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Source: *World Politics*, Vol. 43, No. 2 (Jan., 1991), pp. 169-195

Published by: [Cambridge University Press](#)

Stable URL: <http://www.jstor.org/stable/2010470>

Accessed: 15/09/2014 14:24

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COUNTERFACTUALS AND HYPOTHESIS TESTING IN POLITICAL SCIENCE

By JAMES D. FEARON*

Without the prior democratic modernization of England, the reactionary methods adopted in Germany and Japan would scarcely have been possible. Without both the capitalist and reactionary experiences, the communist method would have been something entirely different, if it had come into existence at all.

—*Barrington Moore*

Nuclear weapons did not cause the condition of bipolarity. . . . Had the atom never been split, [the U.S. and the Soviet Union] would far surpass the others in military strength.

—*Kenneth Waltz*

THE epigraphs¹ provide examples of counterfactual conditionals, or propositions that take the generic form “If it had been the case that C (or not C), it would have been the case that E (or not E).” Counterfactuals make claims about events that did not actually occur. It is argued in this paper that such propositions play a necessary and fundamental, if often implicit and underdeveloped, role in the efforts of political scientists to assess their hypotheses about the causes of the phenomena they study. Particularly in small-N research designs, scholars in comparative politics and international relations routinely evaluate causal hypotheses by discussing or simply referring to *counterfactual cases* in which a hypothesized causal factor is supposed to have been absent. Though this procedure is quite common, its methodological status and its viability are unclear and are worth exploring. How does the strategy of counterfactual argument relate, if at all, to methods of hypothesis testing based on the comparison of actual cases, such as regression analysis or J. S. Mill’s Method of Difference? Are counterfactual “thought experiments” a vi-

* Prepared for delivery at the Annual Meetings of the American Political Science Association, San Francisco, August 30–September 2, 1990. For helpful comments and advice I wish to thank Chris Achen, Robert M. Adams, David Collier, Russ Faeges, Don Green, Marcus Kurtz, Jim Mahon, Merrill Shanks, Laura Stoker, Arun Swamy, and an anonymous referee. I am particularly indebted to David Collier for his encouragement and close reading of several drafts. None mentioned (or unmentioned) are responsible for my mistakes.

¹ Moore, *Social Origins of Dictatorship and Democracy* (Boston: Beacon Press, 1966), 414; Waltz, *Theory of International Politics* (New York: Random House, 1979), 180.

able means of assessing hypotheses about national and international outcomes, or are they methodologically invalid in principle?

The article has three principal aims. First, I seek to support the claim made above, that counterfactual propositions and arguments play a central role in the efforts of political scientists to assess their causal hypotheses. Support is drawn from methodological argument and from examples showing the counterfactual strategy at work in research in the areas of comparative politics and international relations. I discuss examples from the literature on the causes of World War I, the nonoccurrence of World War III, social revolutions, the breakdown of democratic regimes in Latin America, and the origins of fascist and corporatist regimes in interwar Europe.

Second, the paper examines how the strategy of counterfactual argument is related to but also differs from methods of hypothesis testing based on the comparison of actual cases. The two approaches are found to be closely related: analysts with few cases and many variables are compelled to resort to counterfactual argument by a *statistical* principle; and counterfactuals also appear to play a key role in the assumptions that justify large-*N* regression analysis, when the data employed is quasi-, or nonexperimental.² The difference between regression and the counterfactual strategy is not that one relies on counterfactuals while the other does not. Rather, the strategies differ in the way that each employs counterfactuals and in the way that each evaluates support for a causal hypothesis.

Finally, the paper addresses the question posed above: Is counterfactual argument a viable means of assessing causal hypotheses in nonexperimental research settings? I give no firm answer here but instead introduce some of the problems and issues involved. My purpose is neither to advocate the use of counterfactual argument in preference to comparisons with other actual cases nor to suggest that the counterfactual strategy is fundamentally invalid or has no value in principle. Since political scientists often use counterfactual argument when assessing or justifying causal hypotheses, particularly in small-*N* research settings, it seems important simply to understand what the strategy entails. If the paper carries a methodological prescription, it is that researchers who use counterfactual argument to support causal hypotheses should be methodologically aware of what they are doing and should make their counterfactual arguments as explicit and defensible as they can.

These general aims are pursued in three sections. The first distin-

² Data not generated by random assignment to control and treatment groups is referred to as quasi-, or nonexperimental.

guishes between two strategies of hypothesis testing—the comparison of actual cases and counterfactual argument—and examines key methodological similarities and differences between them. The second section shows how the counterfactual strategy appears in practice by considering examples from work in international relations and comparative politics. The examples make clear that counterfactuals matter both when the researcher is focusing on one actual case (for example, the outbreak of World War I or the Brazilian military takeover in 1964) and when the researcher considers several actual cases (for example, social revolutions or interwar European regime types). The third section returns to some theoretical issues concerning the link between causal arguments and counterfactual propositions, issues that bear on the question of whether the counterfactual strategy is a viable one. Two logical problems related to the use of counterfactuals are discussed briefly: (1) Is any event C that appears to satisfy “if C had not occurred, E would not have occurred” to be called a “cause” or E? And (2) Are some counterfactual comparisons more “legitimate” or appropriate than others?

COUNTERFACTUALS, ACTUAL CASE COMPARISONS, AND THE LOGIC OF INFERENCE

Suppose it is hypothesized that C was a cause of event E. I would argue that when experimental control and replication are not possible, analysts have available a choice between two and only two strategies for “empirically” assessing this hypothesis. Either they can imagine that C had been absent and ask whether E would have (or might have) occurred *in that counterfactual case*; or they can search for other *actual* cases that resemble the case in question in significant respects,³ except that in some of these cases C is absent (or had a different value). In the latter procedure, the analyst then checks the association between the occurrence of C and E in the set of actual cases.⁴ If successful (from the analyst’s point of view), both strategies would tend to support the hypothesis that the proposed cause in fact produces (or produced) the effect.

As an illustration, consider the hypothesis that international structural rather than domestic political factors have been the principal causes of

³ The sense of “significant respects” is discussed below.

⁴ These summary statements of the two strategies are not complete. Qualifications and elaborations for each are discussed in the rest of the paper, with more attention paid to the counterfactual case strategy. The potential difficulties with the method of comparing actual cases, which is formally known as regression analysis though informally practiced in such works as Theda Skocpol, *States and Social Revolutions* (Cambridge: Cambridge University Press, 1979), are extensively discussed in the econometrics and statistics literatures.

major aspects of Soviet foreign policy. The analyst applying the counterfactual strategy would evaluate the hypothesis by examining arguments that *any* regime in Russia, Soviet or not, *would have* made essentially the same foreign policy choices. The analyst using the actual case strategy would search for cases of states in both similar and dissimilar structural positions as Soviet Russia and then would check the sample for a relationship between structural position and foreign policies.⁵

Both methodological strategies aim to solve *the same statistical problem*. Our analyst begins with one case and at least one explanatory variable, which means negative degrees of freedom.⁶ Legitimate causal imputations cannot be made on the basis of negative degrees of freedom, so the analyst wishing to assess a causal hypothesis or to assess the relative weights of different causes has no choice but to add or create more cases: either a counterfactual case (or cases) that never actually existed or actual cases.

Put otherwise, the analyst, in explaining why some particular event E occurred, cannot help but explain why E occurred *rather than* some other possible outcome or outcomes. These other possible outcomes define the range of variation that the analyst accounts for, and this range is treated differently in different research traditions. For example, much historical analysis leaves implicit the other things that might have been had the historian's favored causes varied. In more methodologically self-aware small-N work, analysts tend to be more explicit about what might have happened.⁷ Finally, in the actual case strategy, analysts take their cues about what might have happened from other actual cases. Thus, an elections specialist may explain why a respondent voted Republican rather than Democratic (as did other actual respondents); students of international conflict may explain why deterrence failed in one actual case but not in other cases; experts in comparative politics may explain why interwar Germany became a fascist dictatorship rather than a liberal de-

⁵ "Structural position" here would entail the number of great powers and the basic geopolitical circumstances of the Soviet Union. Waltz (fn. 1); and idem, "Another Gap?" in Robert Osgood et al., *Containment, Soviet Behavior, and Grand Strategy*, Policy Papers in International Affairs No. 16 (Berkeley: Institute of International Studies, University of California, 1981). On structural versus domestic political or ideological explanations of Soviet foreign policy, see also Barry R. Posen, "Competing Images of the Soviet Union," *World Politics* 39 (July 1987), 579-97.

