COUNTERFACTUALS AND HYPOTHESIS TESTING IN POLITICAL SCIENCE

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"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 (1966: 414)

"Nuclear weapons did not cause of 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 (1979: 180)

The epigraphs provide examples of counterfactual conditionals, 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)." This paper argues that such propositions play a necessary and fundamental, if often implicit and underdeveloped role in the efforts of political and other social scientists to assess their hypotheses about the causes of the phenomena they study.

The argument is developed and illustrated in three sections. First, I distinguish between the "actual case" and "counterfactual" strategies of hypothesis testing. In the former, actual cases define the range of variation to be explained, and causal claims are made credible by regularities of association in a sample. In the latter, the range of variation to be explained is defined by counterfactual cases that "could have occurred" if independent variables had assumed different values, and causal claims are made credible by the theories and historical facts used to sketch counterfactual scenarios.

In both strategies counterfactuals can be seen to play a role in the logic supporting causal inference. Where analysts have "few cases and many variables" -- that is, in "small-N" work -- exploration of counterfactual cases is often logically necessary to establish the causal importance (absolute or relative) of any one variable. In "large-N" analyses, on the other hand, counterfactual assumptions enter into the logic supporting causal inference via the "ceteris paribus" assumption (i.e., independent variables

uncorrelated with error terms). The section briefly explores key differences between the role of counterfactuals in the actual case and counterfactual strategies.

The paper's primary concern is with the use of counterfactuals in case study, "comparative method," and other "small-N" approaches. The second section shows how the counterfactual strategy appears in practice by considering some 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 (e.g., the occurrence of World War I, or the Brazilian military takeover in 1964), and when the researcher has several actual cases (e.g., "social revolutions," interwar European regime types).

The third section returns to some theoretical issues concerning the link between causal arguments and counterfactual propositions. I briefly discuss two logical problems with counterfactuals that bear on explanatory practice in the field. First, is any event C that appears to satisfy "if C had not occurred, E would not have occurred," to be called a "cause" of E? Second, are some counterfactual comparisons more "legitimate" or appropriate than others?

COUNTERFACTUALS AND THE LOGIC OF INFERENCE

Suppose it is hypothesized that C was a cause of an event E. I would argue that when experimental control and replication are not possible, analysts have a choice between two and only two strategies for "empirically" assessing this hypothesis. They can either (1) imagine that C had been absent and ask whether E would have (or might have) occurred <u>in that counterfactual case</u>; or (2) search for other <u>actual</u> cases that resemble the case in question in significant respects,[1] except that in some of these cases C is absent (or

[1] The sense of "significant respects" is discussed below.

had a different value); the analyst then checks to see how tight is the relationship between the occurrence of C and E in the set of actual cases.[2] Both strategies, if successful (from the analyst's point of view), tend to support the hypothesis that the proposed cause in fact produces (or produced) the effect.

As an illustration, consider the hypothesis that "structural" rather than domestic political factors have been the principal causes of major aspects of Soviet foreign policy. Following the first, or "counterfactual strategy," this hypothesis would be evaluated by examining arguments that <u>any</u> regime in Russia, Soviet or not, <u>would have</u> chosen essentially the same foreign policies. Following the second, or "actual case" strategy, the analyst would search for cases of states in both similar and dissimilar "structural positions" as Soviet Russia, and then check for a relationship between structural position and foreign policies in this sample.[3]

It is important to see that both methodological strategies aim to solve <u>the same statistical problem</u>. Our analyst begins with one case and at least one explanatory variable, which means negative degrees of freedom.[4] 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 is driven to add or create more cases.

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[2] 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 delimited in (1). The potential difficulties with (2), which is formally known as regression analysis though informally practiced in such works as Skocpol (1979), are extensively discussed in the econometrics and statistics literatures.

[3] Following Waltz (1979, 1981), "structural position" here would entail the number of great powers and the basic geopolitical circumstances of the Soviet Union. On structural versus domestic political or ideological explanations of Soviet foreign policy, see also Posen (1987).

[4] "Degrees of freedom" are the number of cases minus the number of independent variables minus one.

In the first strategy, the degrees of freedom problem is solved by making an argument about a counterfactual case (or cases) that never actually existed. In the second, the problem is solved by adding actual cases.

There is another way to make this point that some may find clearer. Whenever we explain why some particular event E occurred, we 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. In much historical analysis, the other things that "might have been" had the historian's favored causes varied are left implicit. In more methodologically self-conscious "small-N" work, analysts tend to be more explicit about "what might have happened" (e.g., Moore 1978, Van Evera 1984). 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 (Huth 1988); comparative politics experts may explain why inter-war Germany became a fascist dictatorship rather than a liberal democracy like England (Moore 1966), or taking a larger range of other actual cases, a social democracy like Sweden or a traditional dictatorship like Austria in the Dolfuss-Schuschnigg period (Luebbert 1987).

