
Single-Outcome Studies

A Methodological Primer

John Gerring
Boston University



abstract: Most methodological work on case studies understands this topic as a study of a case where the objective is to discover something about a broader population of cases. Yet, many case studies (so-called) do not assume this nomothetic goal; their aim is to investigate a bounded unit in an attempt to elucidate a single outcome occurring within that unit. This is referred to as a *single-outcome study* to distinguish it from the usual genre of case study. In this article, the author discusses the utility of single-outcome studies and the different types of argumentation and causal logic that they embrace. The author proceeds to discuss the methodological components of the single-outcome study, which is understood according to three analytic angles: *nested analysis* (large-*N* cross-case analysis), *most-similar analysis* (small-*N* cross-case analysis) and *within-case analysis* (evidence drawn from the case of special interest). The article concludes with a discussion of a common difficulty encountered by single-outcome analysis, that is, reconciling cross-case and within-case evidence, both of which purport to explain the single outcome of interest.

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The term 'case study' refers to many things (Gerring, 2007: Ch. 2). Consequently, there are many varieties of case study. Arguably, the most important distinction is that which separates a focused study that reflects upon a larger population and a study that purports to explain only a single case. The first might be labeled *nomothetic* and the second *idiographic*, though these terms are somewhat misleading, for reasons explored later.

This fundamental distinction is reproduced in various languages and social science traditions. In English, for example, the term case study usually connotes a study whose analytic objective is larger than the case under intensive research. Here, case means 'example' or 'instance' of a

broader population of cases. In French, however, the analogous term, *analyse de cas*, is usually understood to mean the study of a particular phenomenon, which may or may not reproduce features of a larger population.

To date, most of the methodological work on this subject has been focused on case studies of the nomothetic sort.¹ This follows the dominant tradition in the Anglo-American scholarly community and the greater preoccupation with questions of method in that community. Even so, there are many situations in which one might wish to understand specific circumstances, rather than circumstances in general. Pauline Young (1939: 235–6) clarifies that the ‘social case work’ often refers to specific contexts.

In social case work we do not gather data in order to compare, classify and analyze with a view to formulating general principles. We gather the data case by case in order to make a separate, differential diagnosis, with little or no regard for comparison, classification and scientific generalization. The diagnosis is made with a view to putting treatment into operation in this particular case.

While scholars of international relations have an interest in wars, they are also interested in the causal factors that led to specific wars, particularly big wars with large consequences. Every country, every region, every business, every era, every event, every individual, for that matter every phenomenon that a substantial number of people care about, inspires its own idiographic research agenda. Citizens of Denmark wonder why Denmark has turned out the way it has. Chinese-American immigrants wonder why this group exhibits certain sociological and political patterns. Every public figure of note or notoriety sooner or later becomes the subject of a biography or autobiography. Indeed, the vast majority of the books and articles published in a given year are focused on particular events or outcomes.

Indeed, social scientists, in common with the laity, are interested in how their chosen subject plays out in their country of origin or of residence. Thus, French economists study the French economy; French sociologists study French society; and French political scientists study French politics. Of course, they may study particular aspects of these broad subjects; the point is that their concern in this genre of work is not a class of outcomes but rather a particular outcome pertaining to a particular country. It is not merely idle curiosity that fuels this sort of research. Understanding who we are, as individuals and as communities, rests, in part, on an understanding of what factors have made us who we are.

Why is the US a welfare state laggard (Alesina and Glaeser, 2004; Amenta, 1991; Orloff, 1988; Skocpol, 1996)? What caused the First World War (Goertz and Levy, forthcoming)? What accounts for the rise of the West (North and Thomas, 1973)? How can the inception and growth of

the European Union be explained (Moravcsik, 1998)? What are the causes of the terrorist attack on the World Trade Center that occurred on 11 September 2001 (*The 9/11 Commission Report*, 2003)? Evidently, this genre of question is not at all unusual in the social sciences.

Moreover, there are circumstances in which we may be more certain about the explanation of a particular outcome than of a class of outcomes. This is apt to be true when the cross-case evidence available on a topic is scarce and heterogeneous and/or where the proximal evidence available for a given case is strong. Consider the following two questions: (1) Why do social movements happen?, and (2) Why did the American civil rights movement happen? The first question is general, but its potential cases are few and extremely heterogeneous. Indeed, it is not even clear how one would construct a universe of comparable cases. The second question is narrow, but eminently answerable. I hasten to add that quite a number of factors may provide plausible explanations for the American civil rights movement and the methodological grounds for distinguishing good from bad answers is not entirely clear. Even so, I find work on this idiographic question more convincing than work on the nomothetic question of what causes social movements.²

As a second example, one might consider the contrast between the defining questions of two disciplines, the sociology of crime and criminal justice. The first inquires into what causes crime; the second wishes to ascertain who committed a particular crime. It is easy to envision situations in which answers about the latter would be more secure than answers about the former. Often, that is, criminal justice verdicts are more dependable than criminal justice studies. We are able to convict or acquit in most cases with a high degree of certainty, while we are unable to dispense with general questions about crime.