⁶ Degrees of freedom are the number of cases minus the number of explanatory variables minus one.

⁷ For example, Barrington Moore, *The Social Bases of Obedience and Revolt* (London: Macmillan, 1978); Stephen Van Evera, "The Cult of the Offensive and the Origins of the First World War," *International Security* 9 (Summer 1984), 58-107. Of course, historians can be quite careful about their counterfactual arguments. For examples, see McGeorge Bundy, *Danger and Survival* (New York: Random House, 1988); and George Kennan, *Russia and the West under Lenin and Stalin* (Boston: Little Brown, 1960), 29-32.

mocracy like England or (taking a larger range of other actual cases) a social democracy like Sweden or a traditional dictatorship like Austria in the Dollfuss-Schuschnigg period.⁸

Not only do counterfactual and actual case strategies both attempt to solve the same statistical problem, but both also run important methodological risks. Less obvious is that in each of the strategies, the principal risks are closely connected to the role played by counterfactuals.

The main risk in the first strategy is obvious and serious—how can we know what would have happened with any degree of confidence? Historians, when confronted with the suggestion that the validity of their causal inferences necessarily depends on counterfactual argument, have often dismissed out of hand or ignored the idea in favor of the view that their job is to deal with reality.⁹ Political scientists and sociologists, too, with the exception of a neglected methodological piece by Max Weber and some recent work by Jon Elster, have also tended to avoid explicit discussion or open embrace of the counterfactual strategy, probably because it is felt that an empirical political science must deal only with actual cases. This belief would seem to be reflected in the title of a recent book of essays by political scientists working with counterfactual premises: *What If?: Essays in Social Science Fiction*. The play on “science fiction” is no accident here.¹⁰

The risks of the second methodological strategy—that of increasing the degrees of freedom by considering other actual cases—are also well known. Whereas in the counterfactual approach one tries to imagine another (not actual) case in which the presumed causal agent is absent but everything else that is relevant is identical, in the second strategy the analyst adding actual cases may not know if the additional cases are appropriately identical. If there are other causes of the phenomenon in

⁸ On deterrence, see Paul Huth, *Extended Deterrence and the Prevention of War* (New Haven: Yale University Press, 1988); on fascism versus liberalism in Germany, see Moore (fn. 1); on fascism versus liberalism, corporatism, or traditional dictatorship, see Gregory M. Luebbert, “Social Foundations of Political Order in Interwar Europe,” *World Politics* 39 (July 1987), 449–78.

⁹ For example, according to A. J. P. Taylor, “a historian should never deal in speculations about what did not happen”; Taylor, *The Struggle for Mastery in Europe, 1848–1918* (London: Oxford University Press, 1954), 513. Or, in M. M. Postan’s words, “The might-have-beens of history are not a profitable subject of discussion”; quoted in J. D. Gould, “Hypothetical History,” *Economic History Review*, 2d ser., 22 (August 1969), 195–207. See also David Hackett Fischer, *Historians’ Fallacies* (New York: Harper Colophon Books, 1970), 15–21; and examples given in Peter McClelland, *Causal Explanation and Model-Building in History, Economics, and the New Economic History* (Ithaca, N.Y.: Cornell University Press, 1975).

¹⁰ Weber, “Objective Possibility and Adequate Causation in Historical Explanation,” in *The Methodology of the Social Sciences* (New York: Free Press, 1949); Elster, *Logic and Society: Contradictions and Possible Worlds* (New York: Wiley, 1978); citations below (fn. 56); and Nelson Polsby, ed., *What If?: Essays in Social Science Fiction* (Lexington, Mass.: Lewis Publishing, 1982).

question that are not considered explicitly in the analysis, and if any of these are in fact systematically related to the causes explicitly considered, then effects of the other causes will be wrongly attributed to those of the causes that are being evaluated. Simply put, estimates of the effects of the proposed causes will be biased. In statistics this is the familiar problem of whether any independent variables are correlated with the contents of the error term (which contains the effect of all unspecified, unmeasured “other causes”). Such correlation may occur due to failure to include relevant independent variables, errors in measuring the independent variables, or unrecognized reciprocal causation. In the comparative politics literature it is often posed as the question of whether a researcher’s several cases are comparable, or if the *ceteris paribus* assumption is adequately satisfied.¹¹ Because of the severity of this risk, some analysts tend to be skeptical of large-N or comparative historical work; they prefer case studies in which the risks of (an often implicit) counterfactual strategy may seem intuitively less serious.

Less well understood is the link between this central risk run by the actual case strategy and counterfactuals. While the paper focuses primarily on the role of counterfactuals in small-N research, a few words on their role in quasi-experimental regression analysis are useful as a prelude to making clearer exactly how the two strategies differ.

To support a causal interpretation of estimated regression coefficients, the large-N analyst using nonexperimental data needs to make a number of theory-driven assumptions.¹² As noted, chief among these is the as-

¹¹ The notion of comparability plays a major role in the methodological and applied writings of specialists in comparative politics. My impression is that nonetheless the notion remains a deeply vague one. It seems to include, at various times, the idea that the other causes should be uncorrelated with the independent variables ($E(X'e) = 0$); that everything else should have as little influence as possible ($E(e'e)$ should be close to zero); that measures will not be as valid or reliable across countries and cultures; and other meanings. (Throughout, $E(\cdot)$ is the expectations operator; X is an $n \times k$ matrix of n observations on k independent variables; e is an $n \times 1$ vector of error terms.)

Posing the main risk for analysis across sets of actual cases in terms of the regression validity of the *ceteris paribus* assumption also bears qualification. For regression estimates to be unbiased, we do not need the other things to be literally equal, though it is true that the more equal they are, the greater the precision of our estimated effects. For unbiased estimates of causal effects we need only require that the other things not be systematically related to the prospective causes and the dependent variable that we are evaluating. This point appears not to have been fully clear to Mill (working before statistics was well developed), who sometimes writes in his *System of Logic* (London: John W. Parker, 1851) as though everything else has to be literally identical in order for the Method of Difference to work. The same confusion seems to carry over today in the work of some specialists in comparative politics who take Mill as a principal methodological guide (e.g., Theda Skocpol and Margaret Somers, “The Uses of Comparative History in Macrosocial Inquiry,” *Comparative Studies in Society and History* 22 [April 1980], 174–97). That said, I should also note that those who conduct large-N research often do refer to this assumption as the “*ceteris paribus* assumption” simply for convenience, and I will follow this usage here.

¹² This is true as well of actual experiments in which cases are assigned at random to

sumption that explanatory variables and the errors (the other causes) are uncorrelated. Formally, the argument that estimated coefficients are unbiased depends on the assumption that $E(X'e) = 0$. It is easy to show that this assumption is credible *if and only if* a counterfactual proposition is credible; namely, the proposition

(P_I) If the cases in the sample had assumed different values on the independent variables, the contents of the error term would not have differed systematically.

If P_I is false, then $E(X'e)$ does not equal zero. If $E(X'e)$ does not equal zero, P_I cannot be true (Q.E.D.).

This argument says that assuming that $E(X'e) = 0$ in a quasi experiment is *equivalent* to assuming the truth of a counterfactual proposition about what would have happened if we could have altered a variable's value for any case in the sample. Although one may not think about the *ceteris paribus* assumption in terms of a counterfactual proposition, a counterfactual proposition is necessarily involved nonetheless. In actual experiments random assignment guarantees the truth of P_I (within the limits of sampling variance). In quasi experiments, a causal interpretation of estimated coefficients requires belief in the credibility of the counterfactual P_I for justification. If we believe the results of a regression analysis, we must be willing to believe that, say, if Joe Respondent had been a Republican as opposed to a Democrat, he would have been roughly "so much" more likely to have voted for Reagan in 1984; or that if Israel had not moved troops quickly to its northeastern border in September 1970, then Syria would have been much less likely to have been deterred from advancing on the Jordanian capital.¹³

If both strategies of confirmation are means of solving the same statistical problem, and if both depend in some measure on counterfactuals, then how do they differ? The answer is that each strategy provides its "empirical" confirmation for a causal hypothesis in a different way.