Both the "counterfactual" and "actual case strategies" run important methodological risks. In both 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 certainty at all? When confronted with the suggestion that the validity of their causal inferences necessarily depends on counterfactual argument, historians have often simply dismissed out

of hand or ignored the idea in favor of the view that their job is to deal with reality.[5] With the exception of an oddly neglected methodological piece by Max Weber (1949) and some recent work by Jon Elster (1978, 1983), political scientists and sociologists have also tended to avoid explicit discussion or open embrace of the "counterfactual strategy" of confirmation, probably because it is felt that an empirical political science must deal only with real 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 (Polsby 1982). The play on "science fiction" is no accident here.

The risks of the second methodological strategy -- increasing degrees of freedom by considering other actual cases -- are also well known. In the counterfactual approach, one tries to imagine another (not actual) case where the presumed causal agent is absent, but everything else that is relevant is identical. In the second strategy, by contrast, the analyst adding actual cases may not know if the additional cases are appropriately "identical." If there are "other causes" of the phenomenom in question that are not considered explicitly in the analysis, and if any of these are systematically related to the causes explicitly considered, then effects of the "other causes" will be wrongly included with those of the causes we are trying to evaluate. Simply put, estimates of the effects of the proposed causes will be biased. To those with some statistical training, this problem is the familiar one of whether any independent variables are correlated with the contents of the error term, which 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

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[5] For example: "The might-have-beens of history are not a profitable subject of discussion." M.M. Postan, quoted in Gould (1969: 195). See also examples in McClelland (1975: Chap. 4).

question of whether a researcher's several cases are "comparable," or if the "ceteris paribus assumption" is adequately satisfied.[6] On occasion, the severity of this risk leads some analysts to be skeptical of "large-N" or "comparative historical" work, as against case studies where the risks of (an often implicit) counterfactual strategy may seem intuitively less serious.

What perhaps is less well understood is the link between this central risk run by the "actual case" strategy and counterfactuals. While this 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 non-experimental data needs to make a number of theory-driven assumptions.[7] As noted, chief among these is the assumption that explanatory variables and the "disturbances" or "other causes" are

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[6] The notion of "comparability" plays a major role in comparative politics specialists' methodological and applied writings. My impression is that nonetheless the notion as used 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 be as literally "equal" as possible ($E(e^2)$ should be close to zero); that measures will not be as valid or reliable across countries and cultures; and other meanings.

Posing the main risk for correlational analysis across sets of actual cases in terms of the 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 unbiasedness we only need require that the "other things" are not systematically related to the prospective causes we are evaluating. This point was not clearly understood by Mill (working before statistics was well developed), who sometimes writes in his System of Logic (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 comparative politics specialists who take Mill as a principal methodological guide (e.g., Skocpol and Somers (1980)). That said, I should also note that "large-Nists" often do refer to this assumption as "the ceteris paribus" assumption simply for convenience, and I will follow this usage here.

[7] This is true of actual experiments as well. See Neuberg (1988), who shows (among other things) that a counterfactual assumption is needed to justify estimates of sampling variance in actual experiments.

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

> (P1) 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 P1 is false, then $E(x'e)$ does not equal zero. If $E(x'e)$ does not equal zero, P1 cannot be true. (QED.)

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. One may not think about the "ceteris paribus" assumption in terms of a counterfactual proposition, but nonetheless a counterfactual proposition is necessarily involved. In actual experiments random assignment guarantees the truth of P1. In quasi-experiments, a causal interpretation of estimated coefficients requires belief in the credibility of the counterfactual P1 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 a particular child had been exposed to the Head Start program, she would have scored roughly "so much" more on a high school achievement test.

If both strategies of confirmation depend in some measure on counterfactuals, and both are means of solving the same statistical problem, then exactly how do they differ? What separates them is the way each strategy provides "empirical" confirmation for a causal hypothesis. 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.[8] In the "counterfactual case" strategy, on the other hand, frequencies of association cannot be meaningfully assessed. (They are arguably irrelevant in any event, since the researcher is attempting to perform the "perfect experiment," where everything is equal but the test factor.) 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) drawing on knowledge of historical facts relevant to a counterfactual scenario.

An example will help make this point concrete. It has been proposed that a "cult of the offensive" in turn-of-the-century Europe -- the widespread conviction in civilian and military circles that there were enormous strategic advantages to striking first -- was an important cause of World War I (Van Evera 1984, Snyder 1984). According to the analysis above, we have 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 whereas others did not. We could then construct some measure for military and civilian beliefs about first strike advantages, presumably from military writings and statements of politicians' and generals' 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 par-

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[8] Of course, theory-driven assumptions are needed to support or justify any regression result -- the point is that the result one looks for in regression analysis is a coefficient significantly different from the null hypothesis, and this difference derives from a frequency of association in the sample.

ticular, we would check the value of the several independent variables for this case, comparing their various contributions to that of "belief in first strike advantages."[9]

Alternatively, we might employ the "counterfactual case" strategy, which often goes under the name "case study."[10] Here careful researchers would make an explicit effort to imagine the pre-war world without a "cult of the offensive," but otherwise similar. They would then construct an argument showing that a general war would have been much less likely to have occurred in the counterfactual case. For its credibility, such an argument would depend on the principles used to draw the picture of what "would have happened." Stephen Van Evera (1984), wh adopts precisely this strategy to support his "cult of the offensive" hypothesis, relies at bottom on general principles of rationality. He asks, in essence, what crisis behavior by statesmen would have been rational if they had believed that defense rather than offense had the advantage. A reconstruction of what rational actions would have been given these beliefs yields the conclusion that in a crisis like that of July 1914 (the counterfactual case), escalation would have been much less likely.

