**Essay review**

**Counterfactuals, causes and contingency in the history of science**

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**Darwin Deleted**

Peter Bowler, University of Chicago Press: Chicago, IL, 2013, pp. 318, Price $30.00

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**Science as It Could Have Been: Discussing the Contingency/ Inevitability Problem**

Lena Soler, Emiliano Trizio and Andrew Pickering, University of Pittsburgh Press:

Pittsburgh, PA, 2015, pp. 472, Price $61.95 hardcover, ISBN: 9780822944454

Suppose that Charles Darwin had not written *On the Origin of Species*. Suppose that he had not been there to write it because on the voyage with the *Beagle*, he had gone overboard in a storm and drowned. In this scenario, would the history of evolutionary theory have looked anything like the actual history that we know? Would the concept of evolution by natural selection have emerged nevertheless? Or would thinking about evolution have taken a radically different path? Is the history of science contingent?

In *Darwin Deleted,* historian of science Peter Bowler sets out to answer these questions. He presents a counterfactual narrative of evolutionism from the mid-nineteenth to the early twentieth century, identifying Darwin as one of the “nodal points” in history at which a seemingly minor change of events could have set off an alternative trajectory. He describes the landscape of evolutionary thinking that would have emerged without Darwin, explores what shape the conflict between science and religion would have taken, and reflects on the relations between evolutionary thinking and the ideologies of social Darwinism, racism and the eugenics movement in both the actual and the counterfactual world. Bowler’s narrative depicts both contingent and inevitable developments. It identifies those areas of scientific discourse where Darwin’s absence would have made a difference, and those where it would have not.

But Bowler’s understanding of the concepts of “contingency” and “inevitability” is slightly different from what most philosophers of science seem to have in mind when using these terms. *Science as it could have been* – edited by Léna Soler, Emiliano Trizio and Andrew Pickering – is the most comprehensive publication on the problem of contingency in science to date, and as such, it serves well to gauge philosophical opinions on the matter. The volume seeks not only to provide an overview of the current state of the debate, but also to clarify the central concepts and arguments involved.

In what follows, I bring together the philosophical discussion about contingency, and the counterfactual approach taken by Bowler. First, I describe the “comparative approach” to contingency taken by many philosophers of science. On this basis, I highlight differences to Bowler’s historical concerns. I then show that Bowler, as well as some contributors to *Science as it could have been*, think of contingency and inevitability not in comparative, but in causal terms. I indicate how the “comparative” and the “causal approach” can be brought together and conclude with some reflections about the strengths and weaknesses of counterfactual histories à la Bowler.

*The comparative approach*

*Science as it could have been* assembles a plurality of philosophical perspectives and voices on the question of contingency in science. The contributions investigate the problem as it arises in various scientific disciplines, among them mathematics, physics, geology, and psychology. They situate the contingency issue with respect to related and often better-known philosophical debates and topics, such as scientific realism, social constructivism, scientific pluralism, experimental practices, natural laws, and ontology. They also offer sometimes diverging attempts to clarify the main concepts and positions. The plurality of approaches assembled in the book makes it somewhat difficult to find common ground between them. In this review, I focus on contributions that resonate well with Bowler’s work, either because of stark differences from, or similarities to, his approach.

I begin with two contributions that provide a particularly clear understanding of what is at stake when philosophers of science argue about contingency. Léna Soler’s dialogical reconstruction of “contingentist” and “inevitabilist” arguments (Chapter 1), and Emiliano Trizio’s reflections on “contingentism” and scientific realism (Chapter 4). Both take as their starting point, Ian Hacking, who had described the question to which contingentists and inevitabilists give conflicting answers in the following way:

If the results R of a scientific investigation are correct, would any investigation of roughly the same subject matter, if successful, at least implicitly contain or imply the same results? (Hacking, 2000, 61).

While the inevitabilist gives a positive answer, the contingentist holds that “there could be alternative non-equivalent but equally successful sciences” that yield results different from those of actual science (Hacking, 2000, 64). This way of delineating the positions at stake involves an act of imaginative comparison. I therefore call it the “comparative approach” to contingency. The basic move is to imagine a radically different alternative science and to compare it to our actual science. The contingentist, but not the inevitabilist, is willing to imagine this alternative science performing as successfully as our own.