In the actual case strategy support for a hypothesized causal connection comes principally in the form of a frequency or magnitude of association across actual cases. Of course, theory-driven assumptions—among them a counterfactual one—are needed to support or justify any regression result. But the result *one looks for* in regression analysis is an

treatment and control groups. See Leland Neuberger, *Conceptual Anomalies in Economics and Statistics: Lessons from the Social Experiment* (Cambridge: Cambridge University Press, 1988). Among other things, Neuberger shows that a counterfactual assumption is needed to justify estimates of sampling variance in actual experiments. I suggest below, however, that counterfactuals play a key role in quasi-experimental hypothesis testing that they do *not* play in actual experiments.

¹³ The later example comes from Huth (fn. 8), 97, who often uses counterfactual argument about particular cases to make more plausible the results of his regression analysis.

estimated coefficient significantly different from the null hypothesis, and this difference derives from a frequency of association in the sample.

In the counterfactual case strategy, by contrast, frequencies of association cannot be meaningfully assessed. They are arguably irrelevant in any event, since the researcher is attempting to perform the perfect experiment, in which everything but the test factor is equal. **Instead, support for a causal hypothesis in the counterfactual strategy comes from arguments about what would have happened. These arguments are made credible (1) by invoking general principles, theories, laws, or regularities distinct from the hypothesis being tested; and (2) by drawing on knowledge of historical facts relevant to a counterfactual scenario.**¹⁴

An example will help make this point concrete. It has been proposed that a “cult of the offensive”—the widespread conviction held by European civilian and military leaders that there were enormous strategic advantages to striking first—was an important cause of World War I.¹⁵ According to the analysis above, there are two means of empirically checking this hypothesis. Following the actual case strategy, we could assemble a set of international disputes, some of which escalated to war and some of which did not. We could then construct a measure of military and civilian beliefs about the advantages of a first strike, presumably from military writings and from statements of politicians and generals about their expectations for war. Finally, after thinking hard about what other independent variables required statistical control, we could test for the strength of association between commitment to offensive doctrines and escalation. To assess the contribution of this cause to the likelihood of World War I in particular, we would check the value of the several independent variables for this case, comparing their various contributions with that of belief in first strike advantages.¹⁶

Alternatively, we might employ the counterfactual case strategy,

¹⁴ I am relying here on what David Lewis calls “metalinguistic” theories of counterfactuals. These hold that “a counterfactual is true, or assertable, if and only if its antecedent, together with suitable further premises, implies its consequent”; Lewis, *Counterfactuals* (Cambridge: Cambridge University Press, 1973), 65. The “further premises” may include both facts and causal laws, or “lawlike generalizations.” For example, the counterfactual “If that match had been struck, it would have lit” is true given the existence of certain laws concerning sulfur, oxygen, friction, and heat, plus certain factual conditions, including a dry match, presence of oxygen, etc. A counterfactual is thus a “condensed or incomplete argument” (J. L. Mackie, “Counterfactuals and Causal Laws,” in R. J. Butler, ed., *Analytical Philosophy* [Blackwell: Oxford, 1962], 68). There are other accounts of what makes a counterfactual true (or assertable), based on notions of distance between “possible worlds”; see Lewis.

¹⁵ Van Evera (fn. 7); Jack Snyder, “Civil-Military Relations and the Cult of the Offensive, 1914 and 1984,” *International Security* 9 (Summer 1984), 108–46.

¹⁶ Of course, each step of this process—from identifying a sample to interpreting relative importance—is fraught with methodological peril. *Both* strategies, it should be emphasized, are risky.

which often goes under the name “case study.”¹⁷ In this instance, careful researchers would make an explicit effort to imagine the prewar world without a cult of the offensive but otherwise similar. They would then construct an argument showing that the outbreak of a general war would have been much less likely in the counterfactual case. Such an argument would depend for its credibility on the principles and historical knowledge used to draw the picture of what would have happened. Stephen Van Evera adopts precisely this strategy to support his cult of the offensive hypothesis, relying at bottom on general principles of rationality.¹⁸ He asks, in essence: How would statesmen have behaved if they had believed that defense rather than offense had the advantage? A reconstruction of what rational actions would have followed from these beliefs yields the conclusion that escalation would have been much less likely in a crisis like that of July 1914 (that is, in the counterfactual case).

The difference between the two means of hypothesis testing would thus appear to be quite stark, and on one level it is. In the counterfactual strategy the analyst supports one causal hypothesis by *invoking others*—laws, regularities, or principles that are taken as having some independent credibility. In the actual case strategy no other principles need to be invoked directly to support the causal hypothesis: only a strength of association across actual cases matters. Indeed, from this vantage point the counterfactual strategy for “empirically” checking a causal hypothesis seems only indirectly empirical, since the confirmation it provides depends principally on other *theories*, which are presumably themselves supported by empirical evidence from actual case comparisons.

On what may be a deeper level, this apparently central difference between the two strategies seems less sharply drawn. As noted, when the actual case strategy is employed in a nonexperimental setting, the validity of a causal interpretation of the results is contingent on the truth of a counterfactual assumption about the other unspecified, unmeasured causes. We must be ready to accept the proposition that had variable X taken values different from those in the sample, no such other causes of the dependent variable would have been systematically different as well. Our confidence that the other causes would not vary with the independent variables depends on our confidence in our theory about what the

¹⁷ I want to suggest that counterfactual reasoning must underlie efforts to infer or assess the relative weights of causes in case studies where the analyst’s degrees of freedom in the actual world are negative. In practice, those who use case studies often resort as well to causal comparisons with other actual cases (e.g., “Whereas in many other African countries . . . , in Kenya . . .”) and testing multiple implications of a theory; see Donald Campbell, “‘Degrees of Freedom’ and the Case Study,” *Comparative Political Studies* 8 (July 1975), 178–93.

¹⁸ Van Evera (fn. 7).

other causes are and about how they might be related to the variables being tested explicitly.

Two other contrasts between the counterfactual and actual case strategies should be noted. The first concerns the appraisal of relative causal weight. In the actual case strategy, such appraisals can be carried out in several ways, essentially by contrasting our estimates of the effects of different independent variables.¹⁹ Ultimately, we can do this because we have a sample from which relevant frequencies and magnitudes can be extracted. In the counterfactual strategy, by contrast, we have no concrete frequencies or magnitudes, and the degrees of freedom problem will bite every time we introduce a new variable that may have influenced the particular event to be explained. Explicit justification of claims about relative effects will require *a proliferation of counterfactual cases*.

Suppose, for example, a historian or political scientist wishes to argue that both A and B were causes of event E, but that A was more important than B. The above analysis would suggest that we now need not one but at least two counterfactual scenarios to support this claim. We would need to contrast a counterfactual case where A is present but B absent with one where B is present but A absent, and then invoke general principles and relevant facts to argue that E would have been more likely to have occurred in the first instance.²⁰

One might well object that such arguments about what would have happened in multiple counterfactual scenarios will be very imprecise and uncertain. The second contrast between the two strategies relates to this issue of "precision of estimates." In the actual case strategy when N is large, frequencies and magnitudes allow the researcher to get an idea of how much risk attaches to the belief that the true causal effect of a variable is as distinct from the null hypothesis as the results show. In the counterfactual strategy there is no such formal criterion for gauging the risk of error associated with some independent variable. All depends instead on the plausibility of arguments about what would have happened. As will be seen in the example of the debate on the origins of World War I, *arguments about the relative importance of possible causes become arguments about the relative plausibility of different counterfactual scenarios*.

¹⁹ There is, however, more than one meaningful sense to the idea of causal importance in a regression model. See J. Merrill Shanks, "The Importance of Importance" (Working paper, Survey Research Center, University of California, Berkeley, 1982); Christopher Achen, *Interpreting and Using Regression* (Beverly Hills, Calif.: Sage Publications, 1982).

