

Contingency, Predication, and Counterfactuals in the History and Philosophy of Science

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For Judith

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Preface

This thesis might be termed philosophy of history of science. It draws parallels between two current conversations, one in philosophy of science, and one in the philosophy of history. Each of these deals, in its own way, with how we ought to understand science as a historical process. Philosophers of science have recently become concerned with the question: *to what extent can the results of successful science be considered either contingent or inevitable?* Historians, for some time, have confronted a similar concern: *how, if at all, should counterfactuals play a role in historical reasoning?* These are separate questions, but the debates around them share a susceptibility to clarification by a more nuanced understanding of contingency than they currently employ. The thesis develops a classification scheme for contingency and applies it to each of these debates.

The discussion is divided into two parts. Part 1 addresses questions of contingency and inevitability in science, showing that current conversations proceed with an impoverished understanding of contingency and an inappropriate opposition between contingency and inevitability. I propose a new contrary to contingency—predication—and show how fine graining of the contingency concept can expose the complexity that a simple contingent/inevitable distinction belies. Part 2 applies the refined understanding of contingency developed in Part 1 to historiographical questions about counterfactual reasoning. Counterfactuals have long been controversial among historiographers and philosophers of history. I show that a careful treatment of contingency substantiates a

skeptical stance on counterfactual reasoning and characterize the scope in which it might be applicable.

Although presented sequentially, these two discussions are mutually informing. Arguments for and against the value of counterfactual reasoning address issues relevant to understanding the role of contingency in history as well as vice versa. The taxonomy of contingency develops here in the context of discussing debates about contingency and inevitability in the development of scientific knowledge. The core conclusions about contingency as they relate to debates about the causal structure of history, however, might as easily have grown from the discussion of counterfactuals in the history of science. The two parts below can therefore be read as autonomous arguments sharing a common framework.

Part 1: Is the Contingentist/Inevitabilist Debate a Matter of Degrees?

I. Introduction

Ian Hacking, in *The Social Construction of What?*, asks his readers to assign themselves a number from one to five to describe how central contingency is to their personal conceptions of science. If you rate yourself at one, then you are a strong inevitabilist, whereas if you choose five, you are highly contingentist and probably have strong constructionist sympathies (Hacking 1999, p. 99). In response, Léna Soler questions whether this is the correct approach, and asks: “should we introduce degrees of contingentism depending on the kind of contingent factors that are supposed to play a role?” (Soler 2008a, p. 223).

Herein, I answer Soler’s question in the emphatic affirmative, and therefore the question posed in the title of Part 1 with a resounding “no.” I argue that contingency in science can be understood as a collection of distinct concepts, distinguished by how they hold science contingent, by what elements of science they hold contingent, and by what those contingent elements are contingent upon. What separates one contingentist from another is not that one tags herself a two and the other fancies himself a five according with how strongly they believe science might have developed differently. Their disagreement arises from the fact that they understand contingency-producing factors to act differently on different aspects of the scientific process. Contingency is a “what” question, not a “how much” question.

Before beginning this discussion it will be useful to review the contingentist/inevitabilist (C/I) debate. I do so in Section II by reconstructing positions the debate's central figures stake out. Ian Hacking, who coined the terms "contingentism" and "inevitabilism," figures centrally. I also discuss several authors who were retrospectively cast as interlocutors in the debate, such as Andrew Pickering, Sheldon Glashow, and James Cushing, and those who responded to Hacking directly, namely Léna Soler and Howard Sankey. After demonstrating how these authors and their conceptions of contingency have defined the debate, I argue that the conversation wants for a clear understanding of contingency, and identify some places where this ambiguity can be clarified by more rigorous classification of the concepts it groups together.

The third section presents a detailed discussion of the nature of contingency in science, in which I outline a fresh taxonomy of the concept. The taxonomy builds on John Beatty's distinction between "contingent *per se*," or unpredictability contingency, and "contingent upon," or causal dependence contingency. This distinction clarifies the debate substantially, but I argue that a second step is required. Further decomposing unpredictability contingency and classifying causal dependence contingency—based on the things within science considered to be contingent and the factors they are presumed to be contingent upon—allows more precise and faithful characterization of the views under discussion. Clarifying the debate in this way demonstrates that inevitability is inferior to a new category—predication—as a contrary of contingency. A detailed picture of ways different authors use contingency serves as a basis from which to examine how a nuanced account of the concept can clarify some persistent ambiguities in the C/I debate.

II. Contingency and Inevitability

Ian Hacking coined the terms “contingentism” and “inevitabilism” in *The Social Construction of What?*, the same book in which he hinted that contingency might be understood as a spectrum. Contingency appears as a feature of his effort to understand the philosophical stakes of social constructionism. Hacking casts contingency as one of three sticking points between constructionists and their opponents.¹ He identifies the constructionist program, whether applied to science or SARS, as seeking to undermine claims about the inevitability of ideas.² When generalized, according to Hacking, the constructionist argument takes the form “X need not have existed, or need not be at all as it is, is not determined by the nature of things; it is not inevitable.” It quite often proceeds to two other more advanced stages, which contend a) that X is bad in its current form, and therefore b) should be eliminated or radically altered (Hacking 1999, p. 6). The constructionist program meets, on Hacking’s account, irreconcilable opposition from inevitabilists when it claims that the results of scientific investigation are contingent, and therefore unconstrained by the intrinsic structure and properties of the natural world.

Andrew Pickering, author of 1995’s controversial *Constructing Quarks*, is Hacking’s paradigm contingentist. Pickering advanced the view that the development of high energy physics’ Standard Model resulted from an exegesis of data, which could have been produced any one of numerous, ontologically incompatible interpretations. He

¹ The two other sticking points are nominalism and the source of theoretical stability. Constructionists deny that nature has an intrinsic structure that can be discovered and point to factors external to scientific theory
² SARS, which stands for Severe Acute Respiratory Syndrome, was the subject of a wave of global concern following an outbreak in China in 2002. *The Social Construction of SARS* is in actual title (Powers and Xiao 2008) found by using Hacking’s method of scanning the results of a search for “Social Construction of” in the library catalog (Hacking 1999, p. 1).

concludes that physics might have escaped the twentieth century quark free, and that if it had, it would not be any less successful (Pickering 1984). Hacking charitably interprets this argument in light of later work, especially *The Mangle of Practice* (Pickering 1995), wherein Pickering argued that scientific consensus arises from negotiation between theory applied to the world, theory applied to instruments, and the construction of the instruments themselves to develop a robust fit with observed data. The results of science are contingent from this perspective because the negotiation could supposedly be carried out in any number of ways, each resulting in the same degree of success as defined by scientists themselves. For Pickering, the punch line is that twentieth-century physics could have been just as successful if, for example, cyclotrons had not supplanted traditional cloud-chamber technology and the resulting theory of the micro-world therefore had not been dominated by quarks, which he contends are the peculiar progeny of the particle accelerator.

Hacking elaborates on the inevitabilist stance Pickering opposes in “How Inevitable Are the Results of Successful Science?,” writing: “We ask: *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?* If so, there is a significant sense in which the results are inevitable” (Hacking 2000, p. 61). Pickering would answer in the negative, denying that equal success implies equivalence of any sort. But what of the inevitabilist? Hacking casts Sheldon Glashow as arch inevitabilist. Glashow tenaciously holds that any investigation into the natural world starting from reasonable initial assumptions would turn up not only the same answers, but also a

similar set of questions to ask. Glashow imagines intelligent aliens as hypothetical scientists whose physical laws should be isomorphic with ours. In doing so, Hacking charges, Glashow tacitly makes crucial assumptions about the “reasonable” initial assumptions that are necessary before alien science will produce the same results. How do we know, for example, that aliens would identify proton structure as an interesting question? Hacking segues from Glashow into the difficulties with strong inevitability claims: how stringently can you set the initial conditions before the argument dissolves into tautology? If the inevitabilist asserts that a successful scientific enterprise will produce the same results as ours by stipulating that success requires the competing scientists to ask the same questions, use the same instruments to observe the same entities, all while starting from the same assumptions, then the assertion dissolves into trivial observation that effectively identical scientific investigations produce effectively identical results (*ibid.* p. 66).

Pickering and Glashow both represent extreme views; Hacking seeks a middle road between them. Pickering’s radical contingentism undermines science’s epistemic authority altogether on the basis of the counterfactual claim that alternate scientific trajectories could have been just as successful. On the other extreme, Glashow’s radical inevitabilism collapses into triviality. Hacking’s compromise locates contingency at the level of the questions scientists ask. It is contingent, he argues, which questions are “live.” By “live questions” he means questions that make sense within the theoretical framework provided by the science. Once science satisfactorily answers one of those questions we can take that result to be inevitable in some meaningful sense, but we have

no guarantee that it would have been asked in the first place.³ Contingency, for Hacking, enters into science by allowing historical and socio-cultural factors to define what questions scientists find interesting and what questions they are allowed to ask. These questions are not necessarily answerable, and they might not make sense in any theory-independent sense, but once nature proves forthcoming with an answer, that answer can be said to have the tinge of inevitability. Nothing guarantees that anyone would ever have asked the question, but since they did, and since it was adequately answered, it is no longer contingent. Science could have developed differently, but only because it could have turned its attention to a different set of questions. Possible alternate results are never logically incompatible with current successful science (*ibid*, p. 71).

When introducing the distinction between contingency and inevitability, Hacking scrupulously observes the debate's independence from the realism/anti-realism issue: "the contingency thesis itself is perfectly consistent with [...] scientific realism, and indeed anti-realists [...] might dislike the contingency thesis wholeheartedly," (Hacking 1999, p. 80). Howard Sankey (2008) also argues for the independence of C/I from realism/anti-realism.⁴ He defends weak fallibilism, consistent with an inevitabilist viewpoint. He holds that individual results of science are contingent—individual instances of scientific investigation are fallible—but we can be confident in the long term that these local contingencies will be washed out by statistically inevitabilist tendencies.

³ Hacking does not offer an account of just how scientists can determine when a live question has been adequately answered, an issue that is not unproblematic (Cf. Galison 1987).