Soler clearly thinks in comparative terms when analysing how the dialogical dynamics that unfold in the conflict between contingentists and inevitabilists drive them towards reflections about the relative merits of alternative sciences in their long-term development. She draws on Pickering’s *Constructing Quarks* (1984) and Cushing’s *Quantum Mechanics* (1994) to give a face to contingentist argumentative strategies. She finds Cushing’s analysis of the history of the two empirically equivalent interpretations of quantum mechanics – the indeterministic Copenhagen interpretation and the causal, deterministic Bohm view – particularly illuminating. Not only does Cushing deal with actually existing rivals, rather than past or mere counterfactual alternatives, he also provides a clear explication of the comparative concepts of “incompatible alternative” and “equal success”: the rivals are incompatible as far as ontological commitments are involved but as empirical equivalents they enjoy equal predictive success (pp. 68-69).

Soler observes that inevitabilists can challenge a contingentist claims based on Cushing’s narrative by denying that the Copenhagen interpretation and Bohm’s theory are genuine alternatives, treating them instead as different formulations of the same theory (pp. 71-72). In another contribution (Chapter 14), we come across a contingentist rejoinder: Jean-Marc Lévy-Leblond argues that even different theory formulations come with distinct conceptual and practical implications, and should hence be considered genuine alternatives. As Jean Paul Van Bendegem points out in Chapter 9, the general challenge for contingentism is as follows. There needs to be enough difference between two sciences or theories – his contribution deals with mathematics – for them to count as genuine alternatives. But the differences must not be so strong that the alternatives become incomparable (p. 227). The “comparative approach” involves an understanding of theory-individuation, as well as criteria of scientific success that allow for an evaluative comparison between alternatives.

The “comparative approach” also builds the background to Trizio’s analysis of the relations between contingentism and scientific realism, and interestingly, Trizio draws on the same case studies as Soler does. He argues that inevitabilism can come in both realist and anti-realist forms. But while Gregory Radick had argued that realist-contingentist approaches to the history of science are conceivable as well (Radick, 2005, 23-25), according to Trizio, an elaborate version of preservative scientific realism is incompatible with contingentism (p. 142). In particular, the realist cannot allow the possibility of incompatible yet equally successful alternatives to those theoretical constituents that she is confident are (approximately) true and will be preserved in future successful science. The specific challenge that arises from contingentism is that the alternatives in question are thought to be not just logically, but also historically possible (p. 148).

In seeming conflict with this assessment, other contributions to *Science as it could have been* do allow realism and contingency to go together. But they usually moderate on contingentism, or on realism, or on both, for example when Ronald Giere seeks to make a “conditional realism” compatible with “reasonable contingency” (Chapter 7, p.188). Pickering’s and Cushing’s case studies are usually taken to entail much more radical forms of contingentism.

Talk about “reasonable” vs “radical" contingency claims raises the question of how to distinguish between contingentist views of different strengths, and how “strong” contingentism needs to be for there to be interesting debate. Harry Collins argues that we should reduce contingentism to the empirical claim that scientific communities have in fact entertained incompatible beliefs over short periods of time (Chapter 6). But for Soler, a controversial form of contingentism is more demanding. It requires not only the possibility of an alternative science that is incompatible yet equally valuable as ours, but also that the alternative science retains these features over the long haul (pp. 79-80).

With a rough overview over some of the issues that emerge in the context of the “comparative approach”, we are now in a better position to highlight differences from Bowler.

*A world without Darwin*

*Darwin Deleted* lays out in great detail what an alternative history of science without Darwin would have looked like. Bowler argues that there would have been an evolutionary movement even without Darwin, but it would not have involved natural selection. In the absence of Darwin, non-selectionist alternatives – most importantly orthogenesis and Lamarckism – would have filled the gap. The transition to evolutionary thinking would have been less harsh, because Lamarckian progressivism and the developmentalist emphasis on inherent tendencies were less provocative from a theological perspective than natural selection’s randomness. The main line of conflict would have run between developmentalism and adaptationism, with each receiving some degree of evidential support from developments in morphology, palaeontology and biogeography. With the advent of genetics, Lamarckianism would have come under pressure, prompting a search for alternative mechanisms of adaptation. In the early twentieth century, natural selection would have been discovered eventually, giving rise to a set of theories similar to the ones accepted today. What is the philosophical significance of this counterfactual story? Interestingly, Bowler’s historical narrative is not particularly useful in the context of the “comparative approach”.