²⁰ Some philosophers of history working on the problem of how historians can and should attribute causal weightings have proposed similar criteria. See Raymond Martin, "Causes, Conditions, and Causal Importance," *History and Theory* 21 (1982), 53-74, and citations therein.

The analysis in this section bears on current methodological issues in the field in at least two ways. First, scholars in comparative politics and international relations often argue that because statistical methods are inapplicable when we have few cases and many variables, other methods need to be developed to enable sound explanations; these include, among others, the comparative method, structured, focused comparisons, process tracing, and what are sometimes called “qualitative methods.” Following the analysis here, we would emphasize that statistical methods are inapplicable in these circumstances for a good reason, namely, the lack of enough cases to support a causal claim. Further, statistical logic implies that assessing a causal claim would require the addition of counterfactual or actual cases. Statistical principles do not simply cease to operate when the number of actual cases dips below twenty or fifteen or ten, creating room for alternative ways of testing causal hypotheses.

Second, researchers should choose between the two strategies of confirmation on the basis of the types of risks they are willing to run. Sometimes the counterfactual claims needed to support a causal inference seem entirely unproblematic. For example, we do not require a formal survey and regression analysis to support the claim that a gunshot through the heart caused the death. Less trivially, a researcher might be skeptical of regression analysis showing no clear relation between domestic political trouble and the initiation of war if it seemed clear from counterfactual reasoning that in a number of cases, domestic problems were a factor impelling the leadership to start a war.²¹ Where there are serious problems in identifying a sample, operationalizing and measuring variables, and conceiving of relevant controls, counterfactual argument about one or several cases may be more compelling than a statistical effort.

Indeed, understanding that one can try to explain counterfactual variation in single cases, as well as actual variation across actual cases, may help resolve some of the puzzle over how case studies function methodologically to assess theories and hypotheses.²² There is a substantial amount of work in political science where the analyst declares an interest in explaining phenomenon X (for example, war, revolution, democracy), chooses a set of cases where X actually occurred, and ends up drawing conclusions about the causes of phenomenon X. Those who engage in large-N analysis tend to view this procedure as totally invalid. Such analysts “sample on their dependent variables”; if they fail to include cases

²¹ Cf. Jack Levy, “Domestic Politics and War,” *Journal of Interdisciplinary History* 18 (Spring 1988), 653–73.

²² Another tack on this puzzle is taken by Campbell (fn. 17).

where X does not occur, how can they find causes that differentiate between outcomes? But if we see that each case study proposes causes that selected the actual outcome from a range of possible *counterfactual* outcomes in that case, we see the source of the “not Xs” and the variance such analysts account for. This does not justify the approach—particularly since it is usually performed unconsciously—but it does make some methodological sense of it.

COUNTERFACTUAL ARGUMENT IN PRACTICE

The most controversial point made above is probably that concerning the *necessity* of counterfactual argument for justifying causal claims in small-N settings. The approach is not put forward as simply another option on the menu for small-N practitioners. Rather, I have argued, the point is that when degrees of freedom in the actual world are negative, a causal claim *requires* argument about counterfactual cases for its justification (or addition of other actual cases). This section will consider some examples of how this logical constraint makes itself felt in practice.

One does not find counterfactuals playing central roles in *all* small-N political science research. My impression, after reviewing literature for examples and evidence, is that counterfactuals are most likely to be found performing confirmatory work in case studies where the analyst is explicitly concerned with giving a causal explanation for some event or phenomenon.²³ Of course, case studies may be used for other purposes, such as evaluating the performance of rival theories or simply giving information relevant to various theoretical concerns.²⁴ In addition, even in what is nominally a case study analysts often employ both strategies of confirmation.

I will first discuss the use of counterfactuals in three examples of $N = 1$ case studies.²⁵ Here counterfactual scenarios *must* be developed to support explicit causal claims and to support assertions about relative causal weight. Second, I will consider the role of counterfactuals in sev-

²³ E.g., Youssef Cohen, “Democracy from Above: The Political Origins of Military Dictatorship in Brazil,” *World Politics* 40 (October 1987), 30–54; Hyug Baeg Im, “The Rise of Bureaucratic Authoritarianism in South Korea,” *World Politics* 39 (January 1987), 231–57; Joanne Gowa, “Hegemons, IOs, and Markets: The Case of the Substitution Account,” *International Organization* 38 (Autumn 1984), 661–83.

²⁴ For examples, see articles in Frederic Deyo, ed., *The Political Economy of the New Asian Industrialism* (Ithaca, N.Y.: Cornell University Press, 1987).

²⁵ As the preceding discussion should suggest, an $N = 1$ case study in which causal inferences are drawn is, strictly speaking, impossible, since other counterfactual cases must be invoked to support causal claims. I use N here to refer to the number of cases in the actual world. On the idea of actual versus possible worlds, see Michael Loux, ed., *The Possible and Actual: Readings in the Metaphysics of Modality* (Ithaca, N.Y.: Cornell University Press, 1985).

eral $N > 1$ examples from comparative politics. Here analysts often make primary use of the actual case strategy when grounding causal assertions, but even then counterfactuals may be needed to justify inferences fully.

$N = 1$ EXAMPLES

Some of the clearest examples of the importance of counterfactual argument come from research on the causes of World War I. Over the years political scientists and historians have identified an enormous collection of possible factors,²⁶ which are typically argued to be causes on the following grounds: If cause X had not been present, the war either would not have occurred or would have been much less likely to have occurred. Thus, in arguing the causal importance of misperceptions in 1914, Robert Jervis writes, "Had the participants realized not only that the first offensive would not end the war, but also that the fighting would last for four punishing years, they might well have held back."²⁷ Note that Jervis relies on a rationality principle (sensitivity to war costs) to make credible the causal inference drawn from the counterfactual proposition.

On similar grounds, Van Evera has developed the thesis that a cult of the offensive was a major cause of World War I.²⁸ Further, he argues that military and civilian tendencies to glorify the offensive had the effect of "feeding or magnifying a wide range of secondary dangers" that other analysts thought were independent or unrelated causes. To establish this, Van Evera discusses the secondary dangers one by one, arguing in each case that had the cult of the offensive not been present, the secondary cause *would not have* operated with as much (or any) force. His conclusion nicely summarizes these counterfactual arguments. Throughout, Van Evera relies primarily on implicit rationality principles: he supposes leaders had different beliefs and then draws conclusions about appropriate or rational behavior given such beliefs.²⁹

²⁶ These include, but are not limited to, nationalism, imperialism, capitalism, social Darwinism, a fatalistic intellectual mood, the balance of power system, population growth, differential industrialization, a power transition, long cycles, tight alliances, multipolarity, misperceptions, psychological pathologies, leader personalities, essentially aggressive German intent, military doctrine (i.e., the cult of the offensive), military organization, diplomatic errors, the Russian mobilization, the archduke's assassination, and the outcomes of recent crises.

²⁷ Jervis, "War and Misperception," *Journal of Interdisciplinary History* 18 (Spring 1988), 684.

²⁸ Van Evera (fn. 7).

²⁹ I should note that rationality principles are not the only ones that might be used to limn counterfactual scenarios. One might argue, for example, that had some independent variable been different, a key actor would have ignored it due to cognitive dissonance or wishful thinking.

Even so, the frequent use of rationality principles to sketch counterfactual scenarios should not be surprising. The counterfactual strategy is often used by analysts explaining an outcome

The consequences of the cult of the offensive are illuminated by imagining the politics of 1914 *had* European leaders recognized the actual power of the defense. . . . All European states *would have been* less tempted to mobilize first, and each *could have* tolerated more preparations by adversaries before mobilizing themselves, so the spiral of mobilization and counter-mobilization *would have* operated more slowly, if at all. If armies mobilized, they *might have* rushed to defend their own trenches and fortifications, instead of crossing frontiers, divorcing mobilization from war. Mobilizations *could more easily have been* confined to single frontiers, localizing the crisis. Britain *could more easily have* warned the Germans and restrained the Russians, and all statesmen *could more easily have* recovered and reversed mistakes made in haste or on false information. Thus the logic that led Germany to provoke the 1914 crisis *would have been* undermined, and the chain reaction by which the war spread outward from the Balkans *would have been* very improbable. In all likelihood, the Austro-Serbian conflict *would have been* a minor and soon-forgotten disturbance on the periphery of European politics.³⁰

The use of counterfactuals is explicit and clear in Van Evera's analysis because he is methodologically self-conscious about providing a causal explanation. This is less true of much historical scholarship on the causes of World War I, where the key counterfactual propositions are often left implicit or underdeveloped.