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 <u>invoking others</u> -- laws, regularities or principles which are taken as having some independent credibility. In the actual case strategy, no other principles need to be

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[9] Of course, each step of this process -- from identifying a sample to interpreting relative "importance" -- is fraught with methodological peril. <u>Both</u> strategies, it should be emphasized, are risky.

[10] 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 are negative. In practice, users of case studies often resort to undeveloped comparisons to other actual cases (e.g., "Whereas in many other African countries ..., in Kenya ..."), and testing multiple implications of a theory (Campbell 1975), as well as implicit or explicit counterfactual reasoning.

invoked directly to support the causal hypothesis -- all we care about is a strength of association across actual cases. Indeed, from this vantage point, the counterfactual strategy for "empirically" checking a casual 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 seems slightly less sharp. As noted, when the actual case strategy is employed in a non-experimental setting, the validity of a causal interpretation of the results is contingent on the truth of a counterfactual assumption about the "disturbances" or error terms. We must be ready to accept the proposition that had variable X taken a different value, the dependent variable would have differed by a fixed multiple (the estimated "effect") plus a random effect of "other causes." Our confidence that the other causes would not vary with independent variable depends on our confidence in our theory about what the other causes are, and how they might be related to the variables being tested explicitly.

Two other contrasts between the counterfactual and actual case strategies should be noted, before we turn to some more general implications of the argument. 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 the effect estimates of different independent variables.[11] Ultimately, we can do this because we have a sample from which relevant frequencies and magnitudes can be extracted. In the counterfactual strategy on the other hand, we have no concrete frequencies or magnitudes, and the degrees of freedom problem will bite every time we introduce a new vari-

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[11] There are, however, more than one meaningful senses to the idea of "causal importance" in a regression model. See Shanks (1982), Achen (1982).

able 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 a "more important" cause than B. The above analysis would suggest that we now need not one, but at least two counterfactual case 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.[12]

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, on the other hand, there is no 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 debate on the origins of World War I, arguments about the relative importance of possible causes become arguments about the plausibility of different counterfactual scenarios.

To close this section, I wish to point out two implications of the above analysis which bear on some current methodological issues in the field.

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[12] Some philosophers of history working on the problem of how historians can, should, and do attribute causal weightings have proposed similar criteria. See Martin (1982) and references therein.

First, it is often argued by scholars in comparative politics and international relations that because statistical methods are "inapplicable" when we have few cases and many variables, other methods need to be developed to enable sound explanations (e.g. "the comparative method," "structured, focused comparisons," "process-tracing", etc.). Following the analysis here, we would emphasize that statistical methods are "inapplicable" in these circumstances for a good reason (not enough cases to support a causal claim), and that this reason determines its manner of solution (adding counterfactual or actual cases). Statistical logic does not simply cease to operate when the "N" dips below 15 or 10 or 5, creating room for alternative ways of testing causal hypotheses.

Second, researchers' choices between the two strategies of confirmation should depend on the types of risks they are willing to run. In some instances, the counterfactual claims needed to support a causal inference seem entirely unproblematic. To support the claim that a gunshot through the heart caused the death, we do not require a formal survey and regression analysis. Less trivially, a researcher might be untroubled by regression analysis showing no clear relation between domestic political trouble and war initiation, if it seemed clear from counterfactual reasoning that in case X, domestic problems were a factor impelling the leadership to start a war (cf. Levy 1987). 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 methodologi-

cally to assess theories and hypotheses.[13] There is a substantial amount of work in political science where the analyst declares an interest in explaining phenomenom X (e.g. war, revolution, democracy), chooses a set of cases where X actually occurred, and ends up drawing conclusions about "the causes" of phenomenom X. From the "large-Nist's" standpoint, this procedure appears totally invalid. Such analysts "sample on their dependent variables"; if they fail to include cases where X does not occur, how can they find causes which differentiate between outcomes? But if we see that each case study will propose "causes" that selected the actual outcome from a range of possible counterfactual outcomes in that case, we see the source of the "not X's" 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.

COUNTERFACTUALS IN PRACTICE

The most controversial point argued above is probably that concerning the necessity of counterfactual argument for justifying causal claims in small-N settings. I am not arguing that exploration of counterfactual scenarios is simply "another option" that small-N practitioners might consider when going about their business. I am arguing that a causal claim made when degrees of freedom in the "actual world" are negative 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 will not find counterfactuals playing central roles in all small-N political science research. My impression, after reviewing literature for

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[13] Another tack on this puzzle is taken by Campbell (1975).

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 phenomenom (e.g., Cohen 1987, Im 1987, Gowa 1984). 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 (Deyo 1987). In addition, even in N=1 case studies analysts will often mix both strategies of confirmation.

