Forum Essay

Counterfactual History: A User’s Guide

MARTIN BUNZL

Is there a respectable basis for counterfactual reasoning in matters of history? Or is such reasoning what E. H. Carr dubbed a mere “parlor game”? Historians approach counterfactual reasoning with enormous unease, and they do so with good reason. The rhetoric of historical methodology makes evidence central in a foundational conception of the discipline, and (direct) evidence is just what you can’t have for claims that are by definition contrary to fact. Yet counterfactual history lives on, both in the implicit claims of any explanation or causal claim as well as explicitly in a burgeoning recent literature.¹ That literature is the focus for what follows here. My argument is quite simple: counterfactual reasoning comes in two varieties—good and bad. The bad reasoning is bad because it has no grounding; it is merely an act of imagination, and unconstrained imagination at that. The good reasoning is good because it can be grounded. My aim is to detail the varieties of such groundings that are available to the historian.

In what follows, I argue that counterfactuals are not as easy to avoid in the practice of history as one might think. Historians who defend the use of counterfactuals have usually done so by appeal to the role of imagination. I take issue with this and defend the use of counterfactuals by appeal to considerations of three bases: laws, rationality, and causal analysis. I defend these as means of grounding counterfactual reasoning based on indirect evidence. I then try to extend the account to cover other opportunities for defensible counterfactual reasoning by arguing that we should demand no greater standard of evidence here than we do in other areas of historical judgment where informal methods reign. The argument closes by posing a philosophical worry about a regress but offers an answer based on the absence of wholesale determinism.

This essay benefited greatly from my attendance at a conference on counterfactual history held at Princeton University (“What If? Counterfactualism and Early American History,” March 20–21, 2001), organized in honor of John Murrin. I thank all the participants for putting up with me. For Murrin on counterfactuals, see especially “The French Indian War, the American Revolution, and the Counterfactual Hypothesis: Reflections on Lawrence Henry Gipson and John Shy,” Reviews in American History 1 (1973): 307–18. An earlier version of this essay benefited from the discussion at a colloquium at the Hebrew University, Jerusalem. Thanks are due to Gabi Motskin for his comments. Many thanks to John Gillis, Mike Merrill, Sue Cobble, and Deborah Hertz for helpful guidance and to the editor and referees of the AHR.

When Simon Schama writes: "If only the Austrian Archduke Franz Ferdinand’s driver had met a well-meaning man in the Sarajevo street in June 1914" and ends up asserting (tongue in cheek): "no First World War, no Hitler, no Stalin, no nuclear weapons, no Sarajevo crisis (1990s)," there is a reason he does so in the magazine Talk and not in a scholarly publication. This is nothing more than an entertainment. But Robert Fogel is right when he claims that "[e]very historian who has set out to deal with the causes of the Civil War . . . has implicitly or explicitly presumed what would have happened to slavery if some events had unfolded in a way that was different from the actual course."

Fogel’s aperçu is that there is an incestuous connection between causal claims and counterfactual claims. If that is right, and historians make causal claims, then counterfactual claims come along for the ride, like it or not. Of course, not every historian will allow that causation does or ought to play a role in history, and not every philosopher will allow that counterfactuals are as closely allied with causal notions as the above formulation implies. But these reservations notwithstanding, you can get to the same conclusion without relying on causation. Historical narrative of the most innocent-seeming sort is replete with locutions that carry lurking counterfactual commitments:

A myth became established in later years that Napoleon missed the moment of French destiny when he failed to respond to the British approach of 21 February. In truth there was no British approach . . . Apart from this, the time had long passed . . . when England and France could by themselves impose their will on central Europe. Even with Russia, it would have been a speculative affair.

We have seen what a problem the demobilization of the Swedish army in Germany and its withdrawal from the country had constituted at the conference table. But it is doubtful whether the restitutions and restorations stipulated by the Peace of Westphalia could have been realized, particularly in southern Germany, if the Swedish troops had not been in the country.

A review of the results of the battle of Zorndorf leaves no doubt that Frederick would have acted more to his own advantage if on the fateful [day of] August 25 he had contented himself with carrying off the Russian heavy baggage, instead of aiming at the higher goal of crushing the hostile forces.