Explicit treatment of counterfactual cases may in turn have the advantage of sharpening substantive debates. In the example at hand Scott Sagan has offered some important qualifications to the arguments advanced by Van Evera and Jack Snyder. He argues, among other things, that Van Evera and Snyder "have overlooked the negative consequences that *would have resulted* if the great powers had adopted purely defensive military doctrines."³¹ He takes issue, in other words, with Van Evera's counterfactual scenario. Sagan holds that the offensive doctrines of the major European powers were rationally chosen to provide extended deterrence to key strategic allies and were not simply or solely the result of the biases of military organizations. He suggests that defensive doctrines might have left states unable to offer credible threats on behalf of their allies, possibly increasing the chances for smaller wars that would have

as the result of human choices. This entails saying why other possible choices were not seen as desirable by the actors. In game-theoretic terms, analysts using the counterfactual strategy are often describing why some particular set of choices was an equilibrium (or, at least, rationalizable) strategy in the "game" faced by the actors. On Nash equilibrium versus rationalizability as game-theoretic solution concepts, see B. Douglas Bernheim, "Rationalizable Strategic Behavior," *Econometrica* 52 (1984), 1007–28.

³⁰ Van Evera (fn. 7), 105 (emphasis added).

³¹ Scott Sagan, "1914 Revisited," *International Security* 2 (Fall 1986), 151–75, at 159 (emphasis added).

altered the balance of power against them (for example, Germany loses Austria to Russia, or Russia loses France to Germany). Sagan's argument contains excellent examples of the use of general principles and specific historical knowledge to support counterfactual scenarios, as well as some clever uses of the actual case strategy to refute counterfactual claims with which he disagrees.³²

Another good example of counterfactual analysis in international relations research focuses on the *non*occurrence of an important phenomenon, namely, that there has not been a war among major powers since 1945. This outcome might be explained by any of the following causes, according to different theories: bipolarity, the presence of nuclear weapons, successful balance of power politics, or the obsolescence of major war due to the "Hollandization" of the great powers.³³ If either nuclear weapons or Hollandization were in fact the true or major cause of postwar military stability, then we cannot hope to employ the actual case strategy to check this, since neither variable varied much before 1945.³⁴ With John Mueller, who has recently argued the case for Hollandization against the more widely accepted nuclear weapons thesis, we would be compelled to argue about what would have happened had nuclear weapons not been invented and amassed in this period.³⁵ As Mueller puts it:

The postwar world *might well have* turned out much the same even in the absence of nuclear weapons. Without them, world war *would have been* discouraged by the memory of World War II, by superpower contentment with the postwar status quo, by the nature of Soviet ideology, and by the fear of escalation [to conventional war].³⁶

Mueller proceeds to argue the counterfactual case for each of these "independent variables" favoring postwar stability. Though he does not deny that nuclear weapons may have had some damping effect on poten-

³² See also Snyder's response to Sagan's critique and Sagan's reply, *International Security* 9 (Winter 1986–87), 187–98. Their discussion is carried out largely in the realm of the counterfactual (e.g., what was the *probability* that the Schlieffen plan would work).

³³ The Hollandization thesis is developed by John Mueller in *Retreat from Doomsday* (New York: Basic Books, 1989), where he argues that gradual changes in the government and societies of advanced industrial states have made them more peaceable in their external affairs. For a review of arguments on the causes of the long peace, see John Lewis Gaddis, *The Long Peace* (Oxford: Oxford University Press, 1987), chap. 8.

³⁴ Depending on how one counts the "poles," neither does bipolarity; see Waltz (fn. 1).

³⁵ To assess the question of *relative* importance, we would also need to ask about what would have happened if nuclear weapons existed but Hollandization did not. Mueller does not explore this second counterfactual scenario explicitly. To hold that Hollandization has been the more important cause, he would need to argue that postwar states lacking the key Hollandization attributes might not have been deterred from fighting a major war, despite nuclear weapons.

³⁶ Mueller, "The Essential Irrelevance of Nuclear Weapons: Stability in the Postwar World," *International Security* 13 (Fall 1988), 55–79, at 56 (emphasis added).

tial escalation, he holds that their causal effect has been redundant, due to the combined impact of the other variables. The claim about the counterfactual case—the postwar world with no nuclear weapons and no major war—is supported by some specific historical detail (for example, characteristics of Soviet ideology) and by at least one general principle: “Wars are not begun out of casual caprice or idle fancy, but because one country or another decides that it can profit from (not simply win) the war—the combination of risk, gain, and cost appears preferable to peace.”³⁷ Taking this as either a theoretically plausible or an empirically confirmed regularity, Mueller suggests that even disregarding the added costs posed by nuclear weapons, the costs of conventional war in these years would have been enough to deter the U.S. and Soviets from a hot war.³⁸

A final example of the counterfactual strategy as used in an $N = 1$ case study comes from work on the breakdown of democratic regimes.³⁹ Alfred Stepan’s explanation for the 1964 military takeover in Brazil illustrates a fairly common way that counterfactuals are employed in comparative politics and international relations case studies.⁴⁰ Stepan proposes that the actual outcome—the military coup—was made possible by the operation of certain social, economic, and ideological “macropolitical” factors but that these did not make the coup “inevitable.” “There remained a small margin of maneuverability within which the process of increasing democratization and participation could have been expanded.”⁴¹ Stepan is here defining the *range of counterfactual variation* that he wishes to explain. Brazil in 1964 could have seen a democratic outcome but did not. The micropolitical factors that reduced the “margin of maneuverability” and selected the authoritarian outcome from the range of possibilities are then assigned causal status above that of the

³⁷ *Ibid.*, 68–69.

³⁸ The fortunate absence of actual cases of nuclear conflict has led a number of historians and political scientists to reflect on the role of counterfactuals in nuclear history. See John Lewis Gaddis, “Nuclear Weapons and International Systemic Stability,” *American Academy of Arts and Sciences Occasional Paper No. 2* (Cambridge: AAAS, 1990). This paper was prepared for an AAAS workshop entitled “Nuclear History and the Use of Counterfactuals.” In a different vein, Richard Ned Lebow and Janice Gross Stein (“Beyond Deterrence,” *Journal of Social Issues* 43 [Winter 1987], 3–71) have briefly discussed the role of counterfactuals in defining a sample of cases of successful deterrence.

³⁹ Juan Linz and Alfred Stepan, eds., *The Breakdown of Democratic Regimes* (Baltimore, Md.: Johns Hopkins University Press, 1978).

⁴⁰ Stepan, “Political Leadership and Regime Breakdown: Brazil,” *ibid.* For other examples, see Richard Smoke, *War: Controlling Escalation* (Cambridge: Harvard University Press, 1977), and citations in fn. 23.

⁴¹ Stepan (fn. 40), 134, and see also 120.

macropolitical factors, which, in Stepan's view, were not "sufficient" themselves to determine the result.⁴²

In his historical treatment of the events leading up to the coup, Stepan identifies political strategy choices by the incumbent president, João Goulart, as the key micropolitical causes of the breakdown of the democratic regime. In an atmosphere of political stalemate, Goulart lost important military and middle-class allies by proposing major economic and constitutional reforms and bidding for the support of the left to back them. But still, "as late as twelve days after [the declaration of these reforms] no 'winning coalition' existed to overthrow Goulart." A naval mutiny by lower-level officers and sailors then occurred, forcing Goulart to choose between alienating either the mutineers or the higher-level officers, who saw the mutiny as a major "threat to the principle of military discipline."⁴³ His decision to be lenient with the mutineers had the unforeseen effect of galvanizing high-level military support for a coup.