⁴ Giere (2010), while acknowledging the independence of these debates, makes a first attempt to codify their relationship by exploring inevitability as it relates to realism. In doing so, he proposes his own rough classification of the varieties of contingency.

Sankey defends the compatibility of his fallibilist stance with a contingency thesis, which he says is an epistemic claim about scientific practice and the way investigators engage with the world: “Scientist might collect different evidence from the evidence they in fact do collect. They might have developed different instruments and techniques from the ones which have been developed and put to use” (Sankey 2008, p. 259). A geological example discussing the discovery of continental drift illustrates his point: “The epistemic situation is [...] dependent on contingent factors such as the availability of evidence and relevant knowledge, the development of instrumentation and the provision of research funding” (*ibid.*, p. 262). Sankey’s use of contingency differs from both Pickering’s and Hacking’s. Pickering would certainly not contest that the factors Sankey identifies are contingent, but he would compile a list of additional contingencies much longer than Sankey would admit. Hacking argues for contingency of form rather than content of science: difference without incompatibility. Sankey, in contrast, points to the empirical content of science as contingent. These perspectives are not incompatible, but they have different emphases—Sankey focuses on evidence, Hacking on inquiry.

Sankey subtly contrasts James Cushing, who argues that contingency has “an essential and ineliminable role in the construction and selection of a successful scientific theory from among its observationally equivalent and unrefuted competitors” (Cushing 1994, p. xi). Cushing’s use of the word “theory” is equivocal, as the example on which his book is based is the choice between Bohr’s and Bohm’s interpretations of quantum mechanics, which can be construed as competing window dressings of the theory of

quantum mechanics rather than as theories themselves. Quibbling aside, Cushing's argument is that choices between observationally equivalent theories are historically contingent. He does not claim that such choices are irrational, but rather that they are guided by philosophical and other external criteria rather than data. In the case of Bohm versus Bohr, the interpretive question hinges on whether one abandons strict determinacy or strict locality in the quantum realm. Experimental evidence suggests that either particles in quantum states, obeying the probabilities assigned by their wave functions, assume classically observable values for their key properties—charge, spin etc.—during an observation event, or some “hidden variables” determine these properties, but instantaneous signaling across finite distances is permitted. The first violates a deeply ingrained philosophical preference for deterministic processes in physics, while the second runs afoul of a long tradition of skepticism about instantaneous action at a distance. Cushing's view, exemplified by the claim that the Bohmian view's defeat at the hands of Bohr's Copenhagen interpretation was contingent, involves no change in the empirical content of the theories in question. Nor does Cushing's contingency act on the data collection process—the crux of Sankey's argument.

Most of those who have employed the contingency concept in their work, as described above, do so in pursuit of goals other than defining it. Sankey, for instance, wants to show the independence of the C/I debate from discussions of realism. Léna Soler identifies this argument as a premature, writing: “the ‘contingentism versus inevitabilism’ contrast does not exist as an autonomous, well identified issue of significance,” (Soler 2008b, p. 232). On the basis of this ambiguity she sets out to clarify

the issue, employing a thought experiment involving two, isolated communities of physicists, starting with the same initial conditions, but asking their own questions, unguided by the work of the other scientists:

Human beings might have succeeded in developing a physics as successful and progressive as ours, and yet asked completely different physical questions from the ones that have actually been asked, with the result that the accepted answers—in other words the content of the accepted physical theories and experimentally established physical facts—would be at the same time robust and different from ours (*ibid.*, p. 232).⁵

Any non-trivial contingency, Soler argues, requires that two isolated scientific communities starting from the same point produce “irreducibly different” results, while still satisfying a reasonable set of criteria for success (*ibid.*).

The task of empirically determining whether or not two scientific programs are reducible is not straightforward; however, the type of contingency Soler identifies as meaningful involves deep and irreconcilable oppositions between competing physical theories. Furthermore, given the constancy of the initial conditions in Soler’s thought experiment, it tests only whether science is contingent irrespective of the initial conditions, and does not consider to what extent science might be contingent *upon* antecedent conditions.⁶ Soler’s thought experiment tests only whether science is inherently contingent. It does not assess the relative contributions of contingency to the collection of internal and external factors that influence the trajectory of science.

Each author mentioned here attacks the question of how science might be contingent. In doing so, each employs a different understanding of what it means for

⁵ Soler offers three scenarios on which science can be understood to be contingent, but only address this one as offering anything of potential interest.

⁶ Here is an implicit distinction between “contingent *per se*” and “contingent upon,” which comes from Beatty (2006). See Section III below for a more thoroughgoing discussion of this distinction.

science to be contingent, and at what point that becomes a meaningful claim. These authors cast contingency in a qualitatively different ways rather than with differing intensities, indicating that they represent diversity of kind and not of degree. To review:

Hacking: *It is contingent what questions scientists decide are interesting.*

Pickering: *It is contingent what ontological entities scientists claim to find in the natural world.*

Glashow: *The theoretical structure of science is **not** contingent.*

Sankey: *It is contingent what instruments and techniques are available to scientists.*

Cushing: *It is contingent how scientists arbitrate between empirically equivalent theories.*

Soler: *Science is contingent only if it has available at least two equally successful, but irreducibly different paths from any given starting point*

These authors clearly differ on how contingency operates in the historical process, but a smooth scale of contingentism cannot capture that difference, even superficially. The next section describes the ways in which these authors have understood contingency and systematizes the diversity of views sheltered within the concept.

III. Taxonomizing Contingency

A. A Preliminary Distinction

Contingency, as the discussion above demonstrates, is a wildly diverse concept. How can we refine our understanding of contingency so it can be applied with less

ambiguity? John Beatty offers one crucial distinction. In “Replaying Life’s Tape” he distinguishes between “contingent *per se*” and “contingent upon” (Beatty 2006). “*Per se*” contingency describes stochasticity in the historical process; it implies that the process of history itself is *unpredictable*. In contrast, “upon” contingency requires no unpredictability, but rather describes a historical process that is far from robust with respect initial conditions, indicating that outcomes have a measure of *causal dependence* on the relevant antecedent factors. Any change in initial conditions could lead to a drastically different outcome, even if the outcome of the process is, in principle, predictable from any given set of initial conditions.

In drawing this distinction, Beatty invokes Stephen J. Gould’s thought experiment from *Wonderful Life* (Gould 1989): start the story of life on Earth over from the Cambrian explosion, and ask if “replaying the tape” in this way directs the history of life down a different path. Gould argues that evolution is highly contingent, and the rerun would differ dramatically from the initial broadcast. Throughout the book, as Beatty observes, Gould alternates between the unpredictability and causal dependence senses of contingency. Beatty argues that these two conceptions are compatible, but, taken on their own, have different consequences for our understanding of the historical process.

How should recognizing the distinction between these two varieties of contingency inform the C/I debate? Take Pickering’s contingentism: his original claim in *Constructing Quarks* that physics might just as easily have proceeded in a direction that did not include quarks is an unpredictability claim about scientific knowledge. He holds there that scientific knowledge is contingent *per se*. His view, as reinterpreted by

Hacking in light of *The Mangle of Practice*, is an “upon” contingency claim. If the response to new data is a negotiation between existing theories, auxiliary theories about instruments, and the instruments themselves, then the consequent theory is contingent upon each of those three factors. In the second version of the argument, Pickering’s stance gets its constructionist bite from the factors it identifies as causally relevant rather than from the unpredictability of the scientific process.⁷

Hacking, Soler, and Sankey, all observe that even the strongest of inevitabilist is willing to admit that a benign form of historical contingency shapes the course of scientific knowledge. The Bragg family might have gone into sheep shearing rather than physics, and the resulting disturbance in the development of x-ray crystallography would quite likely have substantially altered the story of the discovery of DNA’s structure. The Cold War might have dragged on a few years longer, the United States Congress might have been friendlier towards basic research expenditures as a result, the Superconducting Super Collider might have been built after all, and high energy physicists might not be looking for the Higgs boson any longer.⁸ In Beatty’s language, inevitabilists are happy with the claim that scientific knowledge is contingent *upon* some historical factors, while denying the stronger claim that it is contingent *per se*.

Beatty’s distinction goes a long way towards clarifying the disagreement between inevitabilists and contingentists. They do not disagree about *the extent to which* scientific knowledge is contingent; they disagree about *what kind of* contingency influences the

⁷ The second version of Pickering’s claim could, of course, be paired with an unpredictability element; to wit, it is unpredictable how the negotiation between the three causal factors will be resolved. In this context, however, I have simplified the matter to provide a better illustration of Beatty’s distinction.

⁸ I defended this thesis a day after a CERN announcement indicating possible hints of a Higgs at ~124-126 GeV. This analogy might therefor soon become obsolete, and should be read from a pre-2011 perspective.

scientific process. Contingentists, as described by Hacking, admit both unpredictability and causal dependence contingency, while inevitabilists see no trouble from some types of causal dependence contingency, but draw the line at its more consequential sibling. This distinction does not exhaust the possible positions in the contingency debate, however. It demonstrates that Hacking's method of rating contingency on a one-to-five scale inadequately describes the commitments involved, but it only begins to capture the full range of contingency claims available. Those who allow causal dependence contingency might have reasonable disagreements both about what aspects of science are subject to contingency claims, and what science can be reasonably said to be contingent upon.

B. Reframing the Debate: A New Contrary to Contingency

Distinguishing between unpredictability and causal dependence is useful for understanding contingency, but the distinction is less transparent in the case of inevitability. We are grammatically prohibited from referring to events as “inevitable upon,” leaving the rather awkward distinction between “inevitable period” and “inevitable given the following initial conditions....” The contrast to the elegance and utility of the upon/*per se* distinction suggests that inevitability is not contingency's most appropriate contrary. Any term describing a position contrasting contingency should capture the full range of ways that contrast might be made. Inevitability, with its absolute connotations, effectively contrasts only the unpredictability form of contingency, making it difficult to describe inevitabilist positions with the same degree of specificity as contingentist positions.

