Swansea University, 4 July 2015.

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I am deeply grateful to the BSHS for the privilege of serving as its President and for the opportunity to deliver this address. Many thanks as well to Charlotte Sleigh and colleagues and students in the Leeds HPS Centre for improving comments on a draft of the written version.

<sup>1</sup> Gregor Mendel, ‘Versuche über Pflanzen-Hybriden’, *Verhandlungen des naturforschenden Vereines in Brünn* (1866) 4, second part (Abhandlungen), pp. 3–47; in English translation in, e.g., Curt Stern and Eva R. Sherwood (eds.), *The Origin of Genetics: A Mendel Source Book*, San Francisco and London: W. H. Freeman, 1966, pp. 1–48.

<sup>2</sup> Robert Olby’s writings on Mendel, the rediscovery and the aftermath remain the best entry points into the relevant scholarship. See esp. his *Origins of Mendelism*, 2<sup>nd</sup> edition, Chicago and London: University of Chicago Press, 1985 and ‘The dimensions of scientific controversy: The Biometric-Mendelian Debate’, *British Journal for the History of Science* 22 (1989), pp. 299–320.

<sup>3</sup> Robert Olby, ‘William Bateson’s introduction of Mendelism to England: A reassessment’, *British Journal for the History of Science* 20 (1987), pp. 399–420; Alan G. Cock and Donald R. Forsdyke, *Treasure Your Exceptions: The Science and Life of William Bateson*, Dordrecht: Springer, 2008, pp. 248–250 for Bateson’s use of ‘genetics’ in a letter in 1905 and in print in 1906.

<sup>4</sup> W. F. R. Weldon, ‘Mendel’s laws of alternative inheritance in peas’, *Biometrika* (1901–2) 1, pp. 228–254, photograph in the first insert between 254 and 255; Gregory Radick, ‘Beyond the

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“Mendel-Fisher controversy”: Worries about fraudulent data should give way to broader critiques of Mendel’s legacy’, *Science* (9 Oct. 2015) 350, pp. 159–160.

<sup>5</sup> Here and in the next two points I present schematically what Weldon treated expansively in his writings and correspondence. Among the former, the most important sources include Weldon, *op. cit.* (4); ‘On the ambiguity of Mendel’s categories’, *Biometrika* (1902-3) 2, pp. 44–55; *Theory of Inheritance*, 1904–5, Pearson/5/2/10/4, Pearson Papers, Special Collections, University College London; and ‘Inheritance in animals and plants’, in T. B. Strong (ed.), *Lectures on the Method of Science*, Oxford: Clarendon Press, 1906, pp. 81–109, pp. 92–93. Selections from Weldon’s correspondence can be found in Karl Pearson, ‘Walter Frank Raphael Weldon. 1860–1906’, *Biometrika* (1906) 5, pp. 1–52.

<sup>6</sup> Galton’s take on ancestral influence and its regularities came to fresh prominence in the late 1890s with his framing of what became known as the ‘law of ancestral heredity’. For discussion see William B. Provine, *The Origins of Theoretical Population Genetics*, Chicago: University of Chicago Press, 2001 (first published 1971), pp. 51–54, 179–187. (Cf. the treatment of Weldon on peas here with that in Provine’s book, p. 70.)

<sup>7</sup> Weldon, *Theory of Inheritance*, *op. cit.* (5). For discussion, see Annie Jamieson and Gregory Radick, ‘Putting Mendel in his place: How curriculum reform in genetics and counterfactual history of science can work together’, in Kostas Kampourakis (ed.), *The Philosophy of Biology: A Companion for Educators*, Dordrecht: Springer, 2013, pp. 577–595, esp. 588–589.

<sup>8</sup> In the archive at UCL there is a draft of a chapter from Weldon’s *Theory of Inheritance* manuscript studded with the Edwardian equivalents of post-it notes, on which Galton had scribbled minor amendments to Weldon’s summary of Galton on inheritance (in comparison and contrast with Mendel); Pearson/5/2/9/13, Pearson Papers.

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<sup>9</sup> Jamieson and Radick, *op. cit.* (7), pp. 586–587; also Gregory Radick, ‘Physics in the Galtonian sciences of heredity’, *Studies in History and Philosophy of Biological and Biomedical Sciences* (2011) 42, pp. 129–138, esp. pp. 132–134.

<sup>10</sup> On the controversy, the most extensive study since Olby, ‘Dimensions of scientific controversy’, *op. cit.* (2), is Kyung-Man Kim, *Explaining Scientific Consensus: The Case of Mendelian Genetics*, New York and London: Guilford Press, 1994.