These two key political choices are posed as causes of the breakdown of the regime on counterfactual grounds: had Goulart chosen different strategies, the analysis suggests, a coup might not have occurred. The counterfactual contrasting case is justified by reference to specific historical detail (evidence that the military was divided and generally not supportive of direct military rule before the choices were made) and to general principles (for example, the proposition that plotters do not act unless they expect sufficient support, or lack of resistance, from other key actors).

Though Stepan is much more careful and explicit about his counterfactual comparison than is often the case, I would argue that his analysis only goes part of the way to justify his causal claims, essentially because he does not spell out the counterfactual scenario in quite enough detail. Goulart's *reasons* for choosing the left-oriented, constitutional reform strategy, and thus in a sense the deeper causes of the takeover, are left unclear. Stepan seems to suggest that Goulart's destabilizing move leftward was more a function of his personality and aspirations than it was of the untenability of other alternatives. But we need more careful speculation about what would have happened had he instead stayed with his divided and indecisive coalition. If in the longer run his position was simply impossible—that is, if no civilian leader could govern given the political stalemate under existing institutional arrangements—then the

⁴² The distinction is similar to that between underlying causes and specific or proximate causes—a framework often used by historians.

⁴³ Stepan (fn. 40), 129 and 130.

macropolitical factors would seem to gain in importance as causal factors.⁴⁴

N > 1 BUT STILL SMALL-N EXAMPLES

Researchers with more than one actual case are not logically compelled to use the counterfactual strategy to justify a causal claim, as long as they do not have more independent variables than cases (less one), or two or more independent variables that vary together (perfect multicollinearity). Roughly speaking, these conditions ensure that regression estimates can be derived, and they are usually met with ease in large-N research projects. Quite often, however, researchers in comparative politics and international relations work with few cases and many variables.⁴⁵ There are sometimes opportunities in this intermediate range to employ the actual case strategy, but the application of statistical methods either would fail to yield estimates of causal effects or would yield wildly imprecise estimates. In these circumstances, I would argue, one typically finds a mixing of the actual and counterfactual case strategies, with each used to make the other more credible. In good large-N research, the credibility of causal effect estimates derives in the first instance from ample degrees of freedom. Given the theoretical assumptions supporting a causal interpretation, causal claims are empirically supported by regularities of association, in Humean fashion. With an N between 2 and (say) 15, however, the regularity justification is weaker and may need support from more detailed treatments of individual cases. Readers may want to know not only that the proposed causes correctly partition outcomes across the few actual cases, but that in each case the proposed causes indeed *produced* the effects attributed to them. In such efforts one can find examples where researchers resort implicitly or explicitly to the counterfactual strategy and examples where they would have to use it to defend their causal claims.

One common methodological practice in comparative politics and international relations work could be called the “loading up of explanatory factors.” The researcher lists several causes for the phenomenon being explained, all of which were present in the cases where the phenomenon occurred. In formal terms, the researcher has a multicollinearity problem. In such instances, counterfactual arguments would be necessary to

⁴⁴ In a current project, Stepan uses explicit counterfactual analysis to assess the impact of presidential as opposed to parliamentary systems on democratic regime breakdown in South America and Southern Europe.

⁴⁵ Arend Lijphart, “Comparative Politics and the Comparative Method,” *American Political Science Review* 65 (September 1971), 682–93.

support the claim that *any one* of the proposed conditions has a causal effect.

Consider, for example, one of Stepan's arguments in his work on Peru.⁴⁶ His goal is to explain the success or failure of attempts to install corporatist political arrangements in Latin American states. He identifies five independent variables and gives general hypotheses linking each to the likelihood of success or failure. These five variables are then shown to discriminate between actual cases of success and failure in the following sense: where they were all basically favorable to corporatist installation, installation succeeded; where they were basically unfavorable, it did not. The difficulty here is that without counterfactual argument, we cannot decide which of these variables mattered—whether at all or how much. It could be, for example, that only one or two of these variables is really critical and that the rest are totally irrelevant. There are only two ways to decide: (1) find new actual cases where one explanatory factor is present but others are not, or (2) argue counterfactually that the removal of any one of the variables would have damaged chances for corporatist success in the actual cases we have. The tendency to “load up” explanatory factors is quite common. For instance, Barrington Moore's landmark work contains many examples of this practice, such as the list of five “main conditions that have apparently been most important for the development of democracy.”⁴⁷

Counterfactuals may also come into play in what is nominally actual case work when analysts use historical treatments of particular cases to make credible claims based on actual case associations. For example, Theda Skocpol identifies three key variables that differentiate her “positive cases” of social revolution (1789 France, 1917 Russia, and 1911–49 China) from actual cases in which social revolutions did not occur (for example, Meiji Japan, seventeenth-century England, 1807 and 1848 Prussia, Russia after the Crimean War and in 1905, early-eighteenth-century France).⁴⁸ Rather than simply stating the values of the independent variables for the different cases and showing that they differentiate between outcomes, Skocpol undertakes moderately extensive historical treatments of each positive case, detailing how the independent variables she identifies produced social revolution in each one. Though Skocpol makes

⁴⁶ Stepan, *The State and Society: Peru in Comparative Perspective* (Princeton: Princeton University Press, 1978).

⁴⁷ Moore (fn. 1), 430. See also Alexander L. George et al., *The Limits of Coercive Diplomacy* (Boston: Little Brown, 1971), 227.

⁴⁸ Skocpol (fn. 4). Only four of these “negative cases” are treated explicitly and at length, though Skocpol is well aware that others mentioned are used in the same fashion.

frequent use of the actual case strategy within historical treatments,⁴⁹ her approach is broadly similar to that of Stepan in the Brazil example, in that the independent variables are shown to select out certain historical actualities from a range of possibilities.

Gregory Luebbert's exemplary use of the actual case approach in a small-N setting and provides some final examples of how the counterfactual strategy may be employed in such analyses. Luebbert first identifies two independent variables that perfectly partition his fourteen actual cases of European interwar regime types. "Pluralist democracies" occurred only in countries where liberal parties gained dominance before World War I. Regime type elsewhere was determined by which party successfully formed a coalition with the rural "middle peasants": if it was socialists, then "corporatist democracy" resulted; if liberals, then "traditional dictatorship"; if neither, then fascism. Luebbert does not dwell on the causal links between coalition membership and regime type. In justifying the causal argument that the effects of World War I made pluralist democracy along the lines of Britain and France improbable elsewhere, he *does* explore the following counterfactual: "In . . . Italy, Norway, and Sweden, another generation of peace *might have resulted* in pluralist democratic regimes."⁵⁰ But on the whole he is content to let the perfect association and the intuitively acceptable idea that coalition members determine the policy regime support the causal claim.

Instead, Luebbert turns his analytic attention to identifying "the conditions that produced each of these coalitions."⁵¹ By implication, these will be the final or deeper causes of regime type. This effort is marked by both actual case comparisons and implicit counterfactual arguments. For examples of the latter, consider Luebbert's explanation of why socialist parties allied with middle peasants rather than with the agricultural proletariat in Norway and Denmark (thus yielding corporatist democracy). In Norway, he notes, there were few landless laborers, so they were not a tempting group for socialists to mobilize. Rather than correlating size of rural worker populations with socialist mobilization efforts across several actual cases, Luebbert simply appeals to a rationality principle that would support the appropriate counterfactual argument.⁵² In Denmark the socialists could not mobilize what was a much larger ag-

⁴⁹ *Ibid.*, e.g., 63.

⁵⁰ Luebbert (fn. 8), 457–58 (emphasis added).

⁵¹ *Ibid.*, 452.

⁵² The rationality principle is: Parties desirous of electoral success will seek partners that can carry many votes with them. The implicit counterfactual argument is: If there had been many landless laborers in Norway, the socialists might have sought to form a coalition with them, and fascism might have resulted.

ricultural labor force because “this population had already been heavily mobilized by another party.”⁵³ Instead, they mobilized the middle peasants, leading to the corporatist coalition. The implicit counterfactual is: If the agricultural labor force had not already been mobilized, then it might have been mobilized by the socialists, and fascism rather than corporatism would have resulted. Thus, a particular fact about Danish pre-war politics becomes an ultimate cause of corporatism rather than dictatorship or fascism in that country.

