State of the field: Are the results of science contingent or inevitable?

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Abstract

This paper presents a survey of the literature on the problem of contingency in science. The survey is structured around three challenges faced by current attempts at understanding the conflict between "contingentist" and "inevitabilist" interpretations of scientific knowledge and practice. First, the *challenge of definition*: it proves hard to define the positions that are at stake in a way that is both conceptually rigorous and does justice to the plethora of views on the issue. Second, the *challenge of distinction*: some features of the debate suggest that the contingency issue may not be sufficiently distinct from other philosophical debates to constitute a genuine, independent philosophical problem. And third, the *challenge of decidability*: it remains unclear whether and how the conflict could be settled on the basis of empirical evidence from the actual history of science. The paper argues that in order to make progress in the present debate, we need to distinguish more systematically between different expressions that claims about contingency and inevitability in science can take. To this end, it introduces a taxonomy of different contingency and inevitability claims. The taxonomy has the structure of an ordered quadruple. Each contingency and each inevitability claim contains an answer to the following four questions: (how) are alternatives to current science possible, what types of alternatives are we talking about, how should the alternatives be assessed, and how different are they from actual science?

Keywords

Contingency, inevitability, counterfactual history, social constructivism, alternative sciences

1. Introduction

Could the historical development of the sciences have led to alternative sciences? Say an alternative biology or physics invested in methods and practices different from those of our science, one that devised concepts and explanations incompatible with our actual scientific theories. Might we have come to accept alternative claims about the workings of nature? Indeed, might we have come to accept theories that furnish the world with different entities and causes than our best-confirmed scientific theories do? And if an alternative science had emerged historically, then could it, although radically different from our actual science, have become as successful in its explanations, predictions and technological applications as the biology and physics we know today? To put it briefly, are the results of successful science contingent?

Contingency is a central issue in the philosophy of general history. It also surfaces in a broad range of other academic fields, for example in sociology, economics, and moral and political philosophy (some examples from moral and political philosophy are Cottingham 2008; Rorty 1989; Williams 2002; examples from economic and military history Cowley 1999; Robert 1964; Pomeranz 2000). Over the last decades, contingency also developed into a central theme in the study of scientific knowledge and practice.[1] Sociologists of scientific knowledge presented historical case studies that show scientific results to be decided upon in contingent social negotiation processes (Collins 1985; Pickering 1984; Pinch 1986; Shapin and Schaffer 1985). At the same time, the advent of microhistory in the history of science pushed "big picture"-narratives into the background. Close attention to the particular, the local and the contingent became a hallmark of good historiographical writing. More recently, ventures in counterfactual history explored possible or plausible alternative scientific trajectories (Bowler 2008; Chang 2012, 43–50; Radick 2005).

In the philosophy of science too, contingency is an issue. Attacks on social constructivism pitted scientific reasoning against social causation, arguing that the proper operation of scientific rationality and methodology renders the outcomes of scientific

[1] There are debates about the historical contingency of science in the early twentieth century European tradition, especially in the respective later writings of Martin Heidegger, Edmund Husserl, and Ludwig Wittgenstein. Contingency is also a central concern in the French tradition of historical epistemology, in particular in the writings of Georges Canguilhem and Michel Foucault. In the Anglo-Saxon context, the problem of contingency in science was put on the agenda in the 1960s and 70s by Thomas Kuhn and Paul Feyerabend. An in-depth discussion of these different contributions is beyond the scope of this paper, which focuses primarily on more recent debates.

debates much less contingent than sociologists believe them to be (Franklin 1990; Franklin 1994; Laudan 1981; Laudan 1990). Scientific pluralists and perspectivalists take a more positive stance towards contingency, claiming that scientific progress can tolerate and perhaps even depends on the availability of multiple alternatives (Chang 2012; Giere 2006; Kellert, Longino, and Waters 2006). And questions of contingency are also present in controversies regarding scientific realism and underdetermination (Cushing 1994; Stanford 2006).

But while contingency is implied in all these different sociological, historical and philosophical traditions, the concepts that are used in these debates often remain vague and intuitive. There exists only a small amount of systematic philosophical work that addresses the issue as an independent matter and that seeks to spell out in a rigorous manner what is at stake in claims concerning the contingency (or inevitability) of scientific processes and results.

"Contingentism" and "inevitabilism" made their first appearance as explicit philosophical positions in Ian Hacking's *The Social Construction of What* (1999, 68–80), with further explication following a year later in a paper that asked: "How Inevitable are the Results of Successful Science?" (2000). The issue has since been explored in more detail in a symposium organized by Léna Soler, published in *History and Philosophy of Science* (Franklin 2008; Sankey 2008; Soler 2008a; Soler 2008b; Trizio 2008), in a focus section of *Isis* dedicated to the role of counterfactuals in the history of science (Bowler 2008; French 2008; Fuller 2008; Henry 2008; Radick 2008) and at a conference titled *Science as it Could Have Been*, held in 2009.[2] We can find some further explicit references to the contingency issue (Kidd 2013; Kidd in press; Martin 2013; Radick 2003; Radick 2005) but in general, systematic and conceptually rigorous literature on the problem is rare. Hence, we are confronted with a remarkable discrepancy between the large amount of sociological, historical and philosophical literature that raises vital questions concerning contingency in science on the one hand, and the small amount of philosophical work that is explicitly devoted to this issue on the other. In this paper, I present a survey of the existing work on contingency and inevitability in science. I structure my discussion around three types of challenge that emerge in the current discussions.

[2] In addition, an edited volume on contingency in science is soon to appear with Pittsburgh University Press (Soler, Trizio, and Pickering in press). Léna Soler has kindly sent me the introduction to the volume, but I have not seen the individual contributions.

The first part of this paper deals with the *challenge of definition*. While "contingentism" and "inevitabilism" are sometimes referred to as if they constituted clearly delineated philosophical positions, many commentators are prepared to accept that there can be different types of contingency and inevitability in science. I show that it proves hard to describe the positions that are involved in a way that is conceptually rigorous while also doing justice to the plethora of existing views on the issue.

The second part analyses the *challenge of distinction*. There exist convincing arguments that the debate on contingency is logically independent of the scientific realism vs. anti-realism controversy. But as I will show, the debate is closely related to another long-standing and well-known philosophical dispute, namely that over what types of factors determine the emergence and acceptance of scientific results. It is thus unclear whether the contingency vs. inevitability debate constitutes an independent and distinct philosophical problem.

The third part of this paper is concerned with the *challenge of decidability*. It is often believed that case studies from the history of science warrant specific philosophical views on the contingency issue. However, many commentators also note that it may be difficult, if not impossible, to settle the conflict on the basis of historical evidence. As I will show, any amount of evidence accumulated in favor of one side of the conflict can be rejected by the rival side. How evidence from our actual history of science could settle the disagreement between contingentists and inevitabilists therefore remains an open question.