First, his description of the alternate universe does not involve an account of theory-individuation. For one thing, Bowler does not present evolution by natural selection as strictly incompatible with its alternatives. He notes that Darwin allowed some space for the operation of Lamarckian mechanisms alongside natural selection. And he shows that pronounced “Darwinists” like Herbert Spencer and Ernst Haeckel drew significantly on Lamarckian and developmentalist ideas (pp. 130-133, 156-157, 222-223). His analysis suggests that what is compatible and what is not is not a logically predetermined feature, but subject to how theories are conceptualized and articulated in concrete contexts. Moreover, Bowler does not claim that the alternatives would have been equally successful as natural selection. There would have been differences regarding the timing and mode of theory acceptance, but it is not clear whether this would have made the alternatives more, less or equally successful as actual science, and according to which standard. Bowler is simply not interested in an evaluative comparison between the actual and the alternative science.

Bowler’s narrative is also mute on the question of realism. While in the introductory chapter, Bowler claims that his counterfactual history creates problems for realism, this claim is not elaborated in great detail (pp. 12-13). The real opponents targeted in the book are not scientific realists, but rather historical determinists, religious critics of evolutionary theory, and critics of counterfactual history. Bowler devotes a whole chapter to dismantling the determinist idea that natural selection was “in the air” in the mid nineteenth century and would have been discovered and established without Darwin’s efforts, possibly by Alfred Russell Wallace (Chapter 2). Another chapter takes issue with the idea that the evils of social Darwinism and eugenics arose directly from the theory of natural selection (Chapter 8). The whole book is a reply to the sceptic who takes counterfactual history to be nothing but idle speculation. Bowler engages with these opponents in great length. In comparison, he does not identify the exact points of conflict between his contingentist narrative and scientific realism. Ultimately, I doubt that an elaborate form of scientific realism is threatened by anything Bowler has to say. His real interests and concerns are historical. They have little to do with the philosophical question of whether alternative equally successful sciences are possible.

Finally, it is not clear whether Bowler’s reconstruction should be considered “contingentist” in the sense defined above. After all, Bowler claims that even in the counterfactual world, natural selection would have emerged eventually. In the wake of the rediscovery of Mandelian genetics, scientists would have looked for alternative explanations of adaptation, and natural selection would have been discovered, possibly in the collaboration of Karl Pearson and W.F.R. Weldon (pp. 195-198). This claim is perhaps the least convincing part of Bowler’s book and I will return to this problem below. But taking his narrative at face value, should we classify it as “contingentist” or as “inevitabilist”? Apparently, Bowler believes that the historical development of evolutionism had both contingent and inevitable dimensions. But what do the terms “contingent” and “inevitable” mean then? Obviously, something different is at stake than whether an “incompatible” yet “equally valuable” science is possible or not.

*Contingency as a causal concept*

In order to get a grip on this alternative understanding of contingency, I place Bowler’s narrative in the company of three contributions to *Science as it could have been* which, like Bowler, combine contingentist and inevitabilist perspectives.

First, there is Yves Gingras’ reconstruction of the discovery of the wave properties of the electron in the mid 1920s (Chapter 8). According to Gingras, it was contingent that Clinton Davisson was the first to make the discovery of electron diffraction. Accidental circumstances – such as a laboratory accident and a visit to England, where he learned of de Broglies hypothesis of matter waves – reoriented his research activities and put him in the position to make the discovery. However, the contingent discovery had inevitable consequences. Once the wave properties of electrons were known, physicists’ training in the dominant paradigm ensured that they would deduce that electrons are subject to reflection, refraction and polarization and that they would test these claims experimentally.

A different combination of contingent and inevitable developments is found in Giere’s account of “mobilism” – the view that the distribution of land masses on the earth’s surface underwent massive changes (Chapter 6). According to Giere, the acceptance of mobilism depended on the convergence of different research trajectories. While the discovery of natural radioactivity around 1900 had already allowed for an explanation of how land masses could be torn apart by convection currents, there was no clear evidence for this idea. Only in the 1950s and 60s, when Cold War submarine operations created the incentive of charting the ocean floor, were ocean floor ridges discovered. At the same time, scientists were finding evidence for the periodic reversal of the Earth’s magnetic poles. Mobilism became undeniable when these different strands came together in the 1960s. Mobilism offered the only plausible explanation for the pattern of magnetic bands that run parallel to the ocean floor ridges. While the acceptance of mobilism was contingent before 1960, it was inevitable afterwards.

Michel Bitbol’s and Claire Petitmengin’s account of the fate of introspection in psychology provides yet another picture of how the contingent and the inevitable aspects of science go together (Chapter 12). The authors claim that broader background trends in epistemology made it inevitable that introspection would disappear from twentieth century psychological research. The prevalence of a correspondence theory of truth and a naïve understanding of objectivity, the dominance of representationalists views of cognition, scientist’s limited ideas about neurophysiological research and their lack of understanding of non-behavioral therapeutic methods made the research climate unfavourable to introspection. For Bitbol and Petitmengin, the eclipse of introspective psychology is both contingent and inevitable: it is contingent, insofar as these broader background trends could have been different in principle, but given that they were not different, it was inevitable.