COUNTERFACTUALS AND CAUSATION: TWO THEORETICAL ISSUES WITH PRACTICAL IMPLICATIONS

The proposition that a cause of a particular historical event may be established by imagining the effect of its (counterfactual) absence has been made before. In what remains one of the best essays on the topic, Weber argued vigorously for recognition of the link between causal explanation and counterfactuals in historical research.

[The question of] what might have happened if, for example, Bismarck had not decided to make war [in 1866] is by no means an “idle” one [contrary to the view of historian Eduard Meyer]. It does indeed bear on something decisive for the historical moulding of reality, namely, on what causal *significance* is properly attributed to this individual decision in the context of the totality of infinitely numerous “factors” . . .⁵⁴

Since Weber has been a methodological guru for generations of sociologists and political scientists, it is somewhat surprising that this particular essay has been so little discussed and explicitly applied. In recent years the only serious and sustained debate on the role of counterfactuals outside of philosophy took place among historians (and without reference to Weber), in their discussions of the use of counterfactuals by some practitioners of the “new economic history.”⁵⁵ The only political scientist I know of who has examined the topic at length is Elster, particularly in his *Logic and Society*.⁵⁶ He presents a novel “branching worlds” theory

⁵³ Luebbert (fn. 8), 466.

⁵⁴ Weber (fn. 10), 164 (emphasis in original).

⁵⁵ Robert Fogel, *Railroads and American Economic Growth* (Baltimore, Md.: Johns Hopkins University Press, 1964); McClelland (fn. 9); Gould (fn. 9); Fritz Redlich, “‘New’ and Traditional Approaches to Economic History and Their Interdependence,” *Journal of Economic History* 25 (1965), 480–95; and T. A. Climo and P. G. A. Howells, “Possible Worlds in Historical Explanation,” *History and Theory* 15 (1976), 1–20. Fischer (fn. 9) lists further references.

⁵⁶ Elster (fn. 10). See also Elster, *Explaining Technical Change* (Cambridge: Cambridge University Press, 1983), chap. 1; idem, “Reply to Comments,” *Inquiry* 23 (June 1980), 213–32; Steven Lukes, “Elster on Counterfactuals,” *Inquiry* 23 (June 1980), 145–55; Brian Barry,

for assessing the validity of counterfactual propositions and uses it to analyze some examples in economic history. Outside the social sciences, of course, analytic philosophers have been writing about counterfactuals and causation for years.⁵⁷ While much of this literature (for example, that concerned with the metaphysics of modality)⁵⁸ would seem largely irrelevant to social scientists, some recent work on counterfactuals and explanation by philosophers of history has practical value and probably deserves greater attention.⁵⁹

Scholars who have dealt with counterfactuals have often expressed dismay, doubt, and bewilderment at the sorts of logical and philosophical problems such propositions seem to entail. This section briefly introduces two problems that seem particularly bothersome to social scientists and historians.

The first is sometimes referred to as the “Cleopatra’s Nose Problem.”⁶⁰ According to Pascal, if Cleopatra’s nose had been shorter, Antony might not have been so infatuated, and the course of Western history might have been different. Does this imply that the gene controlling the length of Cleopatra’s nose was a cause of World War I? More generally, if we believe that an event A satisfies

(P₂) If A had not occurred, B would not have occurred,

then are we committed to saying that A was a cause of B?

This is not just an idle question. As we have seen, social scientists often argue that A was a cause of B on precisely these grounds—that had A not occurred, B might not have occurred. How do we distinguish be-

“Superfox,” *Political Studies* 28 (1980), 139–43. Political scientists have broached issues raised by counterfactuals in a variety of places. See, for example, Alexander George and Timothy McKeown, “Case Studies and Theories of Organizational Decision Making,” in Robert Coulam and Richard Smith, eds.: *Advances in Information Processing in Organizations* (Greenwich, Conn.: JAI Press, 1985), 2:33–34; Charles Ragin, *The Comparative Method* (Berkeley: University of California Press, 1987), 39; Donald Moon, “The Logic of Political Inquiry: A Synthesis of Opposed Perspectives,” in Nelson Polsby and Fred Greenstein, eds., *Handbook of Political Science* (Reading, Mass.: Addison-Wesley, 1975), 1:131–228.

⁵⁷ Nelson Goodman, “The Problem of Counterfactual Conditionals,” *Journal of Philosophy* 44 (1947), 113–38, reprinted in his *Fact Fiction and Forecast* (Cambridge: Harvard University Press, 1983); Lewis (fn. 14); Ernest Sosa, ed., *Causation and Conditionals* (Oxford: Oxford University Press, 1975). Part of the philosophical interest in counterfactuals arises from their bearing on key issues in the philosophy of science. See Frederick Suppes, “The Search for Philosophic Understanding of Scientific Theories,” in Suppes, ed., *The Structure of Scientific Theories* (Urbana: University of Illinois Press, 1977), 3–232, at 36–45, and references cited there; Ernest Nagel, *The Structure of Science* (New York: Harcourt, Brace, and World, 1961). Nagel (chap. 15) also saw that counterfactuals play a key role in historical explanation.

⁵⁸ Loux (fn. 25).

⁵⁹ Martin (fn. 20). See also Martin, “Beyond Positivism: A Research Program for Philosophy of History,” *Philosophy of Science* 48 (1981), 112–21; and idem, “Singular Causal Explanation,” *Theory and Decision* 2 (1972), 221–37.

⁶⁰ Edward Hallet Carr, *What Is History?* (New York: Knopf, 1962); Gaddis (fn. 38).

tween the infinity of particular factors that would have precluded or reduced the likelihood of some interesting event had they *not* taken place?

One approach would be to hold that causality should not be defined in terms of counterfactuals like P₂, that “A satisfies P₂” does not imply that A is a cause of B. Consider the view that a cause is something that *produces* its effect whenever (or usually when) it occurs. The cult of the offensive can be understood to have produced World War I in this sense, but Cleopatra’s nose really cannot. This strategy amounts to accepting a regularity theory of causation.⁶¹ Accidental happenings that help lead to specific events are not “causes” but only “conditions”; conditions of particular events that generalize or could regularly produce the effect are labeled causes. The distinction between causes and conditions could conceivably be a useful one for political scientists engaged in small-N work, and particularly for case studies.⁶² The distinction can do violence to common sense and ordinary usage, however, as in the following: the unlucky person’s death was not “caused” by the falling shingle, it was caused by skull fracture; the shingle was only a “condition.”

Another approach would be more lenient with certain accidental happenings. We could argue that the length of Cleopatra’s nose did not make World War I any more likely than myriad other possible worlds that could have followed, whereas the presence of the cult of the offensive did significantly “select out” the particular outcome that was World War I. That is, the same theoretical argument holding that the probability of World War I conditional on Cleopatra’s nose being shorter was zero implies as well that the probability of World War I conditional on her nose being as it was must have been *almost* zero. On this account, an accidental (or “random”) happening—say, a monkey bite leads to the death of a king, whose replacement begins a war⁶³—could qualify as a “cause” of a particular event. The important point is that in both accounts events that satisfy P₂ are not necessarily causes of the phenomenon being explained. Though counterfactuals like P₂ might be explored to lend credence to a causal claim, a cause does more than just satisfy P₂.⁶⁴ Both suggestions could have practical value for political scientists arguing causality on counterfactual grounds.

⁶¹ On these, see Tom Beauchamp and Alexander Rosenberg, *Hume and the Problem of Causation* (Oxford: Oxford University Press, 1981).

⁶² For example, Luebbert (fn. 8) might have distinguished more carefully between the *conditions* prevailing in particular countries that allowed the *causes* of regime type—coalition membership—to operate as they did. On related philosophical distinctions between causes and conditions, see J. L. Mackie, “Causes and Conditions,” in Sosa (fn. 57), 15–38; and Martin (fn. 59, 1981, 1972).

⁶³ Carr (fn. 60), citing Churchill.