<sup>11</sup> Cock and Forsdyke, *op. cit.* (3), pp. 561–564; for discussion, see Sarah Wilmot, ‘Mendel and the culture of commemoration’, John Innes Historical Collections website (3 March 2015), <http://collections.jic.ac.uk/mendel-and-the-culture-of-commemoration/>.

<sup>12</sup> See, e.g., Jeffrey M. Skopek, ‘Principles, exemplars, and uses of history in early 20<sup>th</sup> Century Genetics’, *Studies in History and Philosophy of Biological and Biomedical Sciences* (2011), 42, pp. 210–225; Gregory Radick, ‘Scientific inheritance: How history matters for the sciences’, inaugural lecture, University of Leeds, 16 May 2012, <https://www.youtube.com/watch?v=D3nyB2lqmRo> (very ably summarized by Rebekah Higgitt, ‘Beyond our Kuhnian inheritance’, *Guardian Online* (28 August 2012), <https://www.theguardian.com/science/the-h-word/2012/aug/28/thomas-kuhn>); Jamieson and Radick, *op. cit.* (7), pp. 583–584, 592.

<sup>13</sup> Richard J. Evans, *Altered Pasts: Counterfactuals in History*, London: Little Brown, 2014; ‘“What if” is a waste of time’, *Guardian Online* (13 March 2013), <http://www.theguardian.com/books/2014/mar/13/counterfactual-history-what-if-waste-of-time>.

<sup>14</sup> Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*, Cambridge: Cambridge University Press, 1983.

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<sup>15</sup> Niall Ferguson (ed.), *Virtual History: Alternatives and Counterfactuals*, New York: Basic Books, 1999 (first published 1997); Stephen Jay Gould, *Wonderful Life: The Burgess Shale and the Nature of History*, London: Hutchinson Radius, 1989. On the fictionalizing differences between experimental interventions, computer simulations and thought experiments as matters of degree, see Gregory Radick, ‘Why what if?’, *Isis* (2008) 99, pp. 547–551, p. 551.

<sup>16</sup> On historical explanation as counterfactual, see, e.g., Geoffrey Hawthorn, *Plausible Worlds: Possibility and Understanding in History and the Social Sciences*, Cambridge: Cambridge University Press, 1995; Jon Elster, *Logic and Society: Contradictions and Possible Worlds*, Chichester: John Wiley, 1978, pp. 175–221.

<sup>17</sup> Weldon, ‘Inheritance in animals and plants’, *op. cit.* (5), pp. 92–93.

<sup>18</sup> James T. Cushing, *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony*, Chicago and London: University of Chicago Press, 1994; Peter Bowler, *Darwin Deleted: Imagining a World Without Darwin*, Chicago and London: University of Chicago Press, 2013. For a vigorously critical discussion of Bowler’s book, see ‘What-if history of science’, *Metascience* (2015) 24: 5–24; for a playfully counterfactual complement to the book, see Stephen Jay Gould, ‘George Canning’s Left Buttock and the Origin of Species’, in his *Bully for Brontosaurus: Reflections in Natural History*, New York: W. W. Norton, 1991, pp. 21–31.

<sup>19</sup> Richard J. Evans, ‘In search of German Social Darwinism’, in his *Rereading German History: From Unification to Reunification, 1800-1996*, London: Routledge, 1997, pp. 119–148, p. 137. I learned from Ian Hesketh after my lecture that Adam Shapiro, in a 2014 blog posting, had pointed out Evans’ contradictions on this front; see Adam Shapiro, ‘What if counterfactuals were acceptable in history?’, 17 March 2014, <https://tryingbiology.wordpress.com/2014/03/17/what-if-counterfactuals-were-acceptable-in-history/>.

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<sup>20</sup> Léna Soler, Emiliano Trizio, and Andrew Pickering (eds.), *Science as it Could Have Been: Discussing the Contingency/Inevitability Problem*, Pittsburgh: University of Pittsburgh Press, 2015; Ian Hacking, *The Social Construction of What?*, Cambridge, MA: Harvard, 1999, esp. pp. 78–80.

<sup>21</sup> T. Babington Macaulay, ‘Dryden’ (1828), in his *Essays, Critical and Miscellaneous*, Philadelphia: Hart, Carey & Hart, 1850, pp. 35–50, pp. 35–36; for discussion, see Robert K. Merton, ‘Singletons and multiples in science’ (1961), in his *The Sociology of Science: Theoretical and Empirical Investigations*, Chicago and London: University of Chicago Press, 1973, pp. 343–370, p. 353.