Still, even if it is true that counterfactual claims may be unavoidable, someone may object that this is quite different from them being an appropriate subject of historical inquiry themselves. If historiography were brimming with examples of studies in which counterfactual claims were the subject of interest, one could wave to community standards and practice to answer this reservation. But the fact is that, while perhaps there is a counterfactual supposition in a claim of Thucydides or a counterfactual aside to be found in Edward Gibbon quoted in essays on counterfactual history, what is usually quoted is far from what is needed to make the case

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that counterfactual claims are as old as history, at least qua the subject matter.7 Where that is the interest, we have to look to more recent work, and without an explicit tradition of asking such questions to rely on, it is fair game to ask, "What is going on here?"

PART OF WHAT IS OFTEN GOING ON HERE is, if not a parlor game, an enterprise without serious historiographical aspirations. Instead, what is offered are speculations that are to be judged by their creativity operating under the constraints of professional expertise.

"What if, in the summer of 1941, Hitler had chosen to make his major attack . . . into Syria and Lebanon?"8 Then (skipping the intervening conditionals), had he "used the Balkan victories of the spring of 1941 to align his forces for an Anatolian and Levantine victory, leading to wide conquests in Arabia and securing decisive positions on Russia's southern flank, it is difficult to see how a variant of Barbarossa, conceived as a pincer movement rather than a blunt frontal assault, would not have succeeded."9

John Keegan's imaginings about battles that never were are likely to be more interesting than mine because Keegan is steeped in battle. There is ample reason to think that Keegan's imaginings are not just more imaginative than mine but also more plausible to boot. Indeed, the editor of the volume in which Keegan's piece appears speaks of criteria of selection that include plausibility.10 But other than authorial pedigree, just what makes for such plausibility? In "straight" history, we have what is a straightforward answer, even if it is unfashionable in some quarters—truth. And even if we may not have ready access to the truth, we can usually grab some of its (archival) leavings. In this sense, two things are missing if we want to talk about the plausibility of counterfactual claims. Counterfactuals are contrary to fact, so ipso facto they are not true (in the actual world), and, ipso facto, they leave no evidence. But there is an unnecessary distinction here: if counterfactuals left evidence, we could choose between competing counterfactuals and settle which were plausible and which were not on that evidence. Truth would not need to enter the picture. Indeed, that is what many would say is just the right way to think of straight history—spare us the talk of truth, just give us the evidence.11

Niall Ferguson poses the problem of counterfactuals this way: "how are we to distinguish between probable unrealized alternatives from improbable ones? . . .

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9 Keegan, "How Hitler Could Have Won the War," 305.
11 Although, in the currently favored semantics for possible worlds, counterfactuals are true or false in the following sense: to say A counterfactually implies B in our world is to say that the closest possible non-A worlds are non-B worlds.
We should consider plausible or probable only those alternatives which we can show on the basis of contemporary evidence that contemporaries actually considered. If this were right (and I'll say why I don't think it is later), it would still only solve half of our problem. Think of a counterfactual as a conditional statement of the form "If . . . , then," with the antecedent known to be false but taken as true. (That is, "If Al Gore had been declared the winner in Florida, then . . . ") Ferguson treats the problem of the plausibility of counterfactuals as one of identifying the conditions under which we should take these antecedents as probable. But suppose we do that. We are still left with the question of whether the consequent of the counterfactual conditional may be plausibly thought to follow from that antecedent. So, consider: "If Gore had been declared the winner of the Florida election, then the United States would not have invaded Afghanistan." The plausibility of the overall counterfactual may be thought of as a function of the plausibility of the antecedent and the plausibility that the consequent follows from it.

Now when philosophers think about counterfactuals, being impractical by disposition, we tend to give short shrift to what worries Ferguson. We worry about on what basis one might plausibly claim that the consequent does follow from the antecedent, however implausible the antecedent may be. Let's start worrying there and circle back to where Ferguson starts from (along with most historians). So, for now, our concern can be applied equally well to the speculative antecedent of the counterfactual: "If Adolf Hitler had invaded England in 1940, he would have used tow planes to release gliders carrying parachute troops five miles east of Dover at an altitude of 3,000 feet," as it can to the more sober: "If Jackson had not vetoed the re-charter of the Second Bank of the United States in 1832, the inflation crisis of the 1830's would not have taken place," even though the second has an antecedent that is more plausible than the first. Indeed, we can even apply it to counterfactuals with certifiably wacky antecedents, like: "If the partisans of Vilna had had a nuclear device at their disposal, they would have prevailed."