In the fourth part of this paper I offer some suggestions regarding what direction the debate should take in the future. I argue that in order to make progress in the present discussion, we need to distinguish more clearly between different contingency and inevitability claims. To this end, I present a taxonomy that has the structure of an ordered quadruple. My taxonomy reveals that each contingency and each inevitability claim contains answers to the following four questions: (how) are alternatives to current science possible, what types of alternatives are we talking about, how should the alternatives be assessed, and how different are they from actual science?

2. The challenge of definition

What is at stake in the debate between contingentist and inevitabilist interpretations of science? What exactly do the conflicting interpretations state and what is their disagreement about? At present, the most pronounced attempts to define the philosophical positions of "contingentism" and "inevitabilism" in a systematic manner come from Ian Hacking (1999; 2000) and Léna Soler (2008a; 2008b). Their strategies for arriving at a rigorous understanding of the issue differ markedly. While Hacking tries to circumscribe what it takes for contingentism and inevitabilism to be philosophically *meaningful*, Soler tries to capture what it takes for them to be *controversial*.

In the following I reconstruct Hacking's and Soler's reflections. Their approaches constitute important steps towards clarifying the present issue, but as I will show, both authors fail to do justice to the full variety of contingency and inevitability claims.

Hacking interprets the conflict as centering on the results of science.[3] He formulates the question to which contingentists and inevitabilists are supposed to 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)

The inevitabilist gives an affirmative answer, whereas the contingentist thinks that "there could be alternative non-equivalent but equally successful sciences" (Hacking 2000, 64). The notion of scientific success is central here since for Hacking, the contingency issue is best understood as revolving around an *equal success claim*. Hacking takes Andrew Pickering's *Constructing Quarks* (1984) as the paradigm case for a developed doctrine of contingentism:

> When Pickering says that the actual development of high-energy physics was highly contingent, he intends us to think of something like high-energy physics as a *rich and triumphant* international science that evolved after

[3] Here the notion of a result is construed broadly enough to cover both theories and experimentally established facts, yet narrowly enough to exclude the technological applications and the wider social consequences of science (Hacking 2000, 59).

> World War II and is regarded *as a tremendous success* – but this imagined fundamental and *equally successful* physics does not proceed in anything like a quarky way. (Hacking 1999, 69 my emphasis)

As Hacking defines it, contingentism affirms the possibility of an *equally or comparably successful* alternative to actual science. Inevitabilism, on the other hand, denies this possibility. It claims that any science, if successful, would either have to imply, contain, or arrive at, a state that is roughly equivalent to that of actual science (Hacking 2000, 60). The prototypical inevitabilist according to Hacking is Steven Weinberg, who believes that research in physics follows the "pull of reality" (Weinberg 2001, 103) and will eventually arrive at the discovery of a final theory – "a theory of unrestricted validity, a theory applicable to all phenomena throughout the universe" (Weinberg 2001, 137) – no matter what are the culturally determined starting points of the research process.

Seeking to identify the conditions that are required to create a genuine conflict between contingentism and inevitabilism, Léna Soler goes beyond Hacking's reflections. Like Hacking, she sees the contingency issue as revolving around an equal success claim. But Soler adds two further points that, according to her, lie at the core of the conflict.

First, Hacking does not address the question as to how much difference between two equally successful alternative sciences there has to be for a controversial contingency claim to arise. Soler is more precise. In her view, controversial forms of contingentism must state the possibility of results that are *irreducibly different* from those of actual science. Irreducible difference here refers to "a logical incompatibility or some other kind of insurmountable irreconcilability" (Soler 2008b, 232); for example, "an ontology incompatible with ours" (Soler 2008b, 233).

Second, Hacking remains vague about the time-dimension of the issue. He discusses arguments about what could have happened in the scientific past, as well as claims about what will happen in the future development of science. Soler is less ambiguous. According to her, any conflict between contingentist and inevitabilist views will sooner or later come to focus on the long-term historical development of the sciences, or on the ideal end of scientific research (Soler 2008b, 233). The argument between contingentists and inevitabilists will at some point center on whether the historical trajectories of science converge in the long run, and on the time interval required for them to do so (Soler 2008b, 234).

An analogous description of the temporal structure of historical contingency and inevitability claims has been given by Yemima Ben-Menahem (1997). Ben-Menahem defines contingency in terms of high sensitivity to the initial conditions of a process. Contingent historical processes are such that similar initial conditions can lead to different types of effects and hence to diverging historical trajectories. Inevitability, by contrast, occurs when the outcomes of the historical process are insensitive to the initial conditions, such that different initial conditions lead to the same effects (Ben-Menahem 1997, 100–101).

From Hacking's and Soler's reflections, we can extract three issues that would be involved in any genuine conflict between contingentism and inevitabilism: *equal success*, *irreducible difference*, and *long-term convergence or divergence*. By identifying these issues, Hacking and Soler make important steps towards a clarification of what is at stake in the present debates about whether science and its results are contingent or inevitable. However, as I will show, in the relevant literature we find many claims and views on the issue that are, in fact, nor formulated in terms of equal success, irreducible difference, and long-term development. There is more variety in the present debates than Hacking and Soler acknowledge.

I begin my discussion with the intuition voiced by many commentators, including Hacking himself, that contingency and inevitability come in degrees. Hacking offers a quantitative estimate of his own allegiances, rating himself as a 2 on a scale from 1 to 5, where a score of 5 is a strong contingentist position, while 1 marks a strong inevitabilist view (Hacking 1999, 99). Allan Franklin follows his example, rating himself as a 2 as well (Franklin 2008, 243). However, both authors leave entirely open what the relative "strength" of contingentist and inevitabilist positions depends on. According to Hacking, as reconstructed above, the core contingentist claim is that an alternative, non-equivalent, yet equally successful science is possible. What would constitute a weaker (or a stronger) version of this claim?

As far as I can see, there are three factors in Hacking's characterization over which strength could be quantified: *success*, *difference* and *possibility*.[4] Presumably, the strongest contingentist position would amount to the claim that it is possible for an *exactly equally*

[4] Let me note at this point that many of the central concepts involved when formulating views about contingency and inevitability (alternative science, scientific result, irreducible difference, equivalence, convergence, scientific success, historical possibility, and so on) are potentially problematic. I cannot discuss the difficulties related to these concepts in detail in this article. However, I will return to the question as to how the possibility of alternatives can be conceptualized in the last part of this paper.

successful and yet *insurmountably different* alternative science *to emerge at any point* in the history of science. Weaker contingency claims could then be construed by weakening one, two, or all three aspects. For example, a modest contingentist may hold that equally successful, insurmountably different alternatives are possible, but not at any point in the history of science. Or she might claim that radically different alternatives are always possible, but that such alternatives would not be exactly equally successful. Or that alternatives are possible, but that standards of success are contingent. And so on.

Note that by allowing contingency to come in degrees, we have admitted some variance into the contingentist camp. There can be disagreement among contingentists regarding scientific success, the differences between alternative sciences and the historical possibility of alternatives to actual science. More importantly, it is not clear how the various "weakened" contingency claims should be ranked among each other given that they involve different concepts. Which contingentist claim is stronger: the claim that insurmountably different alternatives are always possible but that they might not be exactly equally successful, or the claim that insurmountably different and equally successful alternatives are possible but cannot emerge at any point in the history of science?