I will first discuss the use of counterfactuals in three case study, "N=1" examples.[14] 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 several comparative politics, "N > 1" examples. Here analysts often make primary use of the "actual case strategy" when grounding causal assertions, but counterfactuals may in fact be necessary to fully justify inferences.

"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 candidate factors,[15] which are typically argued to be "causes" on the follow-

[14] 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 Loux (1985).)

[15] 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 (the "cult of the offensive"), military organization, diplomatic errors, the Russian mobilization, the Archduke's assassination, and the outcomes of recently past crises.

ing 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 (or it would have occurred in a radically different form, e.g., it would have been limited to Eastern Europe, etc.). For example, arguing the causal importance of "misperceptions" in 1914, Robert Jervis (1988: 684) 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." Note that Jervis is relying on a rationality principle (sensitivity to war costs) to make credible the causal inference drawn from the counterfactual proposition.

On similar grounds, Stephen Van Evera (1984) has convincingly developed the thesis that a "cult of the offensive" was a major cause of World War I -- in fact, he argues that military and civilian tendencies to glorify the offensive had the effect of "feeding or magnifying a wide range of secondary dangers" which 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.[16]

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[16] 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 blocked it out due to cognitive dissonance or "wishful thinking." Richard Ned Lebow (1981) seems to adopt this strategy in arguing that the failure of the British to give a clear indication to the Germans of their willingness to defend France did not matter, since the Germans were irrationally committed to believing England would not intervene anyway.

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

> The consequences of the cult of the offensive are illuminated by imagining the politics of 1914 had European leaders recogized 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 (105, emphasis added).

The use of counterfactuals is so 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 or "origins" of World War I, where the key counterfactual propositions are often left implicit or underdeveloped. It should be noted that explicit treatment of counterfactual cases may have the advantage of sharpening substantive debates. In the example at hand, Scott Sagan (1986) has recently offered some important qualifications to the arguments of Van Evera and Jack Snyder (1984, another developer of the "cult of the offensive" hypothesis). Among other things, Sagan argues that Van Evera and Snyder "have overlooked the negative consequences that would have resulted if the great powers had adopted purely defensive military doctrines" (159, emphasis added). He takes issue, in other words, with Van Evera's counterfactual scenario. Sagan holds that the offensive doctrines of the major European powers were rational -- chosen to

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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 Pearce (1984).)

provide extended deterrence to key strategic allies -- and 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 altered the balance of power against them (e.g., Germany loses Austria to Russia, or Russia lose France to Germany). I cannot go into the methodological details of Sagan's argument -- suffice it to say that excellent examples of the use of general principles and specific historical knowledge to support counterfactual scenarios, along with some very clever uses of the actual case strategy to support counterfactual claims can be found in his article.[17]

Some other good examples of the counterfactual strategy at work in international relations concern the non-occurrence of an important phenomenom. There has been no major power war since 1945, a particular event which 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" (the "Hollandization" of the great powers). If either nuclear weapons or "Hollandization" were in fact the true or major cause of post-War military stability, then we cannot hope to employ the "actual case strategy" to check this, since neither "variable" varied much before 1945.[18] With John Mueller, who has recently argued "Hollandization" against the more widely accepted nuclear weapons thesis, we would be compelled to argue about "what would have happened" if nuclear weapons had

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17 Snyder (1986) responds to Sagan's critique, and Sagan has a reply. Their discussion is carried out largely in the realm of the counterfactual (e.g., what was the probability that the Schlieffen plan would work).

18 Depending on how one counts the "poles," neither does bipolarity (Waltz (1979)).

not been invented and amassed in this period.[19] 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] (1988: 56, emphasis added).

Mueller proceeds to argue the counterfactual case for each of these "independent variables" favoring post-war stability. He does not deny that nuclear weapons may have had some damping effect on potential escalation, but holds that their causal effect has been essentially redundant, due to the other variables' joint impact. The claim about the counterfactual case -- the post-war world with no nuclear weapons and no major war -- is supported by some specific historical detail (e.g., characteristics of Soviet ideology), and at least one general principle. Namely, "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" (68-69). Taking this as either a theoretically plausible or 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."[20]

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[19] And, strictly speaking, about what would have happened if nuclear weapons existed but Hollandization did not. Mueller does not explore this second counterfactual scenario explicitly, even though it could conceivably strengthen his case. 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. In doing so, he would be obliged to confront the arguments of deterrence theory more directly.

[20] The fortunate absence of actual cases of nuclear conflict has led a number of historians and political scientists to reflect briefly on the role of counterfactuals in "nuclear history" (e.g., Gaddis (1989), Lebow and Stein (1987)).

Another instructive example of the counterfactual strategy as used in an "N=1" case study comes from work on the "breakdown of democratic regimes" (Linz and Stepan 1978). Alfred Stepan's (1978) explanation for the 1964 military takeover in Brazil illustrates a fairly common way that counterfactuals are employed in comparative politics case studies.[21] 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" (134; see also 120). 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 which reduced the "margin of maneuverability" and selected the authoritarian outcome from the range of possibilities will be attributed causal status above that of the "macropolitical factors," which, in Stepan's view, were not "sufficient" themselves to determine the result.[22]

Through a historical treatment of the events leading up to the coup, Stepan identifies political strategy choices by the incumbent president Joao Goulart as the key "micropolitical" causes of the democratic regime's breakdown. In an atmosphere of political stalemate, Goulart lost important military and middle class allies by proposing major economic and constitutional reforms and bidding for left support to back them. But still, "as late as twelve days after [the declaration of these reforms] no 'winning coalition' existed to overthrow Goulart" (129). A naval mutiny by lower lever officers and sailors then occurred, forcing Goulart to choose between alienating either

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21 For other examples, see Linz and Stepan (1978), Cohen (1987), Ig (1987), Smoke (1977), Gowa (1984).