In order to capture both of the senses Beatty identifies, an alternative to inevitability should contrast predictability on one hand and causal dependence—in a somewhat different sense from contingent causal dependence—on the other. I suggest “predication” as a more accurate characterization of the range of views opposing contingency.⁹ This category implies a similar range of beliefs and addresses both halves of Beatty’s distinction. On one hand the predicatist might believe that science is predicated upon (predictable from) specific antecedent conditions. On the other, she might contend the results of science are predications in the logical sense: consequences that are, *per se*, self-evident.

Comparing contingency to predication make it clearer that the two, in the full range of their meanings, are more complementary than opposite. Hacking aimed, in *The Social Construction of What?*, to explain the core disagreements driving the science wars. Naturally, therefore, he created categories that emphasized the differences between two seemingly irreconcilable points of view. In the years since 1999, the weapons used to wage the science wars have grown rusty and trenches demarcating territorial lines have begun to fill. Demobilization has demonstrated the need for categories capturing a more nuanced range of perspectives. The category of predicatism does that for contingency debates. The belief that the results of science are strongly predicated upon a set of factors implies a corollary that other factors are contingent. All but strict Laplacian determinists will put some factors of science, even if only the trivial ones, in each of these categories. The task of characterizing the contingentist/inevitalist debate (or, in my formulation, the contingentist/predicatist debate), then stops being an exercise in characterizing

⁹ I also considered “consequence,” but moral philosophers have already claimed “consequentialism.”

opposing positions, and becomes an effort to map the range of ways available to distribute the multifarious aspects of science onto two orthogonal axes. Predicatism is the inverse, rather than the opposite, of contingentism.

C. Towards a Taxonomy of Contingency

The *per se*/upon distinction is a necessary first step towards classifying the full range of meanings contingency might assume. As Beatty aptly demonstrated, the unpredictability and causal dependence senses of the word imply dramatically different pictures of the historical process. Each of them can also be broken down further. This section proposes a taxonomy of contingency, the aim of which is to capture and classify the range of ways in which it can be understood.

i. Unpredictability Contingency

First, consider unpredictability contingency. Beatty defines it as the belief that “the occurrence of a particular prior state is *insufficient* to bring about a particular outcome,” (Beatty 2006, p. 339). It appears that the unpredictability contingentist makes a strong metaphysical claim about the historical process: it is indeterministic. Indeed, Gould does appear to be making such indeterminacy claims in *Wonderful Life*. Should we replay the exact same tape of life from the exact same initial conditions and get a different result, then indeed the process by which life develops is not wholly determined by initial conditions and contains some intrinsic stochasticity.

Metaphysical indeterminacy is not the only way in which “*per se*” contingency can be understood. Beatty also observes that contingency is the lynchpin of Gould’s argument that selection should not be the only causal agent evolutionary biologists invoke to explain the features and behaviors of present-day organisms (see Gould and Lewontin 1979). This suggests that unpredictability, as applied to contingency, can be understood as a methodological as well as a metaphysically argument. This weaker understanding would suggest that outcomes are contingent (*per se*) *with respect to* some specified set of causal factors. It does not rule out the ability of other causal factors to provide an exhaustive, deterministic explanation. In fact, it often suggests such factors. Such is Gould’s case against what he calls pan-selectionism—the assumption that natural selection can be invoked to explain any feature of an organism. The weaker version of unpredictability contingency he employs suggests that the features and behaviors of organisms are contingent (unpredictable) *with respect to* selection effects. Such a view is consistent with a fully deterministic universe; it merely implies that factors other than selection are partially responsible for the survival, extinction, and evolution of species.

Unpredictability contingency comes in both strong and weak versions. The strong version, which we might call metaphysical contingency, requires an indeterministic historical process. The weak version, which can be termed incompleteness contingency, claims that some set of causal factors is inadequate offer a complete explanation of the historical process, and that outcomes are therefore unpredictable with respect to that set of factors.¹⁰ These two forms of *per se* contingency do different types of philosophical

¹⁰ “Epistemic contingency” might seem like the natural contrast to metaphysical contingency, especially considering the discussion above; however, that would belie the fact that the two are not actually directly

and rhetorical work. Metaphysical contingency, as the name would imply, says something about how the world is. Incompleteness contingency functions more subtly. It targets some specific set of explanatory tools as inadequate. As such, it depends on the state of scientific practice. For an incompleteness claim to make sense, it must refer to an established explanatory orthodoxy.

Incompleteness claims, betraying the fact that they are not entirely epistemic, also refer to the causal forces at work in the world. The reference is oblique, suggesting the impotence of some factors rather than arguing for the potency of others. Nonetheless, the suggestion that a phenomenon is incompletely explained by some causes suggests that it must be partly explained by others. Incompleteness can be understood as an epistemic argument on one hand and a complexity argument on the other: we cannot explain all we might like to explain about x by appeal established causes, therefore the causal history of x is more complicated than has been previously supposed.

Where does predication fit in? It can offer a contrast to each of these categories. Metaphysical *per se* predicatism amounts to an argument for metaphysical determinism; if outcomes are metaphysically predicated, then deterministic causes are wholly sufficient to produce them. Completeness predicatism would encompass the methodological claim that existing causal explanatory factors are adequate to account for a given phenomenon. An opponent of Gould's, for instance, might insist that selection alone does offer an adequate causal account any organism's features. Expounding predicatism in this way might appear superfluous, but it does indicate its superiority to inevitabilism as a way to

contrasting in the way they are used. While insufficiency contingency does primarily drive epistemological and methodological arguments, it also says something about what causal forces are at work (or are not at work) in the world, and so is not strictly confined to the epistemic realm.

characterize contingentists' opponents. Inevitability can easily capture the concept of metaphysical determinism, but the concept strains somewhat if asked to describe the methodological position.

ii. Causal-Dependence Contingency

Turning to causal dependence further emphasizes that the predicatist stance should not be saddled with the onerous term "inevitable." Although predicatists are likely to admit some causal dependence contingency claims about science, it is not a difficult task to formulate a causal dependence claim that is repugnant to the predicatist. Consider the claim that the Standard Model of particle physics is contingent upon the detailed structure of particle accelerators, or that the emergence of unobservable entities as explanations for natural phenomena is contingent upon a philosophical tradition of reductionism, and is not uniquely motivated by the data. These are types of claims it is easy to imagine a contingentist in the Pickering mold making.

What should differentiate types of causal dependence contingency? The case is more complicated than the case of unpredictability, because the objects of "upon" might be expounded *ad nauseam*. The first step towards classifying this variety of contingency is therefore identifying suitably distinct parts of science that might be held contingent. Science, like contingency, is heterogeneous and the claim that science is contingent can mean drastically different things depending on what parts of science that claim

specifies.¹¹ Science makes ontological claims, formulates methodological procedures, develops models, adopts interpretations, and builds communities. Causal dependence contingency can be initially differentiated based on which of these many aspects of science are claimed contingent. I propose five categories:

- (1) *Trivial contingency* – Science, as a human activity, is part of a historical process, and so is contingent in the same way human history is contingent. This weak claim covers individual scientists and the details of their everyday existences.

This is the most general, least intrusive form, and is similar to Soler’s benign contingency (2008b). All non-Laplacian parties are happy to admit this form of contingency. A claim that science is contingent in the trivial sense, however, offers the hard-boiled contingentist little succor. Trivial contingency is agnostic about the aspects of science that are typically of interest to philosophers, and so has little bearing on the debate. This type of contingency is frequently invoked to argue that contingency need not be wholly repugnant to the sophisticated predicatist. Sankey, for instance, argues that continental drift did not gain traction within the geology community until the 1950s and 1960s, when the U.S. Department of Naval Research began funding ocean floor research to bolster its submarine program (Sankey 2008, p. 262). Naturally, if the research had not been funded, and had not been conducted, the trajectory taken by the science would have been different, but this does bear on the claim that successful science should pass through

¹¹ Because predication is presented here as the inverse rather than the opposite of contingency the reader can substitute “predication” and its forms for “contingency” and its forms in order to create a corollary form of the argument in this section. In this case: “Science, like predication, is a heterogeneous concept, and the claim that science is predicate can mean drastically different things depending on what parts of science that claim specifies.”

stages resembling ours. Trivial contingency alters the route science takes, but remains silent about its destination.

(2) *Sociocultural contingency* – The social structures that constitute scientific activity and science’s interaction with culture are contingent.

At first glance this slightly stronger form of trivial contingency might seem similarly innocuous. Like trivial contingency, it is agnostic about the content of science, instead bearing on scientific institutions, disciplines, communities, political relationships, and laboratory cultures. It is more complicated than trivial contingency, however, because it is the sticking point where some strong contingentists dig in their heels. Forms of contingency that cut closer to the bone (see below) often rest on various degrees of social determinism. So a contingentist claiming that theoretical entities are contingent upon (causally determined by) social structures might want to deny that those social structures are themselves contingent. Similarly, predicatists might flinch when sociocultural contingency is used in conjunction with a stronger form, as in, for example, the controversial Forman thesis, which asserts that quantum indeterminacy was contingent upon the distinctive social conditions of the Weimar Republic (Forman 1971).

(3) *Methodological contingency* – The way in which we do science might have been different. This moderately weak variety of contingency holds experimental and theoretical techniques, laboratory practice, instruments, apparatus, and heuristic devices contingent.

Contingency claims frequently target the way science functions. Sankey approximates this version of contingency when he describes evidence collection and

instrument application as sources of contingency in science and claims that the development of plate tectonics could only come about when specific instrumentation came to be in common use (Sankey 2008). Many historical studies have examined how tool selection influences the way theories develop. The literature on model organisms is an obvious example, representative of which is Robert Kohler's *Lords of the Fly*, which argues that the choice of *drosophila melanogaster* as the model organism for experimental genetics shaped the field's development (Kohler 1994). Experimental apparatus influences what data is collected, how that data is packaged, and which parts of it appear most important, while the available mathematics, heuristics, and analogies guide how that data is analyzed. This type of contingency is not trivial, but it does not necessarily imply incompatibilities between existing science and science that might have proceeded with different experimental or analytical tools. As with sociocultural contingency it can be combined with more potent forms.

- (4) *Interpretive contingency* – The way in which we interpret data in order to fill gaps in theories is contingent.