The very idea that contingentist and inevitabilist views can be arranged on a one-dimensional scale has been forcefully challenged by Joseph Martin (2013). Martin argues that what distinguishes different views on the issue is not how much contingency they allow in science, but rather "how they hold science contingent, (…) what elements of science they hold contingent, and (…) what those elements are contingent upon" (Martin 2013, 919). According to Martin, there are genuinely different types of contingentism and inevitabilism that cannot be arranged on a smooth scale, but that can only be captured in a much more complex taxonomy. I agree with Martin's assessment, and will further develop his taxonomical approach in the final part of this paper. In this section, I want to illustrate that the variety of genuinely different contingency and inevitability claims complicates attempts to reach a coherent and systematic understanding of the issue. When examining the literature in detail, one is confronted with a range of different contingency and inevitability concepts that do not neatly fit the characterizations offered by Hacking and Soler. Here are some examples.

As noted above, Soler and Hacking make equal or comparable success of alternatives central to contingentist scenarios. And yet, many contingentists refer to alternatives that are less or something other than equally successful.

An example of a contingentist scenario without equal success is presented by Bowler (2008). Bowler argues that if Darwin's *Origin of Species* had not been written, the historical development of biology would have taken a different course. In particular, there would have been no theory of natural selection. Evolutionary theories that conceptualized evolution to progress towards a predetermined goal would have dominated the late nineteenth century. Bowler claims that a drastically different alternative biology was possible. But he does not claim that this alternative biology would be equally successful as ours.

A similar insistence on the possibility of alternatives that does without an equal success claim can be found in debates on scientific pluralism. Pluralists believe that there can be multiple legitimate investigative aims and, therefore, multiple legitimate approaches towards a specific subject matter. But these approaches do not have to be exactly equally successful. They often address partly overlapping and partly different questions. They contribute to different aims, and satisfy different epistemic values (Chang 2012, 273–78; Longino 2006). The alternatives are all successful, but not equally or comparably successful. Sometimes, they may even be said to be methodologically incommensurable. Clearly, this view cannot be captured on the basis of the concept of equal success, as the precise point of methodological incommensurability is that the existence of different goals and/or different standards of success obstructs a neutral comparison between the different accounts.

In the Sociology of Scientific Knowledge, we find yet another case of contingency without equal success. Sociologists often argue that a scientific result is contingent because, in a given situation of scientific decision-making, an alternative result could have been rationally accepted. A particularly clear example can be found in Harry Collins' study of the early searches for gravitational radiation. Collins argues that it was rational to reject the early observation reports of high fluxes of gravitational radiation, but that accepting these reports would have been rational too.

> It is quite reasonable that they [the arguments and evidential considerations – K. K.] were made to add up the way they did, and it would

have been quite reasonable had they been made to add up another way. (Collins 1994, 502)

The general idea is that the available evidence and scientific methodology underdetermine the choice between alternative scientific results. More than one result can be reasonably and rationally defended, so that context-specific social factors have to join for scientists to reach a decision. It is rational acceptability, and not equal empirical success, that is at issue here.

Similar problems arise with respect to the temporal dimension of the issue. While Hacking is ambiguous about this point, Soler makes it clear that, for her, disagreement over what happens in the long-term development of science is at the core of the conflict between contingentism and inevitabilism. And yet, if we consider the debates over whether the acceptance of scientific results is determined by contingent factors, the long-term dimension is surprisingly absent.

Consider, for example, the rationalist inevitabilism defended by Franklin. In his criticisms of sociological accounts of scientific controversy, Franklin attempts to show that scientific consensus is not the result of local, social and contingent factors, but rather emerges from critical reasoning being applied in the evaluation of the available evidence. Franklin reconstructs past episodes of scientific decision-making, arguing that in each episode the acceptance of one result over its rivals was rationally mandated and in this sense inevitable (Franklin 1990, 162–192; Franklin 1994; Franklin 2008). This is not an argument about whether or not the historical trajectories of alternative, counterfactual sciences will converge in the long run. It is not an argument about the long-term development of science at all. Rather, Franklin takes a stance on the question as to whether an actual decision that was made in the past of our science was contingent or rationally inevitable.[5]

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[5] In personal conversation, Soler has suggested that even Franklin would at some point be drawn to think about the issue in terms of the long term. The inevitabilist who judges that past decisions were rational is also led to assume that if an alternative result would have been (irrationally) accepted, then this choice would be corrected sooner or later. According to Soler, it is this idea which preserves the conflict between contingentism and inevitabilism. The issue demands more space that I have here. But I am inclined to disagree. I think that the contingentist and the inevitabilist can have a meaningful disagreement about episodes in past science without invoking long-term considerations. First, one may want to avoid long-term considerations altogether, on the grounds that while we can assess whether an actual episode of past science was contingent or inevitable, we lack the epistemic capacities for long-term prognostication. Second, even when we have determined that a specific episode of past science was contingent or inevitable,

Finally, one form of contingentism not captured by Hacking and Soler ties the concept of contingency to the virtue of epistemic humility. According to this view, contingentism, but not inevitabilism, evinces a sense of epistemic humility that acknowledges the limits of human epistemic capacities (Cooper 2007; Kidd in press). The contingentist who is motivated by considerations of epistemic humility does not engage in counterfactual speculation about whether an alternative, equally successful science could produce irreducibly different results in the long run. Rather, she argues that our epistemic situation does not allow us to assess such alternative scenarios in the first place. As a point of epistemic humility, we should be open to the possibility of alternative courses of development – we should embrace a contingentist attitude.

To conclude, while Hacking and Soler take important steps towards clarifying the issue, they do not adequately capture the full variety of different contingency and inevitability claims that we encounter in the literature. This raises a general challenge. We have seen that the various claims about contingency and inevitability discussed do not line up to form two opposing camps or a smooth scale. And perhaps we cannot even identify the essential issues that are at stake in the conflict. Does this leave anything general to say about whether the results of science are contingent or inevitable? The challenge is to reach a coherent and systematic understanding of the issue that does justice to the existing variety of views on contingency and inevitability in science.

3. The challenge of distinction

A recurring theme in the present discussions on contingency is how the issue relates to other philosophical debates, most importantly to the controversies over scientific realism and social constructivism. Hacking suggests that realists will tend to argue that the results of science are inevitable, while social constructivists will take a contingentist stance (Hacking 200, 61). Soler, in contrast, tries to disentangle the two issues, arguing that the contingency problem is logically (although perhaps not psychologically) independent of these more familiar philosophical discussions (Soler 2008b, 231).

Soler's assessment seems plausible as far as the realism vs. anti-realism debates are concerned. Howard Sankey has argued that the central metaphysical, semantic and

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this does not imply that the history of science is characterized exclusively by such episodes. The logical relations between the different temporal dimensions of the issue deserve further investigation.

epistemic commitments of realism do not imply inevitabilism (Sankey 2008, 261–262). And in a different context, Gregory Radick has shown that *anti-realist inevitabilist* and *realist contingentist* interpretations of the history of science are conceivable (Radick 2005, 23–25).