When it comes to examining the consequent of a historical counterfactual, like most historians Ferguson implicitly embraces the view that what is involved is always and only an act of imagination. The difference between plausible and implausible counterfactuals might be thought to be a function of the degree to which we are able to discipline our imagination, and there is a case to be made that one of the effects of being a good (and specialized) historian is that one's imagination is just so disciplined. But even if we do that, can such imaginings be anything more than an aid to the historian in the context of discovery, doing no more than functioning in the construction of claims that must themselves still be

12 Ferguson, "Introduction," 86 (italics omitted).
13 Note to philosophers: our interest here is epistemic, not semantic, qua these counterfactual claims.
settled by evidence? If not, we are back to the question of evidence again when it comes to counterfactuals. But perhaps plausible counterfactuals bear certain evidential markings that we can learn to read.

Philip Tetlock and Aaron Belkin certainly seem to think they do, and they offer six criteria of good counterfactuals. One of the problems of counterfactual reasoning in an even partially deterministic world is that one change begets another. Considering what would have happened but for the occurrence of an event leaves us with a choice that ranges between keeping everything the same or changing all the interconnected events that led to that event. Where we come down on this choice will have repercussions on whether the consequent of the counterfactual follows from the antecedent. All other things being equal, if the partisans of Vilna had had a nuclear device at their disposal, they surely would have prevailed. But changing the past to make the antecedent plausible quickly deprives us of our sense of certainty. After all, they surely would not have had such a device available to them unless others did too, and we are off to the races. In this sense, a counterfactual inference is only as good as the assumptions that one makes about the background conditions (what philosophers call “co-tenability”). But, given the same background conditions, why should we favor one counterfactual inference over another? Demanding consistency with established theoretical and statistical generalizations seems like an eminently plausible condition of adequacy, but notice that this is so only assuming that those generalizations have a property that philosophers call “projectability.” Compare the generalization that all the coins in my pocket are dimes with the generalization that all objects fall to earth accelerating at thirty-two feet per second per second. Both are true generalizations, but the first is accidental. The second is derived from a (Newtonian) law. The first does not carry the implication that the nickel on my desk would have been a dime were it in my pocket. Rather, if it were in my pocket, the generalization would simply be false. But notice that the second generalization does carry the implication that if the nickel had fallen off my desk, it would have fallen to earth accelerating at thirty-two feet per second per second. In this sense, the first generalization is not projectable beyond its founding instances, while the second is, and it is so beyond what has been both to what will be and what would have been. That is to say, generalizations that will support predictions will also support counterfactuals.

16 This reflects the way in which counterfactual claims in history based on imagination might well be thought of as a subset of thought experiments. In “The Logic of Thought Experiments,” Synthese 106 (1996): 227–40, I argued that (at least many) thought experiments are unreliable. But I argued that this notion of unreliability has to be understood relative to the goal of thought experiments as knowledge-producing. However, thought experiments play a varied role, and knowledge production is a goal only under quite limited circumstances.

17 Philip Tetlock and Aaron Belkin, “Counterfactual Thought Experiments in World Politics: Logical, Methodological, and Psychological Perspectives,” in Tetlock and Belkin, Counterfactual Thought Experiments, 16–31. Here I have collapsed some of Tetlock and Belkin’s six criteria. Moreover, I have treated their condition of theoretical or statistical consistency as a virtue only to the extent that they are “projectable.” One condition of theirs I have not included is that the antecedent of good counterfactuals should require changing as few historical facts as possible. I’ll take this up in the discussion of the virtues of antecedents later.
But even if all of this is great news for counterfactuals in physics, how does that help historians who lack laws of nature? Still, just because historians don’t explain events by reliance on laws of history, it does not follow that some explanations don’t draw on laws of specific disciplines. Trivially, as we act, we act as physical agents, and are hence subject to physical laws. Whatever else its causes or effects, this much is true: the assassination of Archduke Ferdinand involved a shot that was “governed” by the generalizations of physics. That that is so is so trivial that we don’t bother to mention it. But we do in some cases, as in the account of the collapse of the first Tacoma Narrows Bridge as provided by Albert Gunns.18 (Here and in subsequent examples, the reader should be aware that I make no claim to any historical knowledge about the cases cited beyond the references given!)