Interpretive contingency relies on the necessity of interpretation for understanding theoretical implications. Data, even if they motivate a particular theory, often do not compel one interpretation of that theory. Cushing's claim about the contingency of the Copenhagen interpretation is the obvious example. Quantum mechanics allows multiple logically consistent interpretations of what *actually* happens when quantum systems are observed. Building a satisfying ontological explanation requires physicists to interpret measurements that, by the very nature of the theory, do not provide the whole story.

Given this necessary appeal to factors other than data, the interpretation we choose is contingent upon the context in which the theory emerges, and an alternate interpretation might well have emerged given different conditions (Cushing 1995).

Hacking's formulation of contingency, which distinguishes between the form and content of scientific activity, encompasses both this and methodological contingency. Unlike methodological, sociocultural, or trivial contingency, however, interpretive contingency can directly imply deep incompatibilities between two possible sciences, as in Cushing's example, in which Bohm's interpretation is deterministic and non-local while the Copenhagen interpretation is indeterministic and local.

(5) *Theoretical contingency* – This is the strongest form of contingency. In the constructionist mold, it holds that scientific theories themselves and the claims they make about the world, are contingent.

This form postulates the possibility of deep incompatibility between two possible scientific trajectories, regardless of the initial parameters. While theoretical contingency can be parsed in “upon” syntax, it approximates a *per se* contingency claim. The main difference between theoretical contingency and the in-principle unpredictability of scientific results is the frequent postulation by advocates of this claim of a causal arrow from specific historical or cultural factors to theories. Forman's argument that cultural instability in the Weimar republic compelled physicists to accept indeterminacy, for instance, makes quantum mechanics' ontological claims contingent upon the Weimar cultural environment (Forman 1971). This is not quite the same as describing science as unpredictable or undetermined, but the factors on which it is contingent make the claim

equivalent with the incompleteness contingency claim that science is unpredictable from internal factors alone. The *per se* claim and the theoretical contingency claim often go hand in hand, as the argument often holds that theoretical contingency works *because* theory is either almost infinitely malleable (metaphysical), and/or subject to external pressures (incompleteness).

It might appear that this constitutes a spectrum, especially given a description beginning with “trivial” and graduating into increasingly more serious contingency claims, but the relationships between the elements are not so straightforward. Trivial contingency plainly does not require a commitment to any of the other four, and theoretical contingency often implies several of the others *a fortiori* (a notable exception being the case in which it resists sociocultural contingency), but the middle-of-the-road contingency claims cannot be so easily ranked. It would be perfectly consistent to hold an inevitabilist stance about methodology, arguing that mature science motivates an optimal form of investigation and modeling, while maintaining the contingency of interpretation. Similarly, it would be just as consistent to be predicatist about interpretation while contingentist about methodology. These examples should make it clear why contingency is a “what sort” question as opposed to a “how much” question. If I claim that one part of the scientific process is contingent while holding that another is not, that alone does not make me more or less contingent than I would be if I held the inverse view.

The categories above provide only half the picture. To complete the taxonomy a second layer is required. Distinctions based on what parts of science are contingent are critical, but we can also, invoking Beatty, draw further distinctions based on what they

consider those factors to be contingent upon. Thus, while two people might agree that the methodological components of science are contingent, they might also disagree substantively about the factors upon which methodology is contingent. The factors upon which science, in all its aspects, might be contingent map onto the aspects that can themselves be held contingent: everyday events, sociocultural contexts, methods, interpretations, theories.

The resulting taxonomy can be graphically represented in two dimensions:

		Upon				
		Everyday events	Soc-cult. context	Methods	Interpretations	Theories
The elements of science are contingent	Everyday					
	Social					
	Methodological					
	Interpretive					
	Theoretical					

Table 1 – Range of available causal-dependence contingentist positions. The full range is indicated by the cross-section of contingent factors and contingent causal factors.

We might also construct a similar chart for predicatism, with a “Nature” column added at the far right:

		Upon					
		Everyday events	Soc-cult. context	Methods	Interpretations	Theories	Nature
The elements of science are predicated	Everyday						
	Social						
	Methodological						
	Interpretive						
	Theoretical						

Table 2 – Range of available causal-dependence predicatist positions. The range can be visualized in the same way as contingency positions.

Tables 1 and 2, showing the size of the phase space available under the umbrella category of causal-dependence contingency and predication claims, make it clear why two positions both espousing contingentism can seem dramatically different, and often incompatible. Painting with a broad brush might lead one to argue that the science wars were about whether the left side or right side of this chart does more of the causal work in

science. That broad characterization glosses over much of the nuance in between those two extremes. Under this framework, it no longer makes sense to talk about positions as “contingentist” or “inevitalist.” Rather, it implies a rich cornucopia of views that might have numerous points of agreement and disagreement. To understand an individual position we need to know, first, what aspects of science it holds contingent, and second, what it holds them contingent upon.

Showing both charts separately illustrates a critical distinction between contingentism and predicatism. If causal dependence contingency identifies the antecedent factors causally responsible for certain outcomes, is it really any different from predication, which also identifies the antecedent factors causally responsible for certain outcomes? Yes. In a contingency relation the causal factors identified are themselves contingent. If interpretations are contingent upon social contexts, then the possibility must remain that the social context in question need not have been as they were. In contrast, a predication relation assumes the opposite. When Glashow claims that physical theories are predicated on nature, he takes nature as a fixed reference point that could not be otherwise. For simplicity’s sake, a combined chart might look like this:

		Upon					
		Everyday events	Soc-cult. context	Methods	Interpretations	Theories	Nature
The _____ elements of science are [contingent/ predicated]	Everyday						
	Social						
	Methodological						
	Interpretive						
	Theoretical						

Table 3 – Combined causal-dependence contingentist and predicatist positions. This chart can be filled in, with either “contingent,” “predicated,” or “X” (indicating no causal relationship) in each box, to exhaustively describe any one position among the array of available stances on contingency and predication.

We now have the tools to clear up some of the stickier ambiguities in contingency debates. No author has provided a single account complete enough to fill in the whole

table, but we can isolate the character of particular stances and disagreements. Take for an example the issue of strong social determinists who admit theoretical contingency but deny social contingency. They would be described as arguing that theories are *predicated* upon social contexts, rather than *contingent* upon them. In defiance of the expectations established by Hacking's equation of contingency with constructionism, it turns out that some of the strongest constructionists are not contingentist at all about the theory-society relation, further illustrating that aligning contingency and predication with distinct ideological camps underemphasizes nuances central to the debate.

Consider, as a further example, Glashow versus Pickering on the status of the Standard Model. Glashow holds that theories are predicated upon nature. Pickering (in his more sophisticated form described by Hacking) claims that the theoretical elements of science are contingent upon sociocultural context, methods, and pre-existing theories. Some interesting consequences emerge. For instance, Pickering remains silent about any purported relationship between theories and nature. We learn that although Pickering and Glashow appear diametrically opposed their perspectives actually encompass slightly different issues: they do not directly disagree on any specific square of the table, even if they do represent two dramatically different perspectives. Such an analysis does not, of course, suppose that their perspectives might therefore be easily reconciled. While Glashow and Pickering do not actively disagree in the array of causal dependence contingency, they do in the realm of unpredictability. Pickering's stance includes an insufficiency claim crafted in the context of 1980s history of science: natural and

theoretical factors do not, on their own, allow us to explain the course of theoretical development. Glashow would be a predicatist on this count.

John Henry, in “Ideology, Inevitability, and the Scientific Revolution,” directly addresses contingency as it regards the constructionist/positivist divide, affording the opportunity to apply this taxonomy in more detail. Henry argues that positivists and social constructionists are not so different after all, on the basis of how the two groups might be expected to treat contingency claims. For the positivist, Henry claims, the development of science can be expected to be inevitable because it is constrained by brute facts about the real world. As Henry reconstructs the positivist’s view:

[T]he history of science is the story of a gradual but inevitable progression towards the truth of how things are. Since there have been many wrong turnings, blind alley’s and unnecessarily circuitous routes taken along the way, there is no real reason to suppose that a diversion down a counterfactual blind alley, or into a counterfactual tradition of thought or practice [...] would halt, or significantly divert, the inexorable progress of science” (Henry 2008, pp. 553–554).

We can expect that contingency analysis will provide only superficial alternative histories for the positivist, for whom the content of science is guaranteed by the nature of the world under investigation.

Henry is similarly pessimistic about the prospect that contingency might do real work for the constructionist. If the constructionist points to social factors to explain how science develops, he argues, substantial changes in social structures would be necessary to produce a substantially different science. In Henry’s words, “the social constructionist historian of science wants to offer an account that is seen as causal; and, given that the account is couched not in terms of a single stroke of genius but 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” (*ibid.*, p. 556). The constructionist, under this view, has freed science from natural determinism only to subject it to social determinism, leaving contingency as impotent it was for the positivist.

The punch-line is that “the recent emphasis in the historiography of science on the social milieu rather than on the roles of individual thinkers [...] has unwittingly provided support for positivistic approaches to science and its history” (*ibid.*) Henry’s strategy for undermining this “unholy alliance” (*ibid.*, p. 557) is to point out a subtle difference in the nature of contingency in each framework: “Whereas the positivist presumably rejects the significance of contingencies in principle, and would wish to suggest that no counterfactuals would make any significant difference to the development of science, the social constructivist historian objects only in practice” (*ibid.*). That this difference has little or no impact for how the two groups view the outcomes of the scientific process, though, belies Henry’s contention that it is “a very important distinction” (*ibid.*). The principle/practice distinction might be an important shibboleth, but it is not significantly forceful to keep Henry from reaching the conclusion that: “We cannot [...] hold that small changes in the actual history of science would have made a difference without simultaneously invalidating the historiography of science of the past half century or so. To do so would be to suggest that the wider social milieu of the sciences is not really a significant factor in understanding their development” (*ibid.*, p. 558).