However, even if the contingency issue is to some degree independent of debates on scientific realism, it is nevertheless possible that it fails to be distinct from the central issues at stake in discussions on social constructivism. After all, the realism vs. anti-realism controversy and social constructivism are not congruent. Central to realism is a claim about the epistemic status of our best-confirmed or most successful scientific theories, which are regarded as true or approximately true. In comparison, social constructivism is not primarily a doctrine about the epistemic status of scientific theories. Typically, it is expressed as a view about the processes of scientific knowledge production, in which social and cultural factors are seen to play a causal role. Social constructivism is not simply a brand of anti-realism.[6] Hence, even if the contingency issue proves to be distinct from discussions about the epistemic status of scientific theories, it may not be clearly distinct from controversies over what types of factors determine the production and acceptance of scientific results.

To outline what the challenge consists in, I first need to introduce a few distinctions. I begin with the observation that inevitabilist and contingentist views often present themselves in the form of nuanced conditional statements, rather than as bold views about the absolute necessity or indeterminacy of science. This is particularly obvious for inevitabilism because usually, the inevitabilist does not take science or its results to be absolutely (metaphysically or logically) necessary (Hacking 2000, 58; Soler 2008a, 232). Even Weinberg, whose teleological picture of science progressing towards a final theory is as boldly inevitabilist as it gets, still acknowledges that the development of physics is contingent in some sense. A collapse of social support and funding could terminate research in particle physics altogether (Weinberg 1994, 234; Weinberg 2001, 226). The inevitability of scientific results is a "conditional inevitability": certain historical conditions need to be in place for scientific results to become inevitable. There are thus

[6] The relations between realism and social constructivism are, of course, complex and cannot be discussed in this paper. But some support for this view comes from recent arguments that the sociological explanation of scientific beliefs may even be compatible with a realist externalist epistemology (Lewens 2005; Kochan 2008).

two possible types of inevitability – *absolute inevitability* and *conditional inevitability* – and most inevitabilist claims about science are of the latter type.

There are also two possible types of contingency: *unpredictability contingency* and *causal-dependence contingency*. This distinction was introduced by John Beatty in the context of evolutionary theory (Beatty 2006). On the unpredictability-view events are contingent because they are underdetermined by antecedent conditions. The developmental process is stochastic and unpredictable. On the causal dependence-view, events are contingent because they are determined by a specific set of antecedent conditions such that changes in initial conditions lead to different outcomes.

A parallel distinction can be made regarding contingency in science (see also Martin 2013, 924–925). One author who may be said to fall on the "unpredictability" side of contingentism is Pickering. His strong emphasis on scientists' unconstrained agency evokes an indeterministic picture of scientific development: "In principle, the decisions which produce the world are free and unconstrained. They could be made at random, each scientist choosing by the toss of a coin which stance to adopt" (Pickering 1984, 405–406). Pickering suggests that at each point in the history of particle physics, scientists' free and unconstrained agency would have enabled them to make decisions diverging from the ones that were actually made.[7] (In less drastic ways the openness and unpredictability of science is also emphasized in French 2008, 572–573; Trizio 2008, 225–256).

However, most contingentists state their views in ways that are compatible with causal determinism. For example, many sociologists claim that scientific results are contingent because their acceptance is dependent upon certain contextually variable social and cultural factors. According to the Sociology of Scientific Knowledge, scientific decision-making cannot be explained "in terms of any general context-independent criteria" (Barnes 1974, 62), or in terms of purely epistemic factors (see also Barnes, Bloor, and Henry 1996, 25–33). And yet, it can be explained causally: the acceptance of certain scientific results becomes explicable when the local context with its particular constellation of biological, social, cultural, psychological and intellectual factors is taken into account (Bloor 1976, 7; Bloor 1981, 199). Contingency then enters along with the local character of the causes that explain why and how a certain result became accepted

[7] Both notions of contingency – contingency as "unpredictability" and contingency as "causal dependence"– can be found in Pickering's writings, which makes his account at times seem inconsistent (indeterministic notions become most explicit in Pickering 1984, 6–8, 404–405; Pickering 1995, 19–24, the theme of causal determination by antecedent factors surfaces in Pickering 1984, 311; Pickering 1995, 185).

or rejected. A different constellation of local factors would have resulted in a different outcome being accepted and subsequently in a different research trajectory being pursued. Contingent means contingent upon a specified set of local, contextually variable factors.

Having acknowledged that inevitabilists typically make conditional statements, and that many contingentists make statements about causal dependencies, the opposition between them appears a lot less drastic. Making a conditional statement the inevitabilist reveals herself to be, in a sense, also a contingentist: she believes that the inevitability of scientific results is contingent upon something – upon the existence of science in its modern, successful form, upon the availability of reliable scientific methods, upon the rational conduct of scientists, or upon some other factor that may have been different. This view is structurally similar to causal-dependence contingentism: the results of science could have been different, if some specified factor had been different.

Conversely, we can see that the causal-dependence contingentist is often also an inevitabilist. Consider once again the sociologists' stance on contingency. The contextualist view that local factors determine scientific consensus formation is contingentist because it implies that variations of the local context will bring about variations in scientific results. However, this view seems to affirm a form of conditioned inevitability. As John Henry points out

> the social constructionist historian of science wants to offer an account that is seen as causal; and, given that the account is couched in terms of (…) a pervasive set of social concerns, it seems hard to deny the suggestion that the development of science is inevitable if these social concerns are dominant. (Henry 2008, 556)

Put differently, *conditional inevitability* and *causal-dependence contingency* are compatible in principle. And because of this compatibility, there often is a contingentist side to inevitabilism and an inevitabilist side to contingentism.

Of course, this does not make the conflict between them disappear. However, it changes our understanding of what the conflict is about. What is at stake is usually not whether scientific results are contingent or inevitable *tout court*. Rather the question is what types of factors determine scientific results and their acceptance. Are scientific methodology and rationality sufficient for determining scientific consensus, or do cultural and social factors play a substantial role in scientific decision-making? Are the drivers of scientific development epistemic, social, or both? Are the determinants of scientific results general, universal and context-insensitive, or are they local, variable and context-specific? In Radick's words, are scientific theories "independent" of their social and cultural history, or are they rather "inseparable" from their concrete historical trajectories (Radick 2003, 144)?

There is, of course, a legitimate discussion to be had about these questions. But when expressing what is at stake in the conflict in this way, the contingency vs. inevitability issue does not appear as a distinct philosophical problem anymore. Rather, it merges with a more familiar debate, namely that over the (internal or external, social or epistemic, universal or particular) determinants of scientific development. One may want to give this a positive twist and argue that contingency has been the "missing word" in debates about the determinants of scientific development. But even then, the question remains as to what exactly is gained by phrasing the issue in terms of contingency rather than simply in terms of the causes that determine scientific consensus formation and scientific change. The challenge of distinction rearises: What does the distinct philosophical contribution and value of the contingency issue consist in? What new insights do we gain once we add the concept of contingency to our discussions of scientific development?