The first Tacoma Narrows bridge collapsed on November 7, 1940, only four months after construction was completed—blown down by winds of forty-two miles per hour. The deck of the suspension bridge rippled and gyrated wildly. At 11 a.m., sections of the center span fell away, the cables slackened under the lowered weight of the bridge, and the main towers “snapped shoreward.”19 This dramatic ending to the bridge’s short life may have been more dramatic than its previous behavior, but the bridge itself was never stable. Engineers had noted movement during construction, but inspectors declared the bridge safe. Safe, despite ongoing undulating movement even after the roadway was opened. “Even on relatively calm days motorists often experienced the unsettling sensation of seeing the car ahead momentarily disappear from view in a trough caused by the rippling of the deck.”20 Numerous investigations were held after the bridge’s demise, and there was consensus that the problem was a matter of design. That said, just what it was about the design that was wrong had two variants: the narrowness of the bridge and the solidity of the side plates.

Planning for a Tacoma bridge began in the 1920s, but it was not until the 1930s that attempts were made to raise funds—initially about 3.5 million dollars. Federal participation was denied in part on the grounds that the construction cost estimates were too low. By the time funding was approved in 1938, federal participation came in at 50 percent of the total budget, which ended up at $6.4 million. But even that budget created a substantial incentive for cost savings in design wherever possible, given the scope of the project. The main cost-saving feature of the final design was a two-lane deck, which was narrower and lighter than the decks of the George Washington and Golden Gate bridges, planned by the same company. As Gunns puts it, “had the roadbed been more than two lanes wide, as was the case with other long suspension bridges, normal design practice would have resulted in a deck that was heavier and more rigid and therefore less susceptible to aerodynamic effects.”21 Those effects were compounded by the side plates, which were solid rather than open, thereby creating much greater wind turbulence and subjecting the bridge to much

19 Gunns, “First Tacoma Narrows Bridge,” 163–64.
20 Gunns, “First Tacoma Narrows Bridge,” 163.
21 Gunns, “First Tacoma Narrows Bridge,” 165 (italics mine).
greater torsion forces. Indeed, when the bridge was reconstructed, it was four lanes wide with open sides, and it has never undulated.

Notice what does the work for us in this case. The plausibility of the counterfactual is carried by the plausibility of the laws of mechanics from which it is derivable. (Strictly speaking, the consequent is derivable from the conjunction of the laws and an antecedent.) Those laws gain their plausibility from their factual instantiations. In that sense, while the counterfactual has no direct evidence, it is connected indirectly to evidence via the laws on which it depends. Notice also that no act of imagination is involved here. It is not that we have to rely on laws to constrain or limit our imagination. Imagination per se never enters into the picture in the first place. Instead, it is the law that "connects" the consequent of the counterfactual to the antecedent.

Laws, even those of disciplines outside history, that get made use of in history may be few and far between, but perhaps not as few as one might think in the areas of population, epidemiological, and economic history. If we want to lean on these laws, we can decide if it is true that if rural migration had been lower, the growth of cities would have been largely unaffected, or if seventeenth-century containment policies had been instituted earlier in Northern Europe, they would have been successful in foreshortening the course of the Plague. And, most famously, how American growth would have been different if the railroads had not been invented, even if you could not do so specifically by way of the claim that growth from the East Coast would have been directed more to the South than the West.

But where the reach of laws ends, can we rely on anything other than imagination if we want to reason counterfactually?