Applying insights from the discussion above can disentangle problematic points in Henry’s discussion. His contention that the distinction between in-principle and in-practice contingency is the main axis on which the positivist and the constructionist differ

ignores the fact that each is willing to hold very different aspects of science contingent or predicate. The constructionist is perfectly willing to admit theoretical contingency, for instance, where the positivist is not. This amounts to a drastic difference in the causal factors that each holds to be relevant to the development of science and means that the inevitability each group confronts is dramatically different in character from the other's.

This point can be elucidated by employing the chart above. Here, we see Henry's exemplar positivist and constructionist compared based how each would carve up science into contingent and predicate parts:

Positivist

		Upon					
		Everyday events	Soc-cult. context	Methods	Interpretations	Theories	Nature
The _____ elements of science are [contingent/predicated]	Everyday	(Trivial)	Contingent	Contingent	Predicated	Predicated	Predicated
	Social	Contingent	(Trivial)	Contingent	Predicated	Predicated	Predicated
	Methodological	Contingent	Contingent	(Trivial)	Predicated	Predicated	Predicated
	Interpretive	X	X	X	(Trivial)	Predicated	Predicated
	Theoretical	X	X	X	X	(Trivial)	Predicated

Constructionist

		Upon					
		Everyday events	Soc-cult. context	Methods	Interpretations	Theories	Nature
The _____ elements of science are [contingent/predicated]	Everyday	(Trivial)	Predicated	Contingent	Contingent	Contingent	X
	Social	X	(Trivial)	X	X	X	X
	Methodological	X	Predicated	(Trivial)	Contingent	Contingent	X
	Interpretive	X	Predicated	Contingent	(Trivial)	Contingent	X
	Theoretical	X	Predicated	Contingent	Contingent	(Trivial)	X

Table 4 – Archetypical positivist and constructionist. Their views are represented in the framework developed here. The comparison highlights differences, similarities, and orthogonalities.

Henry argues that both groups would inevitabilist because each allows predicate zones and neither holds the theoretical elements of science to be contingent upon everyday events, a point upon which positivists and constructionists do agree. He then differentiates them because that one specific contingency fails for different reasons in

each case. This narrow characterization fails to capture the rest of the broad contingency spectrum, in which the positivist and constructionist have points of agreement, points of disagreement, and differences in emphasis. Henry is correct to point out that both perspectives imply predicate elements. This should hardly be a surprise if we consider that contingency and predication may be assessed separately for different parts of the scientific process, and it should not lead us to conclude that two predicatist views arrive at the same fundamental conclusion.

Let us return briefly to Hacking and his primary motivation in *The Social Construction of What?* Hacking sought a description of what was at stake in the science wars, and described contingency and inevitability as one place where constructionists and their opponents differed. The taxonomy presented above allows us to state the disagreement more sharply. Constructionists (like Pickering) and positivists (like Glashow) identify different but not diametrically opposed factors upon which science is both contingent and predicated. They lock horns directly over the question of insufficiency. The disagreement, in short, hinges most crucially on what causal factors the historian must invoke to provide an adequate account of scientific development.

IV. Summary

I have argued that the purported debate between contingentists and inevitabilists can be decomposed into an array of positions that directly oppose one another only over a small range of their total implications. Decomposing several views articulated in the literature makes it evident that predication, rather than inevitability, offers a more

appropriate counterpart to contingency. Within the framework provided by Beatty, I have further decomposed the concept of contingency into seven types, two under the heading of unpredictability and five under causal dependence. Each of these latter five might be further decomposed based on the “upon” relation of the contingency in question (see Figure 1 on p. 35). This taxonomy clarifies the differences between various interlocutors’ stances on contingency in science. Whereas Hacking suggests a smooth scale, I propose that contingency can be sharply differentiated. These different views of contingency can be held alone or in conjunction with others, and each combination constitutes a distinct position, which carries different assumptions about how science engages with the natural world.

Discussions of contingency thus far have been rife with ambiguity. Questions remain about what it means for science to be successful, what it means for theories to be reducible to each other, and what constitutes sciences that are sufficiently similar to serve as test cases for contingency. Beatty’s *per se/upon* distinction does a great deal to clarify the debate. It is also important when making contingency claims to be clear exactly what parts of science are claimed to be contingent and what the source of that contingency is.

Any statement that science, as a whole, is contingent, inevitable, or predicated is necessarily cumbersome without identifying the area of science on which that property acts and specifying how that property operate within it. Science might be interpretively contingent without being methodologically contingent, and it might be both without being theoretically contingent. Many processes play a role in the production of scientific knowledge. Contingency may enter through many doors, but it will take on a different

character, with different consequences, when entering through each. The framework I have outlined demonstrates how science can be considered contingent and predicated in qualitatively different ways and exposes assumptions about the causal structure of the scientific process that would otherwise remain implicit. Such causal arguments are frequently cast in the form of counterfactuals: if such-and-such factor had been different/non-existent, some other subsequent factor would also have been different/non-existent. Exploring how such reasoning operates in historical arguments will be the purpose of the Part 2.

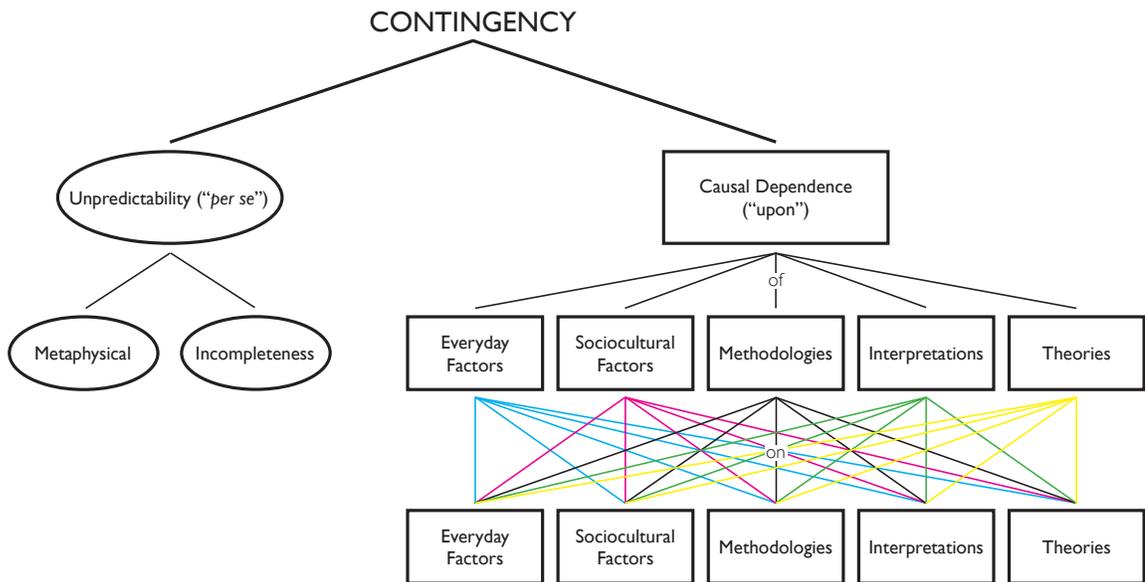


Figure 1 - Chart showing the varieties of contingency described in Part 1

Part 2: Contingency and Historical Counterfactuals

I. Introduction

A counterfactual considers how the world would have been different if something that has happened had not. Counterfactuals implicitly invoke causal dependence contingency: they presume that events subsequent to the event imagined away are *contingent upon* that event. In a more subtle way, counterfactuals also imply unpredictability contingency: they imply that the causal factors remaining after an event is supposed not to have happened offer an *incomplete* account of the actual, observed outcome. Consider the counterfactual, “if Michael Vick had been white, he would not have received such a harsh sentence.”¹² Two contingency claims are evident: first, that Vick’s sentence was contingent upon his race; second, that the sentence was *per se* contingent—was incompletely determined by other factors, such as his supposed guilt or the nature of the crime—in the absence of Vick’s African-American identity. Another way to describe the form of incompleteness contingency implied by counterfactual reasoning is to say that the outcome was not robust—it was highly sensitive to the initial conditions. Any counterfactual claim can be likewise decomposed into parallel contingency claims. The taxonomy of contingency developed in Part 1 can therefore aid in understanding counterfactuals and how they operate.

¹² NFL quarterback Michael Vick was sentenced to 23 months in prison in 2007 for his role in an illegal dogfighting ring. This counterfactual claim subsequently became a commonplace in water cooler bull sessions. In an August 2011 opinion piece in *ESPN Magazine* author Touré attacked the claim as nonsensical, observing both that the sentence was consistent with widespread perception of the crime, and that had Michael Vick been born white he would have led an existence dramatically different from to the one that let him to run afoul of the law (Touré 2011).

Historians have an uneasy relationship with counterfactuals. Although often derided as pure fiction, they nonetheless capture the interest of historiographers. The potential they show to ground rigorous causal arguments in a field where causal explanation is an elusive but seductive goal has cemented their place in methodological discussions. A fundamental tension persists despite these hopes. Historians, who ostensibly describe the actual past, are hesitant to embrace a methodology that emphasizes what did not happen.

Commentators on historical counterfactuals often point out the fanciful narratives that can result from contemplating a past contrary to fact. Popular fiction provides a rich vein of examples, a common invocation being Frank Capra's *It's a Wonderful Life*, which depicts how life in the small town of Bedford Falls would have unfolded differently without the presence of protagonist George Bailey.¹³ These examples focus attention on the claims counterfactuals supposedly make about things that did not happen. Another strand of discussion comes from a philosophical tradition in which counterfactuals are seen as modal claims the truth-values of which must be evaluated. The modal perspective also casts counterfactuals as claims about what did not actually happen.¹⁴ Such approaches do not deny that historical counterfactuals are relevant to history as it did unfold, but by arguing that their validity hinges on possible worlds they commit those who use counterfactual reasoning to superfluous metaphysical consequences.

¹³ See, for example (Tucker 2004, p. 237) and (Ferguson 2000, p. 2). While not citing Capra directly, a similar approach to counterfactuals through fiction characterizes (Rosenfeld 2002).

¹⁴ This approach is evident in (Tucker 2004), (Bulhof 1999), and (Tucker 1999), which continue the tradition established by works such (Lewis 1973).