4. The challenge of decidability

A final problem that I want to discuss relates to the question as to how the conflict between contingentist and inevitabilist interpretations of science could be settled. It is often believed that case studies from the history of science warrant specific philosophical views on the contingency issue. Most arguments in favor of contingentist interpretations of science have been put forward on the basis of historical accounts of past scientific developments (Bowler 2008; Chang 2012, 14–65; Collins 1985, 79–111; Cushing 1994; Pickering 1984; Radick 2003; Radick 2005). And inevitabilists too cite historical material to underpin their views (Weinberg 1994, 236–37; Weinberg 2001, 198–199; Franklin 1990; Franklin 1994; Franklin 2008).

And yet, some commentators have pointed out that it may be hard, or perhaps even impossible, to settle the conflict on the basis of evidence from the actual history of science (Soler 2008b; Trizio 2008, 257–258). As I will show in the following, these commentators are right. It is hard to bring the philosophical positions at stake into contact with the type of neutral empirical evidence that presumably would help to settle the conflict. Therefore, a challenge of decision arises: no amount of evidence accumulated by one side of the debate will suffice to convince the other side.[8]

I begin with the problem as it presents itself for the contingentist. Hacking has suggested that contingentism is vulnerable to the *put up or shut up argument*. "Show us an alternative development" (Hacking 2000, 67), the argument goes, for as long as we never encounter actually existing equally successful sciences that produce rival results, the possibility of such alternatives remains idle speculation.[9]

How could the contingentist *put up* and offer the inevitabilist some evidence that alternative scientific theories could really have developed, become accepted and subsequently entrenched in the history of science? As far as I can see, the contingentist has three options – first, the appeal to past embryonic alternatives, second, the appeal to presently existing alternatives, and third, the rejection of the demand to put up. And yet, unfortunately, none of these strategies will convince the determined inevitabilist.

The first option is to point to embryonic alternatives – theories that have been conceived and at least partly developed by past scientists, and were considered by them as serious rivals to the theories that would later become entrenched in our actual history of science. In order to turn such past embryonic alternatives into evidence for contingentism, the contingentist has to accomplish two tasks. First, she has to show that the embryonic alternatives enjoyed some success and warrant. Second, she must offer a plausible story as to how these embryonic alternatives would have developed into successful theories in subsequent stages of the historical process. Examples for this strategy can be found in Chang's account of the historical fate of phlogiston theory (Chang 2012, 43–50) and in Radick's reconstruction of Weldonian biometry as a possible alternative to Mendelian genetics (Radick 2005, 34–40).

[8] The problem of a lack of neutral historical evidence may not be specific to the contingency vs. inevitability issue. As I have argued elsewhere, it is unclear in general whether case studies from the history of science offer the type of neutral and generally agreed upon evidence that would be required for settling philosophical issues (Kinzel, 2015; Kinzel in press). However, with repect to the contingency issue, the problem takes a distinct form.

[9] At first sight, the *put up argument* appears to rest on a faulty inference from the scarcity of alternatives to the impossibility of alternatives. Read in this way, the argument is to be rejected as invalid. However, the argument can also be read more charitably. In this reading, the *put up argument* attempts to shift the burden of proof on the contingenist, asking her to back up her claims with positive historical evidence.

And yet, despite all the historical detail of Chang's and Radick's reconstructions, this strategy fails to convince the inevitabilist. From her perspective, the contingentist has not put up, but merely offered more speculation. First, the inevitabilist may question whether the embryonic alternatives were abandoned for contingent reasons, rather than because of rational necessity. And second, she may question whether anything interesting would have grown out of these embryonic alternatives had they been retained. Steven French's reflections on what constitutes a genuine historical possibility voice skepticism about both aspects. If the alternatives are conceived of and formulated only in a rough grained, embryonic way, there is no saying whether they are genuine historical possibilities, French argues. But if they exist in the form of elaborate, fine-grained and well-understood theories, then they are not counterfactual possibilities at all, but just actual theories that were abandoned at some point, and presumably for good reasons (French 2008, 569–570). For the inevitabilist, the historical case built on the basis of abandoned embryonic alternatives in the scientific past is simply too weak. It does not truly support the contingentist claim.

If embryonic past alternatives do not convince the inevitabilist, what else does? Presumably, an actually existing alternative to our science that produced radically different results while remaining comparably successful as our science is. What would happen in such a situation is explored in some detail in a thought experiment presented by Soler (Soler 2008b). Suppose there were two alternative mature and successful physics that focused on the same subject matter and were conducted over a sufficiently long period of time. Suppose further that attempts at unifying the sciences failed and that after a certain amount of time and research the two physics continued to produce different results. Would this constitute decisive evidence for contingentism? As Soler points out, it would not: the fact that at a specific point in historical time two scientific theories have not been reconciled does not necessarily indicate that they are in principle irreconcilable. The inevitabilist need not accept an actual episode of unsuccessful reconciliation as evidence for the in-principle impossibility of reconciliation (Soler 2008b, 240).

At this point, it appears that no amount of evidence accumulated for contingentism will ultimately convince the inevitabilist. A third contingentist strategy may thus consist in rejecting the *put up or shut up* argument itself, rather than engaging with it. The rejoinder goes as follows. The task of coming up with successful alternative theories is hard. It demands the collective efforts of whole scientific communities. For an alternative to arise, existing resources would have to be used differently, scientific communities would have to direct their efforts in different directions, the social organization of science would have to be different, and so on. But since one cannot alter these historical conditions, the demand for putting up cannot possibly be fulfilled. And to a demand that is formulated in such a manner that it cannot possibly be fulfilled, one need not answer (Kidd in press; Trizio 2008, 258).

This appears to be a sound strategy, but it comes at a cost. By defending her position in this way, the contingentist has made evidence from the actual history of science irrelevant to the assessment of the counterfactual scenario that she conjured up. The reason for this has to do with the nature of counterfactual statements. In general, how can evidence be gathered for statements about the states of affairs that obtain in counterfactual worlds? The standard answer to this problem is that empirical investigations of the actual world can be used to assess counterfactual conditionals if the hypothetical antecedent refers us to a possible world that differs from the actual world as little as possible – preferably only in that the antecedent is true (Stalnaker 1968, 111–112). For the counterfactual conditional to be empirically assessable, the actual and the counterfactual world have to be different, yet also as similar as possible.