In August of 1944, the U.S. army established a beachhead at Avranches. General Omar Bradley's forces were arrayed to the west of German forces commanded by Field Marshal Günther von Kluge. By O. G. Haywood, Jr.'s account, Kluge was faced with a strategic choice of withdrawing to the east with the aim of establishing a superior defensive position or attacking to the west with the aim of cutting off U.S. forces and establishing his western flank at the sea. Bradley's Third Army (under Patton) was heading south and east, while his First Army was holding Kluge's forces to their west. Bradley faced a decision about what to do with his uncommitted forces—four divisions. He could use them eastward as an adjunct to the Third Army to attack the German forces, or he could use them as an adjunct to the First Army to defend the gap. Finally, he could reserve the decision about how to commit them between these two alternatives for one day.

Kluge decided to withdraw, and Bradley decided to reserve his decision for one day.

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Both of these decisions can be understood in game theoretic terms as the rational course of action to take. Be that as it may, in fact, Hitler ordered Kluge to attack and attempt to drive to the sea. The Allied forces withstood the attack during the first day without reinforcement, and the four divisions being held in reserve were committed to drive east on the second day. By the time the German forces began to withdraw, they were nearly surrounded. Bradley’s choice and the rationality of it did not depend on Kluge’s choices. His choice was rational even in the light of Hitler’s overriding decision because it did not depend on a judgment of Kluge’s intentions. In fact, Bradley’s decisionmaking was based on a general U.S. military doctrine to “select the one [course of action] which offered the greatest promise of success in view of the enemy’s opposing capabilities.” That said, as Haywood asserts counterfactually, “had Bradley acted on an estimate of the enemy intentions, he would have placed the gap in jeopardy” (given Hitler’s decision to override Kluge). That is to say, if Bradley had judged that Kluge would withdraw and based on that assessment had sent his uncommitted forces east on the first day, when in fact Kluge tried to move to the sea under Hitler’s orders, the First Army would have stood alone without reinforcements available.

Here, there are no laws of nature. But considerations of rationality stand in for them. What makes their application reasonable is the reasonableness of the constraints that are assumed to govern how the historical actors see their choices. At the crudest level, in war, the actors aim to win. But we also employ this kind of reasoning every day in attributing motives and predicting the actions of others, because we assume them rational. Positing a person’s wants and beliefs, we can make inferences about how he or she would have acted based on the assumption that agents will choose the best way (by their lights) to achieve their wants based on their beliefs. The problem in extending such an account in history beyond the battlefield has to do with the confidence with which we can specify what historical actors wanted and believed. But that is not to say that such cases do not exist.

26 At least by the lights of a “maximin” strategy in which each party chooses the best of the worst positions available. Following Haywood (“Military Decision,” 375–77), assume Bradley orders his preferences as: gap holds, possibly Germans encircled (1), strong pressure on German withdrawal (2), moderate pressure on German withdrawal (3), weak pressure on German withdrawal (4), gap hold (5), gap cut (6); while for Kluge the preference ordering is reversed. The decision matrix can then be represented as follows:

<table>
<thead>
<tr>
<th>Kluge’s choices:</th>
<th>withdraw</th>
</tr>
</thead>
<tbody>
<tr>
<td>attack</td>
<td>gap</td>
</tr>
<tr>
<td>reinforce gap</td>
<td>5</td>
</tr>
<tr>
<td>move reserves eastward</td>
<td>6</td>
</tr>
<tr>
<td>hold reserves for day</td>
<td>1</td>
</tr>
</tbody>
</table>

(For Bradley, 1 > 2 . . .; for Kluge, 6 > 5 . . .)

As Haywood points out, Bradley’s maximin (*) does not coincide with Kluge’s (**)—which creates a differential advantage to a player willing to assume a less conservative strategy that depends on “betting” on the other players’ intentions. This forms the formal basis for the counterfactual discussed in the text that follows.

Late medieval Genoese communal structure was organized around clans that had no supra-organizational form that provided for regulation. Be that as it may, Avner Greif argues, the clans were able to marshal both economic and military resources on behalf of the community in cooperation with each other when it suited their individual interests.\(^{29}\) At the same time, those very resources could pit clan against clan when, again, self-interest so dictated. Self-interest certainly did dictate cooperation for Genoa when it came to economic growth, since growth depended on overseas possessions that could not be acquired by clans acting on their own. Such possessions were acquired by a complex quid pro quo in which the Genoese provided naval and military protection to overseas entities in exchange for a variety of economic and commercial concessions. (From 1098 to 1110, such arrangements existed between the Genoese and the Crusader states. But in 1170, when Saladin evicted the Crusaders from Egypt, instead of being evicted with them, he negotiated new concessions with the Genoese.)