<sup>22</sup> On Clarkson’s honorary degree, <http://www.brunel.ac.uk/about/people/honorary-graduates/honorary-graduates-2003/jeremy-charles-robert-clarkson>.

<sup>23</sup> For a superb analysis of the anti-individualism that is common to both an older positivist (inevitabilist) historiography of science and the more recent contextualist (contingentist) historiography, see John Henry, ‘Ideology, inevitability, and the Scientific Revolution’, *Isis* 99 (2008), pp. 552–559.

<sup>24</sup> Alfred Kroeber, ‘The superorganic’, *American Anthropologist* (1917) n.s. 19, pp. 163–213, p. 199. Cf. ‘Mendel spoke the truth, but he was not dans le vrai (within the true) of contemporary biological discourse.... Mendel was a true monster, so much so that science could not even properly speak of him.’ Michel Foucault, ‘The discourse on language’ (1970), in *The Archaeology of Knowledge and the Discourse on Language*, trans. A. M. Sheridan Smith, New York: Pantheon, 1972, pp. 215–237, p. 224.

<sup>25</sup> Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, Princeton: Princeton University Press, 1985, p. 13.

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<sup>26</sup> On the epistemology of counterfactuals considered philosophically, psychologically and historically, see respectively, e.g., Timothy Williamson, ‘Knowledge of counterfactuals’, *Royal Institute of Philosophy Supplement* (2009) 64, pp. 45–64; David R. Mandel, Denis J. Hilton and Patrizia Catellani, eds., *The Psychology of Counterfactual Thinking*, Oxford: Routledge, 1995); Martin Bunzl, ‘Counterfactual history: A user’s guide’, *American Historical Review* (2004) 109, pp. 845–858.

<sup>27</sup> Anyone undecided or unpersuaded should consult Daniel Nolan, ‘Why historians (and everyone else) should care about counterfactuals’, *Philosophical Studies* (2013) 163: 317–335.

<sup>28</sup> The abstract puzzles I have confronted most squarely are to do with the bearing of realism/antirealism questions, the theory-ladenness of observations, and the individuation of theories (i.e. how different do they need to be to count as different?); see Gregory Radick, ‘Other Histories, Other Biologies’, in Anthony O’Hear (ed.), *Philosophy, Biology and Life*, Cambridge: Cambridge University Press, 2005, pp. 21–47. Other, less systematic attempts of mine to bring epistemological difficulties into focus include ‘Is the theory of natural selection independent of its history?’, in Jonathan Hodge and Gregory Radick (eds.), *The Cambridge Companion to Darwin*, Cambridge: Cambridge University Press, 2003, pp. 143–167; ‘The case for virtual history’, *New Scientist* (20 August 2005) 187: 34–35; *The Simian Tongue: The Long Debate about Animal Language*, Chicago: University of Chicago Press, 2007, esp. pp. 365–368; and *op. cit.* (15) – the introduction to an edited Focus section on ‘Counterfactuals and the historian of science’, *Isis* (2008) 99: 547–584.

<sup>29</sup> Weldon, *op. cit.* (5), ch. 5, p. 24, Jamieson transcription.

<sup>30</sup> Theodore M. Porter, *Karl Pearson: The Scientific Life in a Statistical Age*, Princeton: Princeton University Press, 2004, p. 302.

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<sup>31</sup> Quoted in Beatrice Bateson (ed.), William Bateson, F.R.S., *Naturalist[:] His Essays & Addresses, Together with a Short Account of His Life*, Cambridge: Cambridge University Press, 1928 [and facsimile reprint, New York: Garland, 1984], p. vi, with the italicization there kept.

<sup>32</sup> On the Resilience Project, see Stephen H. Friend and Eric E. Schadt, ‘Clues from the resilient’, *Science* (30 May 2014), pp. 970–972, and more generally <http://resilienceproject.me/>.

<sup>33</sup> Gholson J. Lyon and Jason O’ Rawe, ‘Human genetics and clinical aspects of neurodevelopmental disorders’, in Kevin J. Mitchell (ed.), *The Genetics of Neurodevelopmental Disorders*, Hoboken: Wiley–Blackwell, pp. 289–317, pp. 291–292.

<sup>34</sup> Evelyn Fox Keller, *The Century of the Gene*, Cambridge, MA: Harvard University Press, 2000; Jenny Lewis, ‘Genetics and genomics’, in Michael Reiss (ed.), *Teaching secondary biology*, 2<sup>nd</sup> edition, London: Hodder Education, 2011, pp. 173–214, p. 175.