The following proposes an understanding of counterfactual reasoning that avoids some serious difficulties with traditional approaches. Historians do not seek to understand alternate historical trajectories or possible worlds; their concern is the actual historical process. Any conception of counterfactuals which casts them as primarily about alternate or possible worlds is therefore of suspect historiographical value; in history, claims about possible worlds are only relevant insofar as they can scaffold claims about the actual world. Using counterfactuals to discuss the actual world, however, is similarly problematic, as it tends to overcommit historians to rigid metaphysical conceptions of the historical process. Counterfactuals might be conceived as tools to test causal hypotheses through the application of causal dependence contingency claims. These claims, however, come with a host of difficulties. They promote reasoning from a fictionalized history and necessarily come with parallel robustness claims, making the causal argument dependent on a metaphysical commitment that cannot be evidentially substantiated. By examining how causal relevance and sensitivity to initial conditions work in concert I suggest that counterfactuals should be properly understood as referring exclusively to historical evidence, rather than to either possible or actual historical worlds.

II. Counterfactuals in the Philosophy of History

Philosophers of history have identified two factors central to counterfactual reasoning: causality and robustness. The former has received disproportionate attention. This section outlines what has been said about these factors, identifies problems with each, and argues that overemphasis on causality in the counterfactual literature has

undersold the metaphysical pitfalls of counterfactual reasoning. By directing discussions toward possible/plausible worlds or alternative histories, previous work has skirted some troubling question about the nature of counterfactual arguments when used in historical analysis of the actual world. The argument that omitting a causal factor would result in a different outcome includes the implicit claim that other causes were insufficient to produce the same outcome. Counterfactuals therefore necessarily include claims about historical robustness. Attention to robustness as a necessary element of counterfactual reasoning on a semantic par with causality is necessary in order to formulate a responsible account of counterfactual thinking in history.

A. Counterfactuals and Causality

Philosophical discussions of historical counterfactuals often point to the power (and in some extreme cases, necessity) of counterfactual reasoning to reveal the causal structure of the past.¹⁵ Aviezer Tucker, one of the most prolific writers on historical counterfactuals, observes: “Since we understand history backwards, it is possible to assess the effects of contingent minute causes, though it is impossible to predict such effects from examining minute causes in the present” (Tucker 2004, p. 229).

¹⁵ Ferguson (1997) argues for the necessity of counterfactual reasoning in causal historical claims. It is not my purpose here to confront the prickly question of how causal claims *can* be justified in history. Ferguson’s extreme position, however, does provide an example of how Part 1’s taxonomy can expose difficulties with causal claims rooted in counterfactuals. Counterfactuals rely on holding some element of history contingent. As I argue above, causal relationships might be cast either as contingency or predication relations. The latter are not subject to counterfactual reasoning because counterfactuals require the possibility that the contingent antecedent condition might have been different. Requiring counterfactuals for historical causal claims therefore prevents the historian from assigning causal efficacy to any necessary antecedent condition.

Counterfactuals, for Tucker, are evidence-based claims that differ only minimally from standard historical argument:

[C]ounterfactual historiographical hypotheses are tested just like factual historiographical hypotheses. The difference between factual and counterfactual hypotheses is ontological not epistemic. Counterfactual hypotheses only require a *ceteris paribus* clause applied to the evidence: We suspend belief in a body of evidence that proves the counterfactual never happened while maintaining all the rest of the evidence. The probability of the counterfactual depends on the likelihood of the rest of the evidence given a counterfactual hypothesis (*ibid.*, pp. 229–230).

Tucker's probability argument is Bayesian in structure. He advocates assessing our belief in the probability of counterfactuals by evaluating the "likelihood of the evidence given the hypothesis multiplied by the prior possibility of the hypothesis" (*ibid.*, p. 231).¹⁶

This approach leads to the odd consequence that we have actual belief about events that did not actually happen. Tucker maintains that counterfactuals are "just like any other historiographic proposition" (*ibid.*, p. 239) because they are based on a (sufficiently lobotomized) body of evidence, and so should be treated as epistemically equivalent to other evidence-based historical claims, even though they refer, at least in part, to non-actual historical events. On this account counterfactuals commit the historian not only to belief in the causal efficacy of historical actors or events, but also to belief in the non-actual causal efficacy of those actors' or events' absence.

Tucker's assertion that the evidence supporting the counterfactual is of an equivalent kind to evidence supporting other claims is problematic; the omitted evidence does real work by virtue of its absence, and the evidence still under consideration supports nothing more than initial conditions. By Tucker's own admission, we cannot

¹⁶ Tucker goes so far as to formalize this statement: " $\Pr(H_{cf} | E') = \Pr(E' | H_{cf}) \times \Pr(H_{cf} | B_t)$ " (*ibid.*).

predict the future by examining minute causes in the present. Reasonably, this should also prohibit us from describing faithfully what the consequences of a set of causes in the past would be without foreknowledge of the result. Tucker presumes that the knowledge we do have of the actual historical outcomes a set of causes produced is grounds for knowledge of the results of that same set minus one cause.¹⁷ The validity of this position relies on a reductionist view of historical causality. The required *ceteris paribus* condition—supposing that fixing all other factors while subtracting one provides a clear picture of what the result would have been—is not innocuous, as it smuggles in the assumption that historical causes operate independently of one another. Using Tucker’s account, the historian plays a heavy price for using counterfactuals to justify causal claims. First, it requires an impoverished conception of historical causes. Second, claims about the actual world depend on the truth of claims about the non-actual world, leading readers who might be willing to accept causal claims to reject them because they are suspicious of the modal claim. Rather than scaffolding a causal claim about the actual world, introducing modal reasoning via counterfactuals undermines what might otherwise be straightforward causal inferences.

An election provides a clear example. It generates evidence—ballot returns—that substantiates a conclusion—the outcome of the election. It is also an attractive target for counterfactual reasoning. In the 1884 presidential election, Grover Cleveland narrowly defeated James G. Blaine on the power of New York’s 36 electoral votes, after winning the state by just 1047 votes. The obvious counterfactual claim is: if Cleveland had not

¹⁷ The line of reasoning emphasizing common evidence and foreknowledge of actual outcomes is common among defenders of counterfactuals. See also: Lebow (2000) and (2008), Ferguson (1997).

won New York, he would have lost the election. In the sense Tucker describes, such a claim does seem to be supported by the evidence. If we suspend belief in the 1047 votes that handed Cleveland New York it becomes mathematically obvious that Blaine would have won the election. It seems we have an evidentially substantiated historical counterfactual.

That conclusion would be premature. Suspending evidence involves more than Tucker recognizes. Cleveland's small margin of victory in New York makes the supposition that he might have lost the state seem innocuous, but even that ostensibly small step carries with it some hidden assumptions. In the election that actually happened, 1047 more people voted for Cleveland than voted for Blaine. If we assume that New York had swung differently, we have to account for at least 524 voters choosing Blaine instead of Cleveland. What could have accounted for that shift? An imagined gaffe on Cleveland's part? A slightly different policy stance on Blaine's? The counterfactualist has three options: provide a reason for the difference, accept a metaphysically indeterminate historical process, or proceed with an explanatory gap.

Probing a seemingly uncontroversial and well-documented counterfactual argument exposes two problems. First, the claim is not, as Tucker asserts, substantiated on the power of existing evidence alone unless the historical process is presumed to be metaphysically random. It requires the introduction of new, imagined evidence, such as the gaffe or the different policy stance. In effect, substantiating a counterfactual claim in the way Tucker suggests is epistemically responsible requires the historian to write fiction. The alternative is indeterminacy or an explanatory gap that would leave the

proposed difference unaccounted for and the counterfactual claim unsubstantiated. Second, the claim ignores interconnection between causal factors and the evidence that supports them. How would a change in Blaine's policy positions substantial enough to win him New York have influenced his performance in Pennsylvania? Answers to these questions are, at best, speculation. Using Tucker's method to develop a straightforward and uncontroversial causal claim—New York was critical to Cleveland's victory in the 1884 Presidential election—paints the historian into some unsavory corners.

A contrasting approach to the question of causality nonetheless leads to similar difficulties. Some writers have looked to chaos in history, which presumes causes to be deeply interconnected rather than reductively independent, as grounding for counterfactuals. Johannes Bulhof takes this approach after setting it up by observing: "As philosophers have known for quite some time, causal explanations are often connected to modal claims and counterfactuals" (Bulhof 1999, p. 147). Bulhof argues that modal claims pervade historical arguments, often implicitly. He focuses on the difficulty of assessing whether counterfactual claims are true, but, like Tucker, neglects the question of *about what* they might be true, assuming that historical counterfactuals, like the philosopher's modal counterfactuals, purport to assign truth-values to the possibilities they propose.

This tendency shows in his illustrative example of a football game. He focuses on the question of whether a difference in the way an individual play unfolded would have impacted the outcome of the game. Conscious of the interconnectedness problem, he objects to such Monday-morning quarterbacking on the grounds that: "Each change in the

game affects the way the rest of the game is played. For example, if you have a lead, you tend to play more conservatively” (*ibid.*, p. 160). Nonetheless, he allows some role for counterfactual reasoning within the football example:

Suppose one team is behind by a point, and is set to kick a field goal with time running out. Suppose that in actual fact the team misses the kick. It is true that, everything else being equal, the team lost the game. But it is also true that had the kick been made, the team would have won. This is uncontroversially true, and shows that we can know the truth about some counterfactuals, even though we can have only justified or unjustified beliefs about others (*ibid.*, p. 161).

Bulhof does not comment on whether missing the kick caused the team to lose, despite his earlier observation that counterfactuals seek causal explanations. His focus has shifted to the truth of the counterfactual claim itself and away from the causal structure of the actual past, which, for the historian, the counterfactual should support. A closer look reveals that the counterfactual has problems supporting the causal story a historian concerned with explaining the actual past might want to tell. Respecting the complexity of history’s causal structure avoids reduction, but it nonetheless requires accounting for differences in the counterfactual scenario or accepting either an explanatory gap or metaphysical randomness. In order to avoid the explanatory lacuna, the supposition that the team did make the kick requires a reason for the ball’s trajectory to be different than it was. If it was a gust of wind, how might the windier conditions have influenced earlier kicks or passes? If we imagine a different kicker, how would the actual kicker’s absence, either through contributions on the field or sideline presence, have borne on earlier plays?