⁶⁴ A third suggestion for resolving this problem would be to add a condition of temporal

The second vexing problem for scholars concerns what Elster calls the “legitimacy” of a counterfactual proposition.⁶⁵ Elster argues that a counterfactual thought experiment undertaken to assess a causal hypothesis is not legitimate if we have a theory saying that the counterfactual antecedent could not have happened. Suppose we wish to learn the effect of the railroad on the growth of GNP in nineteenth-century America and that we attempt to do so by imagining the last century without the railroad.⁶⁶ Elster thinks it nonsensical to speculate whether the internal combustion engine would have been invented earlier than it was (in the counterfactual nineteenth-century America without railroads), since an answer would require a theory of technical change strong enough to make the original counterfactual proposition implausible. If we could predict whether the gas engine would have been invented earlier, surely we would also have a theory showing that the railroads “had to be” invented when they were. Elster calls this “the scissors problem,”⁶⁷ or “the unimportance of inevitable”:⁶⁸ the better our theories, the more things we know “had to occur” as they did, and thus the fewer counterfactuals we can legitimately assert.

In social science practice this problem often appears in the following guise. On the basis of actual case comparisons, a comparativist claims that C caused E in country X, suggesting that if C had been different, the outcome in country X might have been more like the outcome in country Y. A specialist on country X criticizes this as absurd, arguing that due to a complex of historical and cultural factors particular to country X, C could not have been different.

The insistence that counterfactual propositions be “legitimate” may confound, or at least obscure, two distinct problems.⁶⁹ The real issue is not legitimacy, if this means that in our counterfactuals we cannot legitimately vary causes that had to occur as they did. Whether event C had to occur has no direct bearing on its causal status with respect to E. A variable may help explain one outcome and still itself be explained by the action of other variables. In large-N work this pattern is commonly found in structural equations models, in which a dependent variable in one equation may be an independent variable in another equation.

or causal proximity to P₂; that is, A is a cause of B if P₂ is true and A precedes B by a relatively short time period, or if the causal chain is not too long. But this raises the problem of how long?

⁶⁵ Elster (fn. 10).

⁶⁶ Fogel (fn. 55).

⁶⁷ Elster (fn. 56, 1983), 38.

⁶⁸ Elster (fn. 10), 185.

⁶⁹ For related criticisms of Elster’s notion of counterfactual legitimacy, see Barry (fn. 56); and Lukes (fn. 56).

The key issue, it seems to me, is what philosopher Nelson Goodman called “cotenable.” In Goodman’s account, a counterfactual assertion is judged true if (1) the counterfactual antecedent, when joined with appropriate theories and facts, implies the consequent; and (2) the counterfactual antecedent is “cotenable” with the facts or “initial conditions” used to draw the inference, meaning that if the antecedent had actually occurred, the initial conditions could also have occurred.⁷⁰ Thus, in his critique of the cult-of-the-offensive hypothesis, Sagan in effect argues that supposing the absence of the cult is not cotenable with supposing a 1914 otherwise identical to the actual 1914. As noted, he suggests that beliefs in defensive superiority would have created a different strategic problem for state leaders, one that could also have produced war.

The question, then, is not whether a factor had to occur but whether varying the factor implies changing other factors that *also* would have materially affected the outcome.⁷¹ It is not appropriate to criticize a counterfactual argument by saying that the antecedent could not have occurred. Rather, we need an explicit argument saying that if the antecedent had been the case, other changes would be required in the counterfactual scenario that would have affected the outcome in a different way.

An obvious methodological prescription follows: analysts using the strategy of counterfactual argument should pay close attention to whether their counterfactual suppositions are cotenable with the facts and theories used to draw the causal inferences they make. This is perhaps a more precise statement of what it means to make a counterfactual argument plausible. I expect that in practice, the cotenable requirement will be more plausibly satisfied for small causes, such as specific policy decisions, than for big causes, such as nationalism, imperialism, or a cult of the offensive. History often provides evidence that leaders considered several possible choices at certain junctures, and in some instances it may be feasible to imagine a different choice without changing other major influences on the outcome in question. The fewer the

⁷⁰ Goodman (fn. 57, 1983), 15–17. See also references in fn. 14. Goodman points out that it is quite problematic to use a counterfactual to define general truth conditions for counterfactuals. See Mackie (fn. 14) for a possible way around this problem (which at any rate may be of greater interest to philosophers than to political scientists).

I should note that Elster (fn. 10) is well aware of the issue of cotenable, which he refers to as “compossibility” (p. 177) and also “compatibility” (p. 183). Indeed, his “branching worlds” theory for assessing the truth of counterfactuals can be seen as a suggestion for assessing cotenable.

⁷¹ Note the similarity of the cotenable condition to P1, the key assumption justifying a causal interpretation of regression coefficients derived from quasi-experimental data. The likeness underscores the point that quasi experiments and the counterfactual strategy share reliance on counterfactual suppositions.

changes from the actual world required by a counterfactual supposition, the easier it will be to draw and support causal inferences, and the more defensible they will be.⁷²

CONCLUSION

Counterfactuals and the counterfactual strategy of hypothesis testing play an important but often unacknowledged and underdeveloped role in the efforts of political scientists to assess causal hypotheses. I have tried to show that any *nonexperimental* research that makes causal claims, be it of the large-N or small-N variety, must confront counterfactuals in the form of key assumptions or in the use of hypothetical comparison cases. Particularly in small-N research, the common condition of too many variables and too few cases makes counterfactual thought experiments a necessary means for serious justification of causal claims. I close with two simple suggestions for analysts evaluating causal claims via counterfactual argument rather than via regularities of association in a sample of actual cases.

First, small-N analysts could strengthen (or simply specify) their causal arguments by being *explicit* about the counterfactual scenarios needed to support their hypotheses. Quite commonly, researchers in comparative politics and international relations assert that their dependent variable is X, where X is some *particular* event or phenomenon. X might be the failure of the U.S. to play the role of international hegemon between the world wars, a change in the nuclear proliferation regime, the dominance of the Liberal Democratic Party in Japan, or the collapse of communism in Eastern Europe. Analysts explaining such events need to understand that *none* of these are variables. They become *values* of variables if alternative, counterfactual scenarios are identified or if actual cases, some of which differ in outcome, are added to the analysis.⁷³ If, for whatever reason, one is reluctant to add actual cases, then it is essential to make explicit what might have happened if a possible cause had varied. Counterfactual comparison cases need not be exhaustively detailed—just specified—so the reader knows what variation the theory or hypothesis proposes to explain.

The second key step is making inferences drawn from a counterfactual comparison *defensible*. Sometimes the argument implied by a coun-

⁷² This suggestion is influenced by examples provided by McGeorge Bundy (fn. 7), and by Gaddis's discussion of them (fn. 38).

⁷³ The point that a variable is distinct from any particular realization of it should be obvious but is sometimes missed. The point that the variance explained might be defined across actual *or* counterfactual cases is rarely seen.

terfactual proposition is quite obvious and requires little or no unpacking.⁷⁴ Other times, particularly when the hypothesis is evaluated against other hypotheses, analysts should make clear what arguments support it and how they do so. The analyst needs to ask whether the causal inference does indeed follow from the theories and historical facts used to sketch the comparison case and then, whether the counterfactual proposition is cotenable with the counterfactual scenario. Cotenable requires that if the counterfactual assertion had been true (for example, if there had been no cult of the offensive), nothing else would also have been different in a way that would have materially affected the outcome.

Of course, we can never be certain about what else would have been different if C had been different. But perhaps we can venture arguments that can be judged more credible or less credible, depending on our use of historical detail and theories about the way people behave. I should emphasize that I am not suggesting that there can be a special methodology for determining precisely what would have happened. Nor am I advocating a new methodology to rival established approaches to hypothesis testing. Rather, the intent has been, first, to show that counterfactuals cannot be avoided in nonexperimental hypothesis testing, then, to explicate their roles, and finally, to recommend that when political scientists use counterfactuals, they do so explicitly and carefully.

⁷⁴ For example, "if that match had been struck, it probably would have lit" will not be controversial in most circumstances. Neither are the counterfactual arguments implied by Luebbert (fn. 8) on why the fascist coalition did not develop in Norway and Denmark.