During the period 1099–1162, not only was there external cooperation but also internal peace. Political order was maintained in Genoa without inter-clan struggles for supremacy. In the absence of a higher political order, how should we understand the basis of this peace? “[W]hat were the factors fostering or diminishing each clan’s motivation to maintain political order in advancing Genoa’s economy rather than using military force against the other clans to gain political supremacy over the city?”\(^{30}\) Demonstrating the existence of such a self-enforcing mechanism in a time of cooperation and peace is a tricky business. It is not as if one can point to instances where conflict actually broke out or even threatened to break out and then mechanisms were put into play to stop or avert that conflict. That there was a self-enforcing mechanism can only be established to the extent that the situation instantiates the features of a well-established theory—a theory that then allows us to specify how that self-enforcing system would have behaved had things been other than they were. So the claim that a self-enforcing system obtains under such circumstances is not just to make a claim about how that system did behave but also how it would have behaved.

Avner Greif’s way of demonstrating that such a system obtained in Genoa is to show that to assume otherwise would render the actions of the Genoese as not rational. How does all of this theory play out in Genoese history? The period 1099–1162 was one of relative tranquility for Genoa and one of clan cooperation. But it was also a period in which the city acquired few possessions, even though it had a “command of the sea” (Benjamin of Tudela quoted by Greif\(^ {31}\)). Instead, it used that dominance to “make predatory raids... and bring back the spoils.”\(^ {32}\) Greif argues that what made the less profitable raids the rational choice over acquisition of foreign possessions is only apparent if one takes into account the unstated costs of a self-enforcing system.

On the model he proposes, when clans cooperate in mounting military ventures

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to gain possessions, they incur costs that must be offset against their mutual gains. If a clan mounts a challenge against another, then there is a gain to be had from not having to share the net benefit of the possessions, but there are also the increased costs of mounting a military campaign against the other clan. The core assumption of self-interest suggests that there will be no inter-clan rivalry when and only when those increased costs are the same as or more than the increased share in the net benefits. For Genoa to grow, it had to raid and acquire possessions. Clans were not strong enough to do this on their own. Hence the need for cooperation in organizing military ventures. Once a possession had been acquired, each clan had a choice of whether to attack other clans in an attempt to expropriate all of the ongoing flow of resources from that possession. In that sense, the cost of acquiring a possession for a clan included not only the cost of its share of the military requirements for the raid but also the ongoing costs of deterring other clans from expropriating the resources of the resulting acquisitions. For the purposes of argument, assume clans first raided the most lucrative prospective possessions and proceeded in further acquisitions following the same principle. Then at some point, the cost of acquisition would be equal to or greater than the benefit of an acquisition.

As such, a self-enforcing system ought to be expressed in the following empirical way: If possessions are ordered from most beneficial to least, the point at which it makes no economic sense to acquire new possessions will be reached earlier than it would have been without such a system in place. The reason is that the marginal cost must be less than the benefit of the new acquisitions, and a system that includes deterrence has higher costs than one without it. For the same reason, the model also predicts something else. A raid may result in the acquisition of spoils directly or in the acquisition of a possession that will yield ongoing benefits. Even though the latter may be worth more than the former in the long run, raids, unlike acquisitions of possessions, require no outlay for ongoing deterrence. As such, as the marginal benefits of acquiring new possessions approach true costs (including deterrence), the model predicts that raids ought to dominate. Quod erat demonstrandum.