Differences, no matter how small, require explanation, and in a chaotic system we cannot be sure they will not contaminate other elements of our system. A chaotic approach, it is true, makes for smaller fictions. It becomes easier to postulate that minute

changes have substantial consequences, but at the same time the causal interconnectedness a chaotic view admits makes cataloging the ancillary effects of such a change practically impossible. The causal claim that the team lost the game in part because they failed to make the kick is plainly available without recourse to counterfactual analysis, which, even if one respects the interconnectedness of causal processes, requires the invocation of raw fiction. It is equally easy to show that the team lost because they failed to score a touchdown on the previous play. This is an argument counterfactualists are less likely to embrace, as it highlights the difficulty of stating the causal impact of such missed opportunities precisely. Was the team's loss caused more by their failure to make the field goal or by their failure to score a touchdown on the previous play? Or by their failed goal line stand in the third quarter? Like the election example, the highly structured nature of the field goal scenario makes it appear susceptible to counterfactual reasoning. It fails to support the utility of counterfactuals, however, for the same reason more complex or far-fetched counterfactuals fail: the influence of the introduced changes cannot be localized to the counterfactual—no matter how nearly they fall to the end of the game—and so their full impact cannot be charted.

Making causal claims via counterfactuals is a hairier process than it might appear. First, it privileges a reductionist picture in which causes operate independently and therefore can be extrapolated unproblematically in the absence of another causal agent with which they cooperated in actuality. Second, it must either leave the question of why a causal factor was absent in the imagined scenario unanswered, or invent a fictional reason in its place. The choice is between accepting indeterminacy—i.e. claiming that the

factor in question could have been different because the historical process is essentially random—or basing one’s causal claim about the real world on events in a fictional world. While the first problem can be avoided by an approach that acknowledges causal complexity, the second cannot.

B. Counterfactuals and Robustness

Bulhof’s discussion also provides an apt example of the difficulties inherent in the second type of counterfactual contingency. As Bulhof’s exclusive emphasis on causality leads him to focus on truth claims about counterfactual effects, it also compels him to make ontological assumptions about the sensitivity of the historical process to initial conditions. He turns to chaos theory to describe both the possibilities and limitations of counterfactual reasoning: “History is clearly [...] a complex system. It is irreversible, and so is most likely chaotic. Small disturbances in certain contexts can have profound effects that are unpredictable” (*ibid.*, p. 162). Bulhof riffs on a classic thermodynamics heuristic to describe the type of system he believes history is:

Suppose one has a chess board and a flea. It is quite obvious that sooner or later the flea will return to its initial position. This is reversibility. Suppose, however, that you start with the same board, but with 100 fleas all starting in the same square. It is quite obvious that if you wait until they return to their initial position, you will be waiting a long time. The more fleas one starts with the more complex the system. Such a system [...] is chaotic. One cannot predict how it will evolve except in the most general terms (*ibid.*).

The presumption is that history is like a system in which all 100 fleas start from the same square. In such a system, minor differences in the way the fleas are initially arranged can indeed have large consequences for the way they disperse across the board, or, as Bulhof

writes, “Small disturbances in certain contexts can have profound effects that are unpredictable” (*ibid.*, p. 162). It is, however, a raw assumption that history is like a case in which 100 fleas begin on one square. We might also suppose that history is like 100 fleas that are already evenly distributed across the board, in which case minor changes in their initial positions would produce only negligible variations in the system’s evolution. Such a state would have properties more like a thermodynamic system in equilibrium, for which the predictable behavior of the system as a whole is unaffected by minor changes in its substructure.

Commitment to deep metaphysical claims about the historical process is a frequent unintended consequence of counterfactual analysis. Counterfactuals invoke incompleteness contingency in their robustness component. They contend that the set of causes remaining after one is omitted is insufficient to explain the outcome. For such an argument to work it must make assumptions about the way historical causes behave. This can lead either to a reductive view of causes, as in the case of Tucker, or to commitment to an extremely sensitive, non-robust system, as in the case of Bulhof. In either case, the deep metaphysical toll the argument pays far outweighs the utility of the counterfactual for elucidating causal relationships.

Counterfactuals can also lead to an unintended investment in historical indeterminacy. After Capra’s *It’s a Wonderful Life*, the next most prominent metaphorical touchstone for meta-counterfactual analysis is Stephen J. Gould’s near-eponymous *Wonderful Life*, which explores the question of how minor changes during the Cambrian period might have led to a dramatically different picture of life on Earth

(Gould 1989). While Capra provides authors with an example of causal dependence, Gould typically enters as an example of robustness. Niall Ferguson, in the introduction to a 1997 collection of counterfactualist histories, coopts Gould in the hopes that the evolutionist's attack on biological determinism will support a similar case against historical determinism, particularly social determinism (Ferguson 1997, p. 75). Ferguson's discussion of Gould illustrates his argument that history is not deterministic, which he says "does *not* mean that there are no laws in the natural world [...] simply that those laws are so complex that it is virtually impossible for us to make accurate predictions, so that much of what happens around us *seems* to be random or chaotic (*ibid.*, p. 77).¹⁸

Ferguson holds that his notion of indeterminism is epistemic. Events in history are not uncaused, he asserts, but their causes are sufficiently complex and obscure as to make the past effectively random to a human observer. From this he derives the methodological directive: "if we want to say anything about causation in the past without invoking covering laws, we really have to use counterfactuals, if only to test our causal hypotheses" (*ibid.*, p. 81). The existence of possible alternatives, therefore, provides evidence for the causal efficacy of individual people, things, or events in history. At this

¹⁸ Richard Evans observes that Ferguson's articulation of a chaotic history, in which small changes can have large effects, accords with his well-documented political conservatism (Evans 2008b, p. 79). Ferguson's desire to undermine determinism can, in this light, be understood as an outcropping of late twentieth-century methodological debates. Ferguson's tendency to emphasize the role of individual agency over social power dynamics corresponds to his desire to undermine large-scale, social determinist views of history, such as those espoused by Marxist historians. The ideological bent to his work might also explain why he so easily commits himself to an ungrounded metaphysical assertion favorable to his political views. Curiously, the ideological battle lines reverse when traveling from history writ large to the history of science. In history, admitting greater contingency into the historical process is seen as corresponding with a politically conservative preference for granting greater agency to the individual whereas liberal social determinists are supposedly less contingentist as they subject history to indomitable social forces. Within the history of science, social constructionism, a view typically associated with liberal political ideologies, is commonly considered the more contingentist view.

junction, Ferguson, like Bulhof, begs the robustness question. Specifically, he assumes that epistemic unpredictability indicates metaphysical instability. He jumps from the reasonable statement that the complexity of history makes it appear chaotic to the conclusion that because it appears chaotic, it actually could have occurred differently at every juncture, even without alterations in antecedent conditions, describing history as a series of “genuinely chaotic and unpredictable events that could have turned out differently,” (*ibid.*, p. 88). Ferguson uses this un-argued-for assertion to ground the conclusion that counterfactuals are necessary in order to make causal claims.

Ferguson further attempts to skirt the evidential problems of counterfactual claims by insisting that the only counterfactual scenarios historians should consider are those that were in fact contemplated as possibilities by historical actors before the fact. Such a move, he presumes, would fill in the documentary gap in the counterfactual, where we must invent a fiction to account for the postulated departure from actual history. Ferguson fails, however, to show how the existence of such documentation makes the counterfactual outcome any more probable. The plug for the gap in counterfactual analysis must explain *why* the difference occurred. To use Bulhof’s football example, the head coach speculating that his star kicker might come back from injury sooner than expected does not give us any reason to suppose that he would have, nor does it answer the other questions about how that one change would have generated further changes. Combining Ferguson’s misguided methodological admonition to consider only historical actors’ views of the future with his claim that counterfactuals are necessary for causal claims also leads to the bizarre practical consequence that the only historians capable of

making causal claims about history are those who use counterfactual analysis and have a suitably rich supply of historical documents in which historical actors speculate about the future.¹⁹

Assumptions about the non-robust nature of history are used both to dramatically circumscribe the scope of counterfactual history (Bulhof) and to espouse its necessity (Ferguson). This range is indicative of the limited extent to which the nature and consequences of such assumptions and the way they follow from counterfactual thinking have been explored. Yemina Ben-Menahem makes a first step towards addressing this need.

Ben-Menahem's "Historical Contingency" (1997) attempts to rigorously characterize the concepts of contingency and necessity as they are used in historical reasoning. She jettisons the logical concepts of necessary and contingent truth, noting, "all historical truths are contingent in the logical sense" (*ibid.*, p. 99), and recasts them apart from chance and causality. On this view, the degree to which a historical event is either contingent or necessary depends on its sensitivity to initial conditions. An inevitable outcome is one which can be produced by a wide range of causal chains, whereas a contingent outcome is the product of one of very few possible causal chains (*ibid.*, p. 100).²⁰ Ben-Menahem identifies five advantages of this view:

¹⁹ Evans further observes that this view conveniently circumscribes "true" history within Ferguson's own research interests in Western political, military, and diplomatic history, while conveniently leaving the social historians he reviles out in the cold (Evans 2008b, pp. 81–82). This observation exposes the way in which Ferguson attempts to use contingency as a methodological bludgeon, much in the same way Gould does. Just as Gould wants to show that selection is inadequate to explain all the properties of organisms, Ferguson wants to show that large-scale social forces are insufficient to describe the sweep of history. Both make their cases using *per se* contingency claims.

²⁰ Tucker (2004), in fact, cites Ben-Menahem as useful for contingency and necessity but he does not use her in his discussion of counterfactuals.

- (1) Contingency and necessity are not either-or concepts, but define an array of possibilities, avoiding the often puzzling binary question of whether a given event is *either* contingent or inevitable.
- (2) The contingency/necessity distinction is separated from the chance/causality distinction, so that “contingent” need not mean “uncaused.”
- (3) Historians can describe how someone/something “makes a difference” in history without begging the determinism question one way or another.
- (4) It allows us to explain why certain outcomes seem like foregone conclusions without postulating a teleological history.
- (5) It can describe individual processes independently and makes no grand presumptions about the nature of history as a process (*ibid.*, pp. 102–105).