<sup>35</sup> For a more detailed account of the project and its findings, see Annie Jamieson and Gregory Radick, ‘Genetic determinism in the genetics curriculum: An experimental test of the effects of Mendelian and Weldonian emphases’ (submitted). The project’s sine qua non was a generous research grant from the Uses and Abuses of Biology scheme of the Faraday Institute for Science and Religion, Cambridge University.

<sup>36</sup> [http://www.leeds.ac.uk/arts/homepage/399/the\\_genetics\\_pedagogies\\_project](http://www.leeds.ac.uk/arts/homepage/399/the_genetics_pedagogies_project).

<sup>37</sup> Helpful pointers in the right direction from within recent philosophy of science include Kim Sterelny, ‘Another view of life’, *Studies in History and Philosophy of Biological and Biomedical Sciences* (2005) 36, pp. 585–593; Joseph D. Martin, ‘Is the contingentist/inevitabilist debate a matter of degree?’, *Philosophy of Science* (2013) 80, pp. 919–930; and Katherina Kinzel, ‘State of the field: Are the results of science contingent or inevitable?’, *Studies in History and Philosophy of Science* (2015) 52, pp. 55–66. On the way that opposing sides in a scientific

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debate will converge, with the ‘winner’ looking much more ‘loser’-ish by the end than at the start, a philosophical classic remains instructive: William Whewell, ‘On the transformation of hypotheses in the history of science’, in his *On the Philosophy of Discovery; Chapters Historical and Critical*, London: J. W. Parker, 1860, pp. 492-503. Many thanks to Jon Hodge for this reference.

<sup>38</sup> R. J. Redfield, ‘Why do we have to learn this stuff?’—A new genetics for 21st century students.’ *PLoS Biology* (2012) 10, pp. 1–4. For further discussion, see Jamieson and Radick, *op. cit.* (35).

<sup>39</sup> Commenting on a draft of this paper, a Leeds colleague wrote: ‘I wondered whether there is a difference in determinism between introductory university genetics and more advanced. When I learnt genetics in the late ‘70s, I understood plasticity of genes, and all the Weldonian stuff, although I didn’t see it as anti-Mendelian (and I loved Mendelian genetics, and particularly ratios).’ A great deal of what I want to find out about is touched upon here. What is the link between Mendelism’s teachability and its persistence? How, exactly, shall we characterize that persistence? And what difference would it make if students got the Weldonian stuff earlier in their learning?

<sup>40</sup> Peter J. Bowler, ‘What Darwin disturbed: The biology that might have been’, *Isis* (2008) 99, pp. 560-567.

<sup>41</sup> Hasok Chang, *Inventing Temperature: Measurement and Scientific Progress*, Oxford: Oxford University Press, 2004.

<sup>42</sup> For additional remarks on how this sort of enterprise might enrich exchange between science education and the history of science, see Jamieson and Radick, *op. cit.* (35). For future development of the notion of cognitive centrality versus marginality in science, a promising

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resource is Ronald Giere's perspectival philosophy of science; see Ronald Giere, *Scientific Perspectivism*, Chicago: University of Chicago Press, 2006.

<sup>43</sup> Chang, *op. cit.* (41), pp. 235-250.

<sup>44</sup> See, in the interim, Gregory Radick, 'Should 'heredity' and 'inheritance' be biological terms? William Bateson's change of mind as a historical and philosophical problem', *Philosophy of Science* (2012) 79, pp. 714–724; and the three papers authored or co-authored by me in *Owning and Disowning Invention: Intellectual Property and Identity in the Technosciences in Britain, 1870–1930*, eds. Christine MacLeod and Gregory Radick, published as a special issue of *Studies in History and Philosophy of Science* (2013) 44, pp. 188–300.

<sup>45</sup> Lyon and O'Rawe, *op. cit.* (33), pp. 289 and 303. For Lyon's talk at the 'Nurturing Genetics' symposium in Leeds in June 2014, see [http://www.leeds.ac.uk/arts/info/125175/genetics\\_pedagogies\\_project/2094/events](http://www.leeds.ac.uk/arts/info/125175/genetics_pedagogies_project/2094/events).

<sup>46</sup> Pearson, *op. cit.* (5), p. 8.

<sup>47</sup> 'The British Association', *Nature* (2 September 1880) 22, pp. 410-411; *Rep. BAAS* 50, pp. lxxvii, 459, 603-605, 625.