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

the mutineers or the higher level officers, who saw the mutiny as a major "threat to the principle of military discipline" (130). He chose to be lenient with the mutineers, which had the unforeseen effect of galvanizing high level military support for a coup.

These two key political choices are posed as "causes" of the regime breakdown on counterfactual grounds -- the analysis suggests that if Goulart had chosen different strategies, a coup might not have occurred and the democratic regime would not have broken down. 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 general principles -- for example, the proposition that the coup plotters would not act unless they could be assured of sufficient support (or lack of resistance) from society and other sections of the military.

I would argue in addition that Stepan's analysis only goes part of the way in justifiying his causal claims, essentially because he does not spell out the counterfactual scenario in quite enough detail. Goulart's <u>reasons</u> for choosing the 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 leftwards was due to his personality and aspirations more than the untenability of other alternatives. But we need more careful speculation about what would have happened if he had stayed with the divided and indecisive coalition he had. If in the longer run his position was simply impossible -- no civilian leader could govern given the political stalemate under existing institutional arrangements -- then the "macropolitical factors" would seem to gain in "causal importance."

"N > 1" (but still "small-N") examples

Researchers with more than one actual case are not logically compelled to use the "counterfactual strategy" if they wish 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 multi-collinearity). Roughly speaking, these conditions ensure that regression estimates can be derived, and 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," an intermediate range where there are sometimes opportunities to employ the actual case strategy, but where application of statistical methods would either fail to yield effect estimates, or yield wildly imprecise estimates. In these circumstances, I would argue, one tends to find constant mixing of the actual and counterfactual case strategies -- each is 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 -- causal claims are empirically supported by regularities of association, in Humean fashion. With an "N" between 2 and (say) 15, on the other hand, 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 will typically find examples where researchers resort implicitly or explicitly to the counterfactual strategy, and examples where they would have to use the counterfactual strategy to properly justify a causal claim.

One common methodological practice in comparative politics and international relations work could be called the "loading up of explanatory factors."

The researcher will list more "causes" or "conditions" for the occurrence of phenomenom being explained than there are actual cases to allow legitimate imputations. In such instances, counterfactual arguments would be necessary to properly justify the claim that any of the proposed conditions has a causal effect. In formal terms, the researcher has a multicollinearity problem.

Consider, for example, one of Alfred Stepan's arguments in The State and Society: Peru in Comparative Perspective (1978). Stepan's principal goal is to explain the success or failure of attempts to install corporatist political arrangements in Latin America states. He identifies five independent variables and gives general hypotheses linking each to the likelihood of success or failure. These five variables are then found 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 just one or two of these variables is really critical, and the rest 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, say, 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 Social Origins of Dictatorship and Democracy, a paradigm for uses of the actual case strategy in a small-N setting, 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" (1966: 430)).

Counterfactuals may also come into play in what is nominally "actual case" work when analysts use historical treatments of particular cases to make credible the claims based on actual case associations. In States and Social

Revolutions, Theda Skocpol (1979) identifies three key variables which differentiate her "positive cases" of "social revolution" (1789 France, 1917 Russia, and 1911-1949 China) from cases where social revolutions did not occur (e.g., Meiji Japan, 17th century England, 1807 and 1848 Prussia, Russia after the Crimean war and in 1905, early 18th century France, etc.).[23] 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 case. Though Skocpol makes frequent use of the actual case strategy within historical treatments (e.g., p. 63), her approach is broadly similar to that of Stepan in the Brazil example -- the operation of the independent variables is shown to select out certain historical actualities from a range of (often unspecified) possibilities.

An exemplar of the "actual case" approach in a small-N setting, Gregory Luebbert's (1987) "Social Foundations of Political Order in Interwar Europe" provides some final examples of how the counterfactual strategy may be employed in an "actual case" analysis. Luebbert first identifies three independent variables that perfectly partition his 14 actual cases of European interwar regime types. "Pluralist democracies" occurred only in countries where liberal parties gained dominance before World War I. Elsewhere regime type 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 justifying the causal links between coalition membership and regime type. In justifying the causal argument that the

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[23] 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.

effects of World War I made pluralist democracy along the lines of Britain and France infeasible elsewhere, he does elaborate the following counterfactual: "In ... Italy, Norway, and Sweden, another generation of peace might have resulted in pluralist democratic regimes" (457-8, emphasis added). But on the whole he is content to let the perfect association and the intuitively acceptable idea that coalition members determine the policy regime justify the causal claim.