There are similarities between these contributions and Bowler’s narrative. Like Gingras, Bowler focuses on a discovery made by a particular individual and highlights how a specific combination of research interests and contingent events put this individual in the unique position of making the discovery. Like Giere, Bowler studies the long-term trajectories of various research fields and sources of evidence, explains their relations to one another, and traces their sometimes diverging, sometimes converging trajectories. And like Bitbol and Petitmengin, he identifies broader background trends that impact the scientific developments under consideration. All four authors use the terms “contingency” and “inevitability” in a similar manner. They use it as causal vocabulary: the terms express claims about the dependencies between different factors and events, they identify nodal points in historical developments, as well as moments of converge, and they discriminate between different stages in the causal determination of a historical process.

The causal meaning of the terms “contingency” and “inevitability” is evident when we take a closer look at *Darwin Deleted*. When Bowler claims that the discovery of natural selection was contingent, it entails a claim about the local character of the relevant causes. In the mid-nineteenth century, only a very specific set of concerns allowed for assembling and interpreting the available evidence in the right way, and Darwin was the only one who had this unique set of concerns. When Bowler states that evolutionism would have emerged inevitably, this is an implicit verdict on Lamarckian transmutationism having the causal capacity to win over scientists even in the absence of Darwin’s forceful arguments for evolution. When he argues that the conflict between science and religion was contingent, it is a claim about the causal relevance of *The Origin* in the articulation of a radical naturalism threatening to theology. And when he judges that the emergence of the eugenics movement was inevitable, this means that broader causal factors fueled racism and eugenics, and that these factors were independent of the scientific theories in question.

Understood as causal vocabulary, contingency and inevitability can go together. Inevitable causal sequences can exist alongside or surrounded by broader contingent developments, contingent conjunctures can have inevitable consequences, and it is even possible for one and the same event to appear as contingent or inevitable depending on the perspective chosen. One and the same event can be contingent or inevitable depending on the time-frame. Focusing on a time when its determining conditions were not yet in place or had not yet converged, it appears contingent. But if we focus on a later time when the relevant antecedent factors are all in place, it appears inevitable. One and the same event can also be contingent or inevitable depending on whether our perspective is factualist or counterfactualist. It appears inevitable if we focus on the processes that have in fact caused it, but it seems contingent, as soon as we alter one or many of the determining factors in our imagination. Finally, one and the same event can be claimed to be contingent or inevitable depending on the type of information provided. If the causal information provided makes no significant difference to the outcome of the process under investigation, the outcome is inevitable with respect to that piece of information. But it may still be contingent with respect to other factors.

The “causal understanding” of contingency and inevitability is very different from the “comparative approach”, and it makes room for “compatibilist” positions. But given the differences between the two approaches, one may wonder whether the two have anything to do with each other. Can they be integrated in a common framework?

*Plausible counterfactual histories*

Elsewhere, I have argued that each contingency and each inevitability claim contains answers to the following four questions (Kinzel, 2015):

(1) Are alternatives to current science possible and how, e.g. logically or historically?

(2) What types of alternatives are we talking about: alternative methods, practices, theories, or paradigms?

(3) How should the alternatives be assessed in comparison to actual science? Are they equally successful, equally rational, successful or rational according to a different standard?

(4) How are they different from actual science? For instance are they incompatible, incommensurable, or equivalent?

One way of understanding the difference between the “comparative” and the “causal approach” is that the former tries to answer questions (2)-(4), while the latter deals with question (1), in particular with the problem of historical possibility. The “causal approach” contributes to an analysis of what is and what is not historically possible in a given context, at a given time by

1. elucidating causal dependencies
2. identifying the stages of development and/or the nodal points at which things could (still) have gone differently, and
3. by tracing the convergences of different trajectories.

The “comparative” and the “causal approach” therefore need not be in conflict. Even if they answer to different questions, they can be combined to contribute to an understanding of contingency in science more broadly.

I want to conclude with some reflections about the possibility of constructing plausible counterfactual histories in general, and the plausibility of Bowler’s narrative in particular. After all, counterfactual history is often greeted with scepticism. The general worry is that it is empirically uncontrollable. As Robert Richards put it: “in a complex matrix of interacting events, the conceptual addition or elimination of a significant cause must have unpredictable consequences.” (Richards, 2015, 13) Keeping this problem in mind, how can plausible counterfactual scenarios be construed and empirically supported?