The fifth advantage serves as an umbrella category for (2) – (4), and provides the framework with much of its strength. Unlike other attempts to address robustness in counterfactual reasoning, Ben-Menahem perceives and avoids much of the danger posed by the specter of unintended metaphysical commitments. Because her approach makes no sweeping claims about the nature of the historical process as a whole, it is flexible enough to be incident-specific.

Ben-Menahem’s analysis makes it clear that the deep ontological potholes that lie in the path of counterfactual reasoning are not unavoidable. By neatly distinguishing between chance and contingency, cause and necessity, we can arrive at a more malleable understanding of the past, which allows us to assess the robustness of each historical trajectory on its own merits. We can have the “failure of predictability without failure of

causality” (*ibid.*, p. 102). When Ferguson attempted the same move, he was forced to adopt a chaotic history on specious grounds. Ben-Menahem shows that he need not have gone so far.

This view does have its limitations. Foremost, it is not clear how robustness claims made within Ben-Menahem’s framework would be documented. The challenge of substantiating the claim that one historical sequence was more or less susceptible to initial conditions than another would be monumental. It offers a useful way to characterize historical instincts about contingency, but has less utility as a methodological directive. A local robustness assumption might be less pernicious than a global robustness assumption, but it is still an auxiliary assumption to which counterfactuals commit the historian. Although Ben-Menahem helps us avoid one major problem with the robustness claims intrinsic to counterfactuals—the fear that it (or its absence) must simply be postulated as a general characteristic of history—we are left nursing a weakened form of the same malady.

Robustness is often neglected at the expense of causality when discussing counterfactuals, but the implications of the robustness claims counterfactuals include, usually implicitly, are often more general and so often have a bigger impact on the historical arguments counterfactuals motivate. These general consequences are a steep price to pay for the local causal claims counterfactuals are supposed to support. Adopting Ben-Menahem’s understanding of local sensitivity to initial conditions can blunt this difficulty, but local robustness claims are just as challenging to substantiate as global ones. When combined, the difficulties posed by causal and robustness components leave

counterfactuals in a difficult place. In the next section, I begin to resolve this quagmire and define a realm in which counterfactuals can be of some limited use.

III. Towards a Cautious Account of Counterfactual Reasoning

Causality and robustness are both central to debates about counterfactual reasoning. The current literature addresses each, but has not yet offered an account of how they do and should work in concert within historical reasoning. This section takes a first step towards addressing that need, and proposes one metaphysically and methodologically cautious way in which historians can understand counterfactual reasoning as a heuristic tool.

Much of the difficulty with counterfactuals stems from the fact that they combine two qualitatively different types of contingency claims. Attempts to deploy them in the service of causal argumentation therefore saddles what should be straightforward causal arguments with problematic metaphysical baggage. Combined with the red herring of possible worlds, this difficulty means that counterfactuals often miss their mark entirely. They make actual claims about non-actual worlds only by adopting deep assumptions about the nature of the historical process. These difficulties can be resolved by appropriately narrowing the scope of counterfactual reasoning. I have already argued that counterfactuals should not be understood as being about possible historical worlds; I now want to argue that they should not be construed as being about the actual historical world either. One way in which counterfactuals can avoid the problems described above is by referring strictly to historical evidence.

To illustrate this claim, I return to the example of the election. The counterfactual is: “if Cleveland had lost New York, he would have lost the election.” On Tucker’s view this is a claim about the world, supported by the same body evidence that supports the historical fact of Cleveland’s victory. The discussion above identified several problems with this view. These problems evaporate, however, if we consider it a claim about the evidence itself, rather than a historical claim.

As usual, the evidential counterfactual can be decomposed into two contingency claims. First is the claim that Cleveland’s victory in New York is causally central to our belief that he won the election; second is the robustness claim that such a belief is highly sensitive to a particular initial condition. These are claims about the dynamics of a body of evidence that expose how it influences our historical judgments. Such manipulation of the evidence is an experiment we can actually perform. We can shift subsets of electoral votes from Cleveland to Blaine and see what the outcome would be. Doing so does not tell us anything about the world, but it tells us quite a bit about how our data works, and as such can help us identify areas of interest. For example, if we observe that shifting a small number of votes from the Cleveland column to the Blaine column in New York radically alters the outcome of the election, that might help us conclude that voter sentiment in New York poses an interesting area for traditional historical analysis.

The function of evidential counterfactuals is straightforward in the highly structured context of a political election, which has few possible outcomes and a relatively homogeneous body of evidence. How might this interpretation operate in a less structured context? Peter Bowler (2008) offers a suitable example, advancing the

counterfactual argument that without Darwin—whose interests and abilities were sufficiently unique for him to make a substantial impact on the course of nineteenth century biology—the theoretical content of twentieth century biology would have been largely the same, but it would have been inflected differently. Bowler claims that a nineteenth century without Darwin would have led to a biological community that emphasized different theoretical elements of evolutionary biology from the ones it actually did. For instance, he retrodicts a less virulent eugenics movement and a less monopolistic focus on a molecular basis for heredity in the absence of Darwin's influence (Bowler 2008). In decomposed form, Bowler claims that Darwin's work was causally important for the development of late nineteenth and early twentieth century biology in a broad range of ways, and claims that the theoretical trajectory of science was robust while the social trajectory was not.

Taken as a claim about either the possible world in which Darwin did not influence biology, or about the actual importance of Darwin in this world, Bowler's account encounters several of the problems outlined above. We are left to wonder what factors account for Darwin's absence and how their other impacts might have propagated. We must also confront unnecessary metaphysical assumptions about the robustness of Victorian science. Bowler's thought experiment is more useful when understood as a claim about evidence. A wide range of evidence is relevant when considering Darwin's influence. First of all, we have Darwin's works themselves: *The Origin of Species*, *The Descent of Man*, his private correspondence, notebooks, etc. We also have the work of his contemporaries, predecessors, and followers who were working on similar questions: the

work of Alfred Russell Wallace, for instance, which was proceeding along similar lines. Finally, we have evidence documenting the broader social and intellectual context in Victorian England. Debates about the age of the earth that bore heavily on evolutionary questions and the social structure of British and British colonial science, to name but one of each, would both be critical to understanding the totality of Darwin's importance.

Now consider Bowler's counterfactual as an evidential claim. Cordon off evidence related to Darwin and examine what remains. Examine it not to determine how a history proceeding from such a point would have unfolded, but rather to understand how relationships between pieces of the evidential landscape change, do not change, or appear differently as a result. As Tucker observes, raw extrapolation is not available in the present to predict the future. As a result it should not be available to predict imagined pasts either if we take causal interconnection seriously. Instead, use that body of evidence as a way to isolate interesting questions. If that body of evidence suggests, as Bowler asserts, that evolution's social impact was not primed to be so severe, that would suggest that we believe Darwin to be influential because of his work's impact on social factors, and that examining that impact would be fruitful. It might also suggest other interesting areas of Victorian biology that have been neglected, but which shine a little brighter when not eclipsed by Darwin's star.

An evidential view of counterfactuals allows them strictly heuristic utility.²¹ They can give us a causal account of why we interpret our data the way we do, and describe how robustly a body of evidence motivates a particular interpretation. This view denies

²¹ Nolan (2011) provides some other reasons why counterfactuals might be rhetorically or heuristically interesting, even though the account therein does not avoid the metaphysical problems outlined above when it turns to questions of explanation and causation.

counterfactuals the capacity to play any important role in historical epistemology or causal reasoning. Counterfactuals, as many authors have recognized, are commonplace in everyday thinking. Some authors go on to argue that they should therefore be characterized rigorously so that they can play a similar role in structured historical reasoning. Instead, I suggest that counterfactuals are acceptable in everyday thinking precisely because that type of reasoning does not aim for rigor. The stance sketched above circumscribes their role to the everyday realms of historical work: counterfactuals can help historians understand their evidence in a way that points out interesting features before they embark on serious historical analysis.

IV. Summary

Historical counterfactuals are deeply fraught. They simultaneously advance two different types of contingency claim, one of which is frequently unintended. Historians who use them risk making sweeping metaphysical commitments. These difficulties make counterfactuals unsuitable tools for understanding the past. Constraining counterfactuals so that they do not purport to make claims about the past, however, helps define a range of applicability in which they can do useful historiographical work without overcommitting the historians who use them.

Counterfactuals are not necessary to make causal claims, contrary to what some have asserted.²² The causal claims they do inevitably make are often reductionist, and necessarily invoke fictional imaginings, on pain of either incompleteness or

²² Namely Lebow (2008) and Ferguson (1997).

indeterminacy. As Ben-Menahem aptly shows, historical causation is distinct from scientific or philosophical conceptions of causation. Attempts to shoehorn it into those categories from the standpoint of either scientific history or analytic philosophy are therefore doomed from the outset. The commitments counterfactuals require are too steep a price to pay to formalize causal historical reasoning.

The robustness claims counterfactuals necessarily involve can be an asset rather than a liability if understood as claims about sources rather history. By adopting Ben-Menahem's notion of robustness and understanding the available supporting evidence as the initial conditions, counterfactuals can be understood to evaluate how strongly the available documentation promotes belief in particular historical outcomes, making them a useful tool for identifying fruitful research areas. This interpretation avoids sweeping claims about the nature of the historical process and eliminates the need to make loosely supported or unsupported claims about the sensitivity to initial conditions either of individual chains of events or of history as a whole.

The role of counterfactuals is thereby substantially curtailed, but not eliminated. The way in which they combine two forms of contingency means that they are almost always doomed to come saddled with ancillary claims that expand beyond their targets. The above discussion has outlined one way they can be understood so as to avoid extraordinary or unsupported claims about history as it happened, history as it did not happen, or the nature of the historical process; however, avoiding these problems leaves the counterfactual to occupy a small and specific niche in the historian's toolkit.

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