IF THE ABOVE ARGUMENT IS CORRECT, we can lean on more than laws in talking about counterfactuals: where considerations of rationality are in play, they will serve just as well. But even then, we do not have much of a range within which to make such assertions when it comes to historical practice. Considerations of rationality, much as laws, don’t make enough of a showing. Still, if we attend to the argument so far, we can see how to extend it. The reason we can rely on a law or considerations of rationality to ground a counterfactual is because counterfactuals are implicitly implied by laws and considerations of rationality. The mere assertion of a law carries all sorts of counterfactual implications. All we are doing is singling out one of interest. But with that in mind, remember Fogel’s assertion about causation and the Civil War: that “[e]very historian who has set out to deal with the causes of the Civil War . . . has implicitly or explicitly presumed what would have happened to
slavery if some events had unfolded in a way that was different from the actual course." He can say that because, implicit in every causal assertion, there is a set of counterfactual implications. So, if we can choose to make the counterfactuals implicit in laws and rationality the object of our interests, why not here, too? If we do that, our reach will be considerably extended to the extent that we can rely on our causal judgments. This last point is important. On the model I am offering here, the problem of the basis for deeming counterfactual judgments to be reliable is a function of the reliability of our claims on which we base the counterfactual judgments. If we base a counterfactual judgment on a causal claim, then the question is, how reliable is that causal claim? Whatever the answer to that may be, the purposes to which we put our causal judgments are irrelevant. That is to say, most of the time in history, we make causal judgments and we have a community standard for when we take those judgments to be reliable. Here, all we do is put those judgments to a different purpose than those usually pursued by historians. That being so, the acceptability of counterfactual claims in history based on causal considerations should be no different than the acceptability of causal claims themselves in history.

The counterfactual claim that "If Jackson had not vetoed the re-charter of the Second Bank of the United States in 1832, the inflation crisis of the 1830s would not have taken place" stands or falls straightforwardly from the causal claim that his veto did cause the inflation crisis of the 1830s—niceties aside (like preemption).

But wait! How do we ground such causal judgments? Of course, we are in trouble if we do so in circular fashion by appealing to just the counterfactual that interests us. That is, we can't make our basis for the claim that Jackson's veto caused the inflation crisis of the 1830s our counterfactual judgment that, if he had not vetoed the re-charter of the Second Bank, the inflation crisis of the 1830s would not have taken place. But then, even if we avoid such circularity, how do we make such causal judgments? In history at least, the overwhelming answer is that we do so informally. Indeed, the same is true when it comes to considerations of rationality as well. But then to the extent that the community standard of the discipline allows for causal claims and considerations about rationality to be arrived at informally, we should expect no more about counterfactual claims based on those very considerations. In this sense, we have much more to go on than Geoffrey Hawthorn thinks when he defends counterfactual reasoning in history based on (only) considerations of practical reasoning. Instead, historians draw on a wide array of what philosophers call "folk theories," beyond just physics and economics, that are no different than what we do in our everyday life in constructing inferences about the world.

There is a temptation here to look for poor relatives of the tools available to the

33 Fogel, *Without Consent or Contract*, 413.
34 Following Temin, *Jacksonian Economy*.
35 That is, x's causing y prevented z from doing so, and z would have done so if x had not. So it does not follow straightforwardly that, but for x, y would not have happened even though x caused y. Although the details are messy, there is a way to save the counterfactual implications by finegraining the situation to render the outcome by z to be different from the outcome from x.
36 Hawthorn, *Plausible Worlds*.
laboratory scientist, rather like what might be called Mill’s Methods on the Cheap. Under the fully controlled37 circumstances of a laboratory, we make causal inferences when we note that two outcomes are the same but share only one common antecedent (the Method of Agreement) or that two outcomes are different and differ in only one antecedent (the Method of Difference). Lacking a laboratory, the next best thing for historians to resort to is comparative history, and while those comparisons are far too messy to allow for just one difference, under some circumstances they can yield a basis for causal claims.38

Still, what is of much greater importance is the need to pay attention to the variety of subtle bases for grounding counterfactual reasoning in historical accounts that may be hidden by its lack of formalism. There is little doubt that the damming of New England rivers caused the collapse of migrating fish stocks in the late eighteenth and nineteenth centuries as industrialization took place. But would the fish stocks have survived without industrialization? Daniel Vickers argues not, on the basis of a study of the fishing culture of New England from the seventeenth century on.39 His argument does not depend on any assumptions about the technology of fishing beyond what was already in hand. Instead, the argument depends on a cultural claim about the preindustrial New England economy: “Men in particular felt morally bound in the heart of their being, as fathers charged by God and the state ‘to provide Competent and convenient food and raiment for their Children’ and to assemble the property their offspring would require one day ‘to be for themselves,’ to extract more from their surrounding environment than that environment could in the long run sustain.”40 I can’t do justice here to the narrativist structure of Vickers’ account, which draws on diary entries of the period. But to the extent that history explains by narrative, his account is extended into counterfactual space as a projection of his factual narrative.41 Why have confidence in such a projection? In large part, I suspect, because his topic is Braudelian in its dimensions in the way it taps into some of the most stable cultural or economic parameters of the period.