And yet, in her rejection of the put up or shut up demand, the contingentist has violated the requirement of similarity. She has exploded the differences between the actual and the possible world. It is not one historical fact, one local aspect of a specific historical situation, but the history of science on the whole that we imagine to have been different in the counterfactual scenario. Now if the possible world we are referring to differs from ours so substantially, it becomes hard to imagine what would be the case in that possible world. Evidence from our actual history of science cannot serve as a clue to what is the case in this substantially different, distant possible world. (For a similar argument regarding feminist historiography of science see Henry 2008, 558–559).[10]

Now interestingly, the inevitabilist suffers from symmetrical difficulties. As Hacking points out, there is an equivalent of the *put up charge* on the side of inevitabilism, namely the *tautology argument*. Hacking uses the value of the velocity of light to illustrate the problem. Take the claim that scientists would inevitably have discovered the correct

[10] Note that this is not as big a problem for the contingentist who is motivated by epistemic humility, since she does not try to settle the issue by recourse to evidence from the history of science, but by recourse to non-evidential considerations regarding what constitutes virtous behavior in the face of limited epistemic capacities (Kidd, in press).

value for the velocity of light, about 186.000 miles per second. The velocity of light is a fact of nature, and no serious and laborious enough attempt to investigate the properties of light could have failed to discover this constant. So the inevitabilist claims. But the contingentist will be quick to reject this claim on the basis of historical evidence. The first calculations of the velocity of light undertaken by Ole Römer in the 17th century reached results of about 140.000 miles per second. Römer pursued serious and laborious research that did not lead to our results. And this shows that the inevitabilist is wrong (Hacking 2000, 65).

As a possible response, the inevitabilist may point out that Römer was not in a position to find the correct value of the velocity of light because he lacked sufficiently precise scientific instruments. Refining her claim, the inevitabilist will now state that if Römer had used our equipment with sufficient skill, and without making a mistake, then surely he would have reached the value that we have arrived at.

The resulting inevitability claim sounds much more plausible. But unfortunately, the inevitabilist position has become vacuous: "So the claim is that if they used our techniques and made no mistakes, they would get our answers? We are close to an empty platitude, a tautology." (Hacking 2000, 65–66) The problem is symmetrical to the problem concerning counterfactuals that troubled the contingentist: in order to make her claim plausible, the inevitabilist has imploded the differences between the actual and the counterfactual scenario. She has made the alternative science referred to in the antecedent look so much like actual science that we are not dealing with a meaningful counterfactual conditional anymore.

Can the inevitabilist escape this predicament and offer some historical evidence that the development of science is in fact inevitable? A possible way out appears to be given by episodes of convergence or reconciliation in which two or more strands of historical development that can be seen to be sufficiently different did in fact produce the same results. According to Radick, historical convergence constitutes a straightforward empirical test of inevitabilism. "The greater the number of past trajectories that converged on the same conclusion, and the greater the independence of those trajectories, the more plausible will be the idea that the conclusion was inevitable." (Radick 2005, 25)

In order for historical episodes of convergence to work as evidence in favor of inevitabilism, the inevitabilist has to show two things. First, she has to demonstrate that the convergent historical trajectories were truly independent of each other, rather than the product of common influences. And second, she has to show that the different historical trajectories did really reach the "same" theory. On both fronts, the contingentist can attack. Radick's example is the convergent rediscovery of Mendelism in 1900 by De Vries, Correns and Tschermak. Radick shows that it is not clear that the discoveries of De Vries, Correns and Tschermak were truly independent of each other, as they shared common influences and had all been aware of Mendel's work at earlier stages of their research (Radick 2005, 27–29). Moreover, it is not obvious that they were really discovering the same theory that Mendel had formulated a generation earlier, because "Mendelian principles resembled Mendel's conclusions only in a piecemeal and distorted way" (Radick 2005, 28). The inevitabilist point is harder to establish than it may appear.

But more fundamentally, it is not even the case that actual examples of convergence constitute conclusive evidence for inevitabilism. To illustrate the point, suppose, as we did in the case of contingentism, that there existed two truly independent, mature and successful sciences that focused on the same subject matter and were conducted over a sufficiently long period of time (Soler 2008b). Suppose further that after a certain amount of time and research, the two sciences would be reconciled and hence converged on one and the same theory. Would this now constitute decisive evidence for inevitabilism? As before, it would not. As Soler notes, scientific theories are "empirical, in-progress systems, that might be transformed, and are often transformed, *during* and *by* the attempts of reconciliation" (Soler 2008b, 234). The reconciliation process resulting in the convergence of the two sciences may thus have changed the theories substantially (Hacking 1999, 76 makes a similar argument with respect to deduction). Rather than an expression of the inherent inevitability of science, the convergence of two sciences can be seen as a contingent result of the creative and open-ended practices of theory change and theory transformation. For the contingentist, convergence thus does not constitute conclusive evidence for inevitability.

We can conclude that, when it comes to historical evidence, contingentism and inevitabilism suffer from symmetrical difficulties. Contingentism tends towards non-assessable counterfactual scenarios, while inevitabilism tends towards meaningless tautologies. But even if these obstacles are avoided, no amount of evidence accumulated by one side in the debate will suffice to convince those on the other side. The inevitabilist can reject unsuccessful reconciliation as evidence for contingency, while the contingentist can reject convergence as evidence for inevitability. As Soler puts it, "(w)e must have already chosen our camp (…) to be in a position to conclude something about the significance of what is found" (Soler 2008b, 240). We are thus confronted with the challenge of decision: can we even conceive of a type of empirical evidence that would be sufficiently neutral and could be agreed upon by all participants in the conflict?

5. Progress in the Contingency/Inevitability Debate: A Taxonomical Strategy

The preceding sections surveyed the present debates on contingency and inevitability in science. Three major challenges have been identified – the *challenge of definition*, the *challenge of distinction* and the *challenge of decidability*. In the worst case, these problems hamstring meaningful debate on the present issue. In the best case, they provide incentives for the articulation of more precise answers to the question as to whether, in what sense, to which extent and in which aspects science may be said to be contingent. In any event, attempts at formulating convincing positions towards the question of contingency and inevitability in science will have to face up to these challenges.

Answering to all three challenges is beyond the scope of this paper. However, I believe that in order to make progress in the present discussions, we need to refine our conceptual toolbox. I think that a more systematic overview over the different conceptual options that we have when expressing contingentist and inevitabilist views can help to move the debate forward significantly.

In the remainder of this paper, I will therefore sketch a taxonomy that allows us to distinguish more clearly between the different options that we have. My approach is inspired by Martin's discussion of different forms of contingency, but tries to be more inclusive. Martin's taxonomy is two-dimensional, it distinguishes between two types of contingency and five aspects of science that contingency claims might be about (Martin 2013, 927–928). My own taxonomy is a quadruple of four dimensions, such that each claim contains an answer to the following four questions:

A. Possibility of Alternatives: (how) are alternatives to current science possible?

B. Type of Alternatives: what aspects of science, what types of alternatives are we talking about?

C. Assessment of Alternatives: how should the alternatives be assessed?

D. Relation of Alternatives: how are the alternatives related to one another, how different are they from actual science?[11]

On my account, each contingency and each inevitability claim can be described as a function of these four dimensions.[12] I explain my taxonomy beginning with the contingentist side (Table 1). On the basis of an understanding of the different forms that contingency claims can take, I will then also explain the different options for expressing inevitabilism (Table 3).