The problem of counterfactual historiography of science has recently been addressed in a special issue of *Isis* (Radick, 2008). My own discussion is loosely inspired by Jon Elster’s theory of historical counterfactuals (Elster, 1978). Elster’s analysis placed a strong emphasis on the causal theories that underwrite the construction and assessment of historical counterfactuals – with both the term “causal” and the term “theory” being understood rather broadly. Elster’s main idea was that when constructing a counterfactual scenario, we always rely on an understanding of the causal connections that hold in the real world. This perspective resonates nicely with the above analysis of contingency and inevitability as causal concepts. It also helps to specify the conditions that plausible counterfactual histories have to fulfil.

A first condition is that the change made to the historical universe needs to be consistent with the broader historical context. A whole range of background conditions need to remain unaffected by, and consistent with, the introduction of the counterfactual antecedent. Second, the change in antecedent needs to have determinate consequences for specified aspects of the historical universe. Put in Elster’s terms, in order to connect the counterfactual antecedent with the consequent(s), we need a theory of the causal factors operative in the historical context at hand. This theory will specify which events are altered by the change in the antecedent, which remain unaffected, and which are overdetermined by other factors. The broader the scope of the theory, and the firmer the causal connections that it postulates, the more warrant does it provide for the counterfactual conclusion. Finally, evidence from our actual history warrants not the counterfactual directly, but rather it supports the causal theory that is the basis for the construction of the counterfactual. Put differently, investigations of the actual history tell us what the relevant causal connections are, and the counterfactual narrative is constructed on the basis of our knowledge of these connections. A plausible counterfactual is based on an empirically well supported causal theory.

 These clarifications help us to better understand the worries about counterfactual history. They indicate three distinct ways in which the enterprise may go wrong: the change in antecedent may be inconsistent with the historical context, the causal theory may be too weak to support the conclusions, and the causal theory may not be sufficiently supported by evidence from actual history. How well does Bowler’s narrative fare with respect to these questions?

 The first requirement causes no problems. The introduction of Darwin’s premature death is perfectly consistent with all significant parts of the historical context remaining stable. However, there is room for debate about the extent to which Bowler’s causal theory is supported by actual events and hence fulfils condition three. When Richards argues that Bowler underestimates the importance of Wallace’s work, while overestimating the potential of Lamarckism to win over scientists, he is not targeting the counterfactual approach as such (Richards, 2015, 14-15). Rather, the charge is that a misinterpretation of the actual history has led Bowler to false causal claims. Similarly, Alan Love’s criticism that Bowler equates science with its resultant theories, at the expense of scientific practices, is not an argument against counterfactual history, but an argument against trying to find the relevant causal connections on the level of theoretical discourse only (Love, 2015, 8).

The question that I find most interesting, however, is whether Bowler’s historical narrative satisfies the second condition. Is the causal theory that he offers strong enough to carry his conclusions? It seems to me that that the prejudice against counterfactual history stems primarily from worries about this question. We tend to have the intuition that the causal connections in history are either too complex, or too indeterminate, or both, for there to be a causal theory that is sufficiently detailed and sufficiently strong to guide the construction of counterfactual scenarios. In this context, it is surprising how far Bowler gets with his analysis. If we accept his understanding of the relevant causes – that is, if we ignore the question of empirical support – his conclusions are quite convincing. Only one part of his story stands out as clearly beyond the limits of what his casual theory can support. This is the claim that natural selection would have been discovered eventually and that it would have been integrated with the alternative evolutionisms creating something similar to contemporary evolutionary biology.

The problem emerges from the difficulty of predicting scientific discoveries. Bowler not only argues that Darwin was unique in bringing the relevant theoretical and evidential resources together, but also says that putting them together in the way that Darwin did required a “distinct act of conceptual innovation” (p. 50). Making a discovery involves more than the mere conjunction of pre-existing elements and therefore, the eventual discovery of natural selection is beyond what Bowler’s causal account can predict. So is the claim that the integration of natural selection with developmentalist stands would have led to the emergence to something similar to our modern views. If the integration of different theories involves “conceptual innovation”, then it simply cannot be predicted in the way that Bowler requires for his counterfactual narrative to go through. Here we are confronted with yet another dimension of the problem of contingency in the history of science. We encounter a form of contingency rooted in the unpredictability of scientific discovery and innovation.

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