There are two features of Vickers’ counterfactual that are worth noting in passing: its subject is how things would have been the same, not different, had the antecedent not taken place. And although the counterfactual involves agents, it makes no demands as to how they viewed the alternative in question. Both underscore the potential range of interesting counterfactual questions that we can ask. If the rivers of New England had not been dammed, the fish would still have gone extinct. So far, so good. But how plausible is the antecedent of this counterfactual consideration in the first place? How plausible is it to think that if things had not gone the way they did, the rivers would not have been dammed?

37 Speaking with the vulgar from the point of view of a philosophical skeptic.
38 Primarily based on our ability to discount most of the differences between the cases as irrelevant based on our general knowledge derived from other cases.
41 The philosophical locus classicus is Arthur C. Danto, Narration and Knowledge (New York, 1985), orig. pub. as Analytical Philosophy of History.
Notice how this reformulation of the question wears its colors on its sleeve: it is itself a counterfactual. The seeming distinction between the historian’s interest in the plausibility of the antecedent of a counterfactual and the philosopher’s attention to the plausibility of the assertion that the consequent of the counterfactual follows from that antecedent is really specious—the first is just a special case of the second.

But why isn’t there an infinite regress here? If the plausibility of the antecedent of a counterfactual is itself settled by a counterfactual, what about the plausibility of that counterfactual, and so on? First, we should notice that, in fact, not all historically interesting counterfactuals need to be plausible. If our primary interest is in understanding what difference an event made, whether or not its occurrence is plausible doesn’t really matter. (When slavery would have ended in the United States absent the Civil War is an interesting question independent from the equally interesting question of how likely it is that the war might not have taken place.) In this sense, it is a confusion to think that counterfactuals are incompatible with determinism. Indeed, some of the most interesting historical questions have to do with probing the stability of an outcome under a range of variations from the way things were. Here, our interest is not in how outcomes would have been different with changes in their antecedents but, rather, what changes in antecedents would have been compatible with the same outcome. But, that said, where we do care about plausibility, the regress looms large, especially if we assume determinism. So it is not surprising that historians focus on counterfactuals involving alternative choices that fall within “the horizon of possibility” of historical actors, since these are the most obvious cases that (one hopes) dodge such a regress. But they are far from unique in this respect. Nowhere is this more true than when it comes to matters of just plain luck. As Ernest May puts it in his account of Germany’s victory over France in 1940, “Even with their failures in executive judgment, French and British leaders might have been able to halt or at least check the German offensive had it not been for the extraordinary, almost incredible good luck on the German side and bad luck on theirs.”

I have argued that counterfactual reasoning plays an unavoidable implicit role in history. Where it plays such a role, historians always have the option of making it the object of their interest. In doing so, I have argued, indirect evidence, not

42 Contra, for example, Ferguson, “Introduction.”
44 The phrase comes from the late Tim Mason (in conversation with the author).
imagination, should be the basis for evaluating such counterfactual claims—even if that indirect evidence is based on the kind of informal considerations that form the basis for other kinds of judgments in historical scholarship. All such judgments confront the interconnectedness of our reasoning. Any one such judgment forces us to rely on others. To echo Willard Quine’s use of Otto Neurath’s metaphor, we are like sailors who must rebuild our ship at sea.\textsuperscript{46} I have argued that counterfactual judgments are only possible when we do just that. But the same is true of all other judgments made in the writing of history.

\textsuperscript{46} Willard V. Quine, \textit{Word and Object} (Cambridge, Mass., 1960), ii.

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\textbf{Martin Bunzl} is a professor of philosophy at Rutgers University, New Brunswick. He is the author of \textit{Real History} (1997) and \textit{The Context of Explanation} (1993), as well as numerous articles in the philosophy of science. With K. Anthony Appiah, he is currently editing a volume on the ethics and economics of slave redemption.