Table 1: Taxonomy of Contingency Claims

A. Possibility of Alternatives	**B. Type of Alternatives**	**C. Assessment of Alternatives**	**D. Relation of Alternatives**
Logical possibility	Science as such	equally successful	logically incompatible
Mere logical possibility	Theories	not less successful	Ontologically incompatible
Historical possibility	Paradigms	comparably successful	Irreconcilable
Unpredictability	Methodology	rationally acceptable	non-translatable
Causal dependence	Technology	useful for achieving goal X	non-convergent
Short term / Long term	Data interpretation	virtuous according to norm Y	non-deducible
	Social structures of science	etc…	Incommensurable
	etc.		etc.

Each contingency claim picks (at least) one item from each of the four sections and combines these items in a consistent manner. Because of the consistency requirement, not all combinations are acceptable. But there are enough unique and consistent combinations for there to arise an interesting variety of contingency claims. For example,

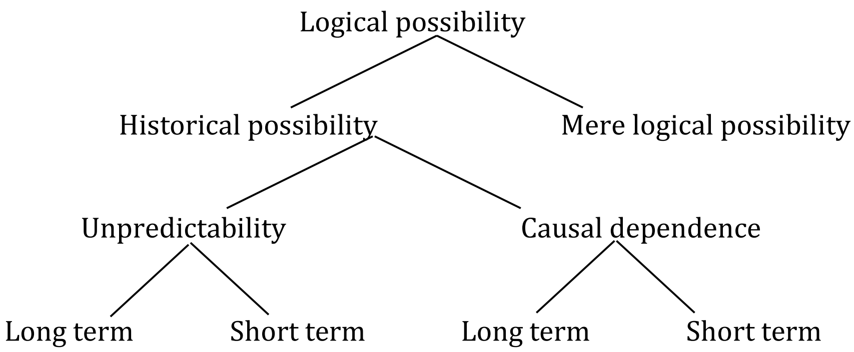
[11] A fifth dimension worth exploring is whether our epistemic standpoint allow us to make the sorts of appraisals of counterfactual histories that answering questions A-D would require. This issue is brought up in the context of discussions of epistemic humility.

[12] I want to thank Martin Kusch for this suggestion.

based on the underdetermination thesis, a contingentist may claim that *equally successful* but *ontologically incompatible* alternative *theories* are *logically possible*. Another contingentist may prefer to base her views on a Kuhnian conception of paradigm-driven research, and may hence claim that alternative *paradigms*, that are *virtuous* according to their own norms of what constitutes good science, could have emerged historically (are *historically possible*), and that such alternative paradigms would be *incommensurable* with actual science. We can see that different contingency claims emerge, depending on how alternatives are thought to be possible, how they are described, how they are assessed and how they are seen to relate to actual science.

I believe the contents of sections B. – D. are relatively clear. Section A. however is more difficult. It deals with the different options we have for expressing how the alternatives in question are possible. The items in this section cannot simply be presented in the form of a list. Rather, they present themselves as a branching tree-diagram. In the following, I will take a closer look at the contents and structure of section A (Table 2).

Table 2: Section A. Possibility of Alternatives



Logical possibility. Any form of contingentism involves what Emiliano Trizio calls a “multiplicity thesis” (Trizio 2008, 254). In its most reduced form, this is the thesis that alternatives to actual science and its results are logically possible. This thesis builds the basis of all contingency claims.

Historical possibility. But logical possibility does not yet give us historical contingency. As Trizio points out, one could “maintain that a non-quarky high energy physics is possible (…) and still believe that, given the starting point of that research program, the introduction of the concept of quark was inevitable.” (ibid.). In some contexts, for example in debates on underdetermination, the mere logical possibility of certain types of alternatives may already be a controversial issue. But for a full fletched historical contingentism to arise, the multiplicity thesis needs to be accompanied by the view that the alternatives said to be possible in principle could have emerged in our actual history of science. One has to introduce a notion of historical (rather than mere logical) possibility. A lot then depends on how the idea of historical possibility, or historical emergence, is spelled out. There are two basic ways of how this can be done that we have already broached earlier.

Unpredictability. The unpredictability contingentist believes that antecedent conditions underdetermine later events, so that there is an indeterminist element in the processes of science. New, unpredicted developments and alternatives can spring from scientific practice at any point in a scientific research trajectory. Pickering's claim that "it is always possible to invent an unlimited set of theories" (Pickering 1984, 6) is a case in point. It expresses in a straightforward manner how the unpredictability contingentist thinks about the historical possibility of alternatives: the practices of science are such that alternatives can emerge at any time in the history of science.

Causal dependence. The causal-dependence contingentist thinks of historical possibility in a conditional manner. She does not believe that alternative approaches can always emerge out of the indeterministic practices of science. Rather, she thinks that specific historical circumstances determine whether alternatives can emerge, and if so, what form they take. The causal-dependence contingentist claims that an alternative could have emerged, had the historical circumstances been (perhaps only slightly) different.

Long term and short term. But historical possibility still only gives us part of the story. Even if everyone agreed that alternatives to actual scientific theories and approaches could have emerged (either because of the unpredictable nature of scientific practice or on the basis of causal dependence), the question remains whether the alternatives in question could have become accepted by rational scientists, and how they would have fared in subsequent scientific developments. Regarding these questions, two different options for formulating contingency claims arise: One can focus on the short-term dimension of scientific practice and claim that the acceptance of scientific results is contingent. Or one can focus on the long-term dimension of scientific change and claim that not only the acceptance of results, but also the subsequent historical trajectories scientific results are contingent. A combination of these two claims is possible and common, but for the sake of clarity, one should keep them apart.

Short term. Many contingentists focus on local and relatively short-termed processes. As an example, consider the sociologists' claim that the outcomes of scientific controversies, and hence the acceptance of scientific results, are contingent (Collins 1985; Pickering 1984; Pinch 1986). The general argument is typically as follows: in an actual historical situation of scientific controversy, scientists were confronted with a decision about which of competing results to accept. Scientists eventually accepted one of the rivals. But they could have opted differently and would have done so, if the local historical circumstances had been different.

This is a short-term contingency claim. It focuses on local and temporally restricted episodes of scientific practice, rather than on how the results and research trajectories of science develop in the long run.

Long term: Note the marked difference between this short-term perspective and Soler's view of contingency in terms of failure to converge, or Ben-Menahem's clarification of historical contingency in terms of branching trajectories (Ben-Menahem 1997). While contingentism about the short term is typically linked to a theory of how scientific results become accepted, contingentism about the long term is linked to a theory of what happens to scientific results after they have become accepted – in their subsequent historical development. Long-term contingency is connected to questions of change, continuity and progress in science. It presents a picture of the history of science as meandering and unpredictable, or as a complex branching configuration in which earlier stages of the process enable multiple trajectories at later stages.

The acceptance of some form of short-term contingency claim is a condition for contingentist visions about the long term. But the relations between the two temporal dimensions are not yet sufficiently explored.