In sum, idiographic analysis is both intrinsically valuable, because we wish to know who did what to whom, and at least in some circumstances, methodologically tractable. There must, then, be some general principles upon which idiographic analysis rests, a topic which this article undertakes to address. Before beginning, we must spend some time defining terms, which I have done in a preliminary but, as will shortly be clear, quite unsatisfactory way.

Defining the Topic

Conventionally (in the Anglo-American academy), case study research implies the study of a population of cases through intensive study of one or several examples of that phenomenon. The population may be small or large, but the method is synecdochic. It infers a larger whole from a smaller part (Gerring, 2004).³ This is what I have referred to, provisionally,

as a 'nomothetic' case study. At times, however, the case study may refer to a piece of research whose inference is limited to the case under study. This sort of case study is narrowly scoped to one particular (relatively bounded) unit, and in this respect may be labeled 'idiographic'.

The problem with this terminology is that the terms nomothetic/idiographic carry so many extraneous meanings, many of which are inappropriate to our subject. For example, nomothetic implies to most readers that the generalization is of a precise, universal (law-like) nature. This may or may not be true of a generalizing case study. Similarly, idiographic implies that a study focuses on the unique qualities of a case. This, also, is a matter of degree and need not be true at all. One may wish to investigate a particular outcome of special importance even though it reflects universal causal properties.

Thus, I impose a somewhat different vocabulary on our subject matter. Narrowly scoped studies that reflect upon a broader population will be referred to as *case studies* (the usual Anglo-American usage). Studies that investigate a bounded unit in an attempt to elucidate a single outcome occurring within that unit will be referred to as *single-outcome studies*. The latter is my primary concern. However, since this concept gains meaning only by reference to its counterpart, the article proceeds through series of contrasts drawn between case studies and single-outcome studies.

Note that for present purposes it is assumed that all case studies make causal claims. Naturally, such studies may also make descriptive claims. However, this article explores the logic and method of case study analysis where the principal objective is to establish a causal relationship between X and Y. Thus, methodological issues pertaining only to descriptive inference are ignored.

What does it mean, then, to investigate a 'single outcome for a single case'? The phrase seems rather obscure, if not obscurantist. Yet, it is essential. Note that the definition of a case has nothing to do with the temporal or spatial boundaries of a subject. Cases may be big (countries, continents, the world) or small (individuals, events). A case must be 'bounded' in some fashion, and it must reflect the primary inference that a writer is attempting to demonstrate or prove. If the argument is about nation-states, then the latter are regarded as cases, even though sub-case observations (e.g. individuals) or supra-case observations (e.g. continents) may be enlisted as part of the argument. In short, cases always rest at the same level of analysis as the primary inference. (It follows that whenever this inference changes, the definition of 'case' may change.)

Now, let us turn to the definition of a 'single outcome'. A single-outcome study, to reiterate, refers to a situation in which the researcher seeks to explain a single outcome for a single case. This outcome may register a change on Y, something happens. Or it may register stasis on

Y, something that might have happened but, in the event, does not. That is, the outcome may be 'positive' or 'negative'. The actual duration of the outcome may be short (eventful) or long (static). A revolution (e.g. the American Revolution) and a political culture (e.g. American political culture) are both understood as outcomes since they register distinct values for a single case.

For the statistically minded, the single-outcome study may be understood as a study oriented toward explaining the point score for a single case rather than a range of values across a population of cases, or a range of values occurring within a single case. One might also describe this as a 'single-observation study', for an outcome may be recorded on a single line of a rectangular data set, registering only one value for each of the relevant variables. However, the term is awkward and also somewhat misleading since it implies that there are other comparable observations adjacent to the observation of interest, which may or may not be true. In any case, the key contrast is with studies that purport to explain a *range* of variation on an outcome, exemplified across a population of cases.

By way of illustration, consider the following research questions focused on the contemporary welfare state (as measured by revenue or expenditure levels as a share of total GDP):

1. What explains the relatively weak American welfare state?
2. What explains welfare state development within the OECD?
3. What explains variation in US welfare spending over time?
4. What explains variation in US welfare spending across states?

Of these, only the first is correctly classified as the study of a single outcome. Research question 1 pertains to a single outcome because the case (the US) is understood to have achieved a single, relatively stable, value on this outcome. There is implied variation on the dependent variable (welfare state spending) across cases; indeed, the research question implicitly compares the US to other countries. However, the researcher is not interested in explaining that variation; she or he is, instead, motivated to explain the point score of the US. By contrast, research question 2 envisions the study of a population because it establishes a range of variation, at least one differentiable outcome for each OECD country; more if each case is observed over time. Research question