Instead, Luebbert turns his analytic attention to identifying "the conditions that produced each of these coalitions" (452) -- 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 for why socialist parties allied with middle peasants rather than the agricultural proletariat in Norway and Denmark (thus yielding "corporatist democracy"). In Norway, he notes, there were very 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.[24] In Denmark, the socialists could not mobilize what was a much larger agricultural labor force, because "this population had already been heavily mobilized by another party" (p. 466) -- 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 they might have be mobilized by the socialists, and fascism rather than corporatism would have resulted. Thus a

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[24] 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.

particular fact about Danish pre-war politics becomes an "ultimate" cause of corporatism, rather than dictatorship or fascism, in this country.

COUNTERFACTUALS AND CAUSATION

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, Max Weber (1949) 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 <u>significance</u> is properly attributed to this individual decision in the context of the totality of infinitely numerous "factors" ... (164).

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), as they discussed the explicit counterfactualizing of some practitioners of the "New Economic History" (Fogel (1964), McClelland (1975), Gould (1969), Redlich (1965), Climo and Howells (1976)). The only political scientist I know of who has examined the topic at length is Jon Elster, particularly in his <u>Logic and Society</u> (1978, but see also Elster (1980) and (1983), Barry (1980), Lukes (1980)). Elster there presents a novel "branching worlds" theory of truth conditions for counterfactual propositions, and uses it to analyze some examples in economic history. Of course, outside the social sciences, analytic philosophers have been writing about counterfactuals and causation for years (Goodman 1947, Lewis 1973, Sosa 1975). While much of

this literature -- e.g., that concerned with "the metaphysics of modality" (Loux 1985) -- would seem largely irrelevant to working social scientists, some recent work on counterfactuals and explanation by philosophers of history has great practical value and probably deserves greater attention (e.g., Martin (1972, 1981, 1982)).

Wherever scholars have dealt with counterfactuals, they have often expressed dismay, doubt, and bewilderment at the sorts of logical and philosophical problems such propositions seem to entail. In this section I briefly introduce two problems that seem particularly bothersome to social scientists and historians. Due to space and competence constraints, I can only describe what the problems are, state why they are relevant, and indicate what I think are promising lines of argument about them.

The first is sometimes referred to as "the Cleopatra's Nose Problem" (e.g., Carr (1962), Gaddis (1989)). According to Pascal, if Cleopatra's nose had been an inch longer, Antony would not have been so infatuated, and the whole course of western history would have been different. Does this imply that the gene controlling Cleopatra's nose length 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" or fanciful 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. We would like to know, then, if there is anything that distinguishes the causal status of Cleopatra's nose length from, say, the cult of the offensive.

I would argue that there is. One line of 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. Intuitively, a "cause" is something the <u>produces</u> 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.[25] "Accidental" happenings that help lead to specific events are not to be called "causes," but only "conditions"; conditions of particular events which generalize or could "regularly produce" the effect are labelled causes.[26] 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. On the other hand, the distinction can do violence to common sense and ordinary usage: for example, the unlucky woman's death was not "caused" by the falling rock, it was caused by skull fracture; the rock was only a "condition."

Another approach would be more lenient with certain accidental happenings. We could argue that Cleopatra's nose being as long as it was 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. On this account, an accidental (or "random") happening -- say, a monkey bite leads to the death of a king, whose replacement begins a war (Carr (1962) citing Churchill) -- 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 phenomenom being explained. Though counterfactuals like (P2) might be explored to lend credence to a causal claim, a "cause" does something more than just satisfy (P2).[27]

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[25] On these see Beauchamp and Rosenberg (1981).

[26] On this distinction see also Mackie in Sosa (1975), and Martin (1972, 1982).

[27] A third suggestion for resolving this problem would be to add a condition of temporal 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. But this raises the question of "how long?"

The second problem concerns what Jon Elster (1978) calls the "legitimacy" of a counterfactual assertion. Elster argues that a counterfactual thought experiment undertaken to establish or confirm a causal hypothesis is not "legitimate" if we have a theory saying that the counterfactual "could not have happened." Suppose we wish to learn the effect of the railroad on the growth of 19th century American G.N.P., and attempt to do so by imagining the railroad's absence (Fogel 1964). Elster thinks it nonsensical to wonder if the internal combustion engine would have been invented earlier than it was (in the counterfactual 19th 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" (1983: 38) or "the unimportance of inevitable" (1978: 185): the better our theories, the fewer counterfactuals we can "legitimately" assert. Elster thinks this a fundamental problem for the counterfactual strategy, calling it "the basic paradox of counterfactuals" (1978: 184).

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 C "could not have been different" due to a complex of historical and cultural factors particular to country X.

Both Brian Barry (1980) and Steven Lukes (1980) have criticized Elster's notion of counterfactual "legitimacy," and I join them here, taking a slightly different approach. Whether event C "had to occur" or not has no bearing on its causal status with respect to E. A variable may serve to help explain one

outcome, and still itself be explained by the action of other variables. (In "large-N" work this set-up is commonly found in structural equations models, where a dependent variable in one equation may be an independent variable in another equation.) In the railroads example, we do not have to worry about whether our theory of technical change, given the values of the independent variables in that theory, predicts the invention of the railroad in the early 19th century. We simply suppose a 19th century without a railroad, and do not bother about whether in some sense it "could not have been" that way. If we have a theory saying that the internal combustion engine would then have been invented sooner, so much the better for the counterfactual comparison we are trying to flesh out. The counterfactual comparison case that allows us to make an inference about the contribution of the railroads to G.N.P. does not have to have been "really possible," it merely has to be an appropriate comparison.