Having presented an overview over the different expressions that contingency claims about science can take, we can now proceed to exploring the inevitabilist side of the debate. As we can see, the taxonomy of inevitability claims (Table 3) differs from the contingentist taxonomy in two important respects. First, in inevitabilist arguments, there is a conditional relation between how the alternatives are assessed and how they are seen to relate to each other. The contingentist had formulated some version of the claim that an alternative science could be both as virtuous as our science *and* significantly different from it. The inevitabilist, in contrast, express the view that *if* an alternative, equally virtuous science existed, *then* its results would be equivalent to those of our science (or an analogue view based on some of the other concepts listed in the taxonomy). In the table below, the conditional structure of inevitabilist arguments is symbolized by an arrow that connects sections C and D.

Table 3: Taxonomy of Inevitability Claims

A. Possibility of Alternatives	B. Type of Alternatives	C. Assessment of Alternatives ➡	D. Relation of Alternatives
Logical impossibilty	Science as such	equally succesful	identical
Historical impossibility	Theories	comparably successful	equivalent
Historical possibility, but…	Paradigms	rationally acceptably	translatable
not rational acceptability (short term) / subsequent elimination (long term)	Methodology	equally useful	deducible
	Technology	etc.	reconcilable
	Data interpretation		convergent
	Social structures of science		etc.
	etc.		

Another difference concerns the structure of possible claims regarding the (im-)possibility of alternatives to actual science. The contingentist's options presented themselves as a branching tree-configuration, in which each decision opened up a set of new options. The inevitabilist's options, in comparison, are arranged in a much simpler manner. They are given by which step in the contingentist argument regarding the possibility of alternatives is denied by the inevitabilist.

Logical impossibility. One form of inevitabilism denies that a certain type of alternatives is logically possible. For example, based on arguments against the underdetermination of theory by data, an inevitabilist may hold that empirically equally well-confirmed yet logically incompatible alternative theories do not exist, not even as a matter of principle (Laudan and Leplin 1991). As a consequence, contingentism about empirically equivalent, strongly underdetermined scientific theories would be ruled out.

Historical impossibility. A completely different strategy for arriving at an inevitabilist position would consist in denying that alternatives to actual science could emerge historically. Support for such a form of inevitabilism can come from social and historical determinism. A social determinist may hold that constraints and pressures from the socio-cultural world make the emergence of alternative scientific approaches historically impossible. Our actual theories would thus be inevitable, not in virtue of what the natural world is like, but as a consequence of what the socio-historical world is like. Note that the concepts listed in sections C. and D. are irrelevant for expressing this view. The idea is that scientific development is completely determined and alternatives cannot emerge. The fact that they cannot emerge is independent of whether they are successful, virtuous, equivalent, reconcilable, or not.

Historical possibility, but... Most inevitabilists formulate their points not in terms of logical or historical impossibility, but in terms of restrictions on possible alternatives that stem from the workings of rational and successful scientific practice. The general strategy is to accept that alternatives can emerge historically, but to argue that there are constraints on their acceptance and subsequent development. In general, the inevitabilist has two options about how to restrict the space of alternatives. The two options mirror the distinction between short-term and long-term contingentism.

…not rationally acceptability (short term). One way of upholding an inevitabilist claim while allowing that alternatives are logically and historically possible, is to utilize a strong criterion of rationally acceptability. This criterion serves to constrain the space of

acceptable alternatives to only a few. Examples for such arguments can be found in criticisms of the sociology of scientific knowledge (Laudan 1990; Franklin 1994; Gingras and Schweber 1986). The general idea is that situations of conflict between alternatives are decided on the basis of rational considerations. While many alternatives can emerge historically, in a conflict between them not all are rationally acceptable. The result that is eventually accepted can be regarded as inevitable in the light of scientific rationality. This form of inevitabilism focuses on the short-term processes of theory choice and on the acceptance of scientific results.

…subsequent elimination (long term). A different claim concerns the long-term dimension of scientific development. An inevitabilist like Weinberg can accept that alternatives are historically possible. However, he believes that alternatives that emerged from different starting points will either be eliminated in the course of scientific research, or will be transformed and eventually unified into a common framework. In the long-term inevitabilist view, the realm of possible alternatives shrinks down as science progresses. As in the case of contingentism, short-term and long-term inevitabilism are related. However, the precise character of these relations remains to be explored.

I hope that my taxonomical overview over the different expressions that contingency and inevitability claims about science can take has conveyed an understanding of the myriad options we have for expressing meaningful claims towards the issue. My analysis provides further support to a point that has already been emphasized by Martin: different views on the issue of contingency and inevitability in science cannot be arranged in form of a dichotomy, or on a smooth one-dimensional spectrum. There is not one conflict between "contingentism" and "inevitabilism", but rather a plethora of conflicts that may arise between the different claims and arguments regarding contingency and inevitability in science.

Given the complexities that come into view, it becomes obvious that existing concepts for thinking about contingency and inevitability in science need to be deepend and enriched, but also rendered more precise. I hope that in offering a taxonomy of contingency and inevitability claims, I have a provided a first step towards a conceptually more rigorous approach towards the issue.

Conclusion

In this paper, I presented a survey of the present debates on the problem of contingency in science. I identified three challenges facing current attempts to understand the conflict between contingentist and inevitabilist interpretations of scientific knowledge and practice. First, I have shown how difficult it is to define the positions of contingentism and inevitabilism in a way that is conceptually rigorous while also doing justice to the existing variety of views on the issue. Second, I have argued that the contingency issue may not be sufficiently independent from the questions as to which types of factors determine the course of scientific development, and thus may not constitute a distinct philosophical problem. And third, I have examined whether and how the conflict could be settled on the basis of empirical evidence from the actual history of science, finding that neither contingentism nor inevitabilism can produce the sort of neutral empirical evidence that would convince the other side. Finally, I have argued that in order to make progress in the present debate, we need to distinguish more clearly between different forms and expressions that claims about contingency and inevitability in science can take. To this end, I have presented a taxonomical overview over various contingency and inevitability claims. In my taxonomical approach, each contingency and each inevitability claim appears as a function of four dimensions: whether and how alternatives are claimed to be possible, how the alternatives are characterized, how they are assessed, and how their relations to one another are understood.

While I hope that my taxonomy helps to bring structure to the present debates on contingency in science, it does not reveal how the various views on contingency and inevitability are related to one another. Many open questions regarding the relations between different contingency and inevitability claims remain: do there exist relations of implication among different forms of contingentism? For instance, how are short-term and long-term contingentism related to one another? Do different forms of inevitabilism exclude one another? For example, can one be both a logical impossibility inevitabilist and a short-term inevitabilist about rational acceptability? Which types of contingentism are in conflict with which types of inevitabilism? Conversely, are some contingency claims compatible with some inevitability claims?

My suggestion for the future of the debate is not only to deepen our concepts and pay more detailed attention to the different ways in which views on contingency and inevitability are formulated, but also to explore more systematically the complex interrelations that hold between these different views.

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