Of course we then need to know what the "appropriate" counterfactual comparison would be. Elster's concern about "legitimacy" resurfaces here, in a slightly different form. Suppose we wish to assess the effect of the high dollar from 1981-85 on the U.S. trade deficit. The simplest counterfactual comparison would assume a dollar valued at, say, the December 1980 price, and then use elasticities (estimated from actual case comparisons) to get an estimate of the desired effect. But the high value of the dollar was itself a caused largely by government fiscal and monetary policies, so perhaps it is more appropriate to draw a counterfactual comparison where these policies differed in a way that would have produced a lower value for the dollar. But then these different policies may themselves have had an independent effect on the trade deficit, and this would change our estimate.

In practice, political scientists dealing with few cases and many vari-

ables go through these sorts of arguments quite often.[28] In effect, they are exploring counterfactual comparisons in greater and greater detail, questioning the plausibility of various scenarios and questioning whether certain factors "could have been different" due to the operation of other factors. The problem of the "legitimacy" or "appropriateness" of counterfactual comparison cases could either be a hopelessly unresolvable morass, or amenable to methodological discipline and rules. My impression is that counterfactual arguments are, on occasion, convincing and effectively decisive -- if so, the possibility that there can be "right" or at least generally good ways to draw counterfactual comparisons should be explored.[29]

CONCLUSION

Counterfactuals and the "counterfactual strategy" of hypothesis testing play an important but often unacknowledged and underdeveloped role in the explanatory efforts of political scientists. I have tried to show that any non-experimental 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 non-actual comparison cases. Particularly in small-N research, the common condition of negative degrees of freedom -- "too many variables and too few cases" -- make counterfactual "thought experiments" obligatory means for serious justification of causal claims. In practice, as some of the examples in the second section suggest, small-N analysts could strengthen (or simply specify) their causal arguments by <u>being explicit</u> about the counterfactual scenarios needed to support their causal hypotheses.[30] By making it clearer

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[28] Excellent examples can be found in the Sagan-Snyder "cult of the offensive debate (Sagan (1986), Snyder (1984), Snyder/Sagan (1986-87).

[29] The analogy with a structural equations system might be interesting path.

[30] Of course, I don't want to discourage anyone from looking for actual case comparisons.

what various causal arguments rest on, self-consciousness about the methodological role played by counterfactuals might have the effect of improving substantive and theoretical arguments in the field .

The analysis suggests a number of methodological problems concerning counterfactuals that may deserve more attention. Under exactly what research conditions, for example, will application of the "counterfactual strategy" seem more or less compelling than the "actual case strategy"? Could the results of large-N regression work be "checked" by applying the counterfactual strategy to individual cases in the sample?[31] Finally, both of the logical problems with counterfactuals discussed in the third section are relevant to current explanatory practice in political science, and both deserve fuller treatment. Given their often skeptical orientation towards the idea of "scientific explanation," historians may perhaps be forgiven for a general disinterest in the methodological issues and problems entailed by counterfactuals. Political scientists, on the other hand, really cannot.

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[31] Paul Huth (1988) takes exactly this approach. He tests hypotheses about the causes of successful deterrence first on a "large-N" data set of international incidents, using regression. He then shows why the regression results are plausible by illustrating them with cases studies drawn from the sample. Every case study is marked by counterfactual propositions that effectively go to support the "ceteris paribus" assumption in the first part of the analysis. David Friedman's (1986?) criticism of social science path models would also seem to turn on the issue of whether the regression results are really "plausible" or "imaginable" for particular cases.

References

Achen, Christopher (1982) Interpreting and Using Regression. Beverly Hills: Sage Publications.

Barry, Brian (1980) "Superfox." Political Studies 28, 1: 138-43.

Beauchamp, Tom and Alexander Rosenberg (1981) Hume and the Problem of Causation. Oxford: Oxford University Press.

Campbell, Donald (1975) "'Degrees of Freedom' and the Case Study." Comparative Political Studies 8, 2 (July): 178-193.

Carr, Edward Hallet (1962) What is History?. New York: Knopf.

Climo, T.A. and P.G.A. Howells (1976) "Possible Worlds in Historical Explanation." History and Theory 15: 1-20.

Cohen, Youssef (1987) "Democracy from Above: the Political Origins of Military Dictatorship in Brazil." World Politics 40, 1 (October): 30-54.

Deyo, Frederic (1987) The Political Economy of the New Asian Industrialism. Ithaca: Cornell University Press.

Elster, Jon (1978) Logic and Society: Contradictions and Possible Worlds. New York: John Wiley.

Elster, Jon (1980) "Reply to Comments." Inquiry 23, 2 (June): 213-32.

Elster, Jon (1983) Explaining Technical Change. Cambridge: Cambridge University Press.

Fogel, Robert (1964) Railroads and American Economic Growth. Baltimore: Johns Hopkins University Press.

Gaddis, John Lewis (1989) "Sufficiency, Necessity, and Paradigm Fratricide." Mimeo, Ohio University.

Goodman, Nelson (1947) "The Problem with Counterfactual Conditionals." Journal of Philosophy 44: 113-28.

Gould, J.D. (1969) "Hypothetical History." The Economic History Review (Second Series) 22, 2 (August): 195-207.

Gowa, Joanne (1984) "Hegemons, IOs, and Markets: the case of the substitution account." International Organization 38, 4 (Autumn): 661-683.

Huth, Paul (1988) Extended Deterrence and the Prevention of War. New Haven: Yale University Press.

Im, Hyug Baeg (1987) "The Rise of Bureaucratic Authoritarianism in South Korea." World Politics 39, 2 (January): 231-257.

Jervis, Robert (1988) "War and Misperception." Journal of Interdisciplinary History 18, 4 (Spring): 675-700.

Lebow, Richard Ned (1981) Between Peace and War. Baltimore: Johns Hopkins University Press.

Lebow, Richard Ned and Janice Gross Stein (1987) "Beyond Deterrence." Journal of Social Issues 43, 4: 3-71.

Levy, Jack (1988) "Domestic Politics and War." Journal of Interdisciplinary History 18, 4 (Spring): 653-673.

Lewis, David (1973) Counterfactuals. Cambridge: Harvard University Press.

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

Loux, Michael [ed.] (1985) The Possible and the Actual: Readings in the Metaphysics of Modality. Ithaca: Cornell University Press.

Luebbert, Gregory (1987) "Social Foundations of Political Order in Interwar Europe." World Politics 39, 4 (July): 449-78.

Lukes, Steven (1980) "Elster on Counterfactuals." Inquiry 23, 2 (June): 145-155.

Martin, Raymond (1972) "Singular Causal Explanations." Theory and Decision 2:221-237.

Martin, Raymond (1981) "Beyond Positivism: A Research Program for Philosophy of History." Philosophy of Science 48: 112-121.

Martin, Raymond (1982) "Causes, Conditions, and Causal Importance." History and Theory ...

McClelland, Peter (1975) Causal Explanation and Model-Building in History, Economics, and the New Economic History. Ithaca: Cornell University Press.

Mill, J.S. (1851) A System of Logic. London: John W. Parker.

Moore, Barrington (1966) Social Origins of Dictatorship and Democracy. Boston: Beacon Press.

Moore, Barrington (1978) The Social Bases of Obedience and Revolt. London: Macmillan.

Mueller, John (1988) "The Essential Irrelevance of Nuclear Weapons: Stability in the Postwar World." International Security 13, 2 (Fall): 55-79.

Mueller, John (1989) Retreat from Doomsday. New York: Basic Books.

Neuberg, Leland (1988) Conceptual Anomalies in Economics and Statistics: lessons from the social experiment. Cambridge: Cambridge University Press.

Pearce, David (1984) "Rationalizable Strategic Behavior and the Problem of Perfection." Econometrica 52: 1029-1050.

Polsby, Nelson, [ed.] (1982) What If? Essays in Social Science Fiction. Lexington: Lewis Publishing Co.

Posen, Barry (1987) "Competing Images of the Soviet Union." World Politics 39, 4 (July): 579-97.

Sagan, Scott (1986) "1914 Revisited." International Security 2, 2 (Fall): 151-175.

Shanks, J. Merrill (1982) "The Importance of Importance." Working Paper, Survey Research Center, University of California, Berkeley.

Skocpol, Theda (1979) States and Social Revolutions. Cambridge: Cambridge University Press.

Skocpol, Theda and Margaret Somers (1980) "The Uses of Comparative History in Macrosocial Inquiry." Comparative Studies in Society and History ...

Smoke, Richard (1977) War: Controlling Escalation. Cambridge: Harvard University Press.

Snyder, Jack (1984) "Civil-Military Relations and the Cult of the Offensive, 1914 and 1984." International Security 9, 1 (Summer): 108-146.

Snyder, Jack and Scott Sagan (1986-87) "Correspondence: The Origins of Offense and the Consequences of Counterforce." International Security 2, 3 (Winter): 187-198.

Sosa, Ernest [ed.] (1975) Causation and Conditionals. Oxford: Oxford University Press.

Stepan, Alfred (1978) "Political Leadership and Regime Breakdown: Brazil." In Linz and Stepan (1978).

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

Van Evera, Stephen (1984) "The Cult of the Offensive and the the Origins of the First World War." International Security 9, 1 (Summer): 58-107.

Waltz, Kenneth (1979) Theory of International Politics. New York: Random House.

Waltz, Kenneth (1981) "Another Gap?" In Osgood, Robert et al., Containment, Soviet Behavior, and Grand Strategy. Berkeley: Institute for International Studies, Policy Papers in International Affairs, no. 16.

Weber, Max (1949) "Objective Possibility and Adequate Causation in Historical Explanation." In The Methodology of the Social Sciences. New York: The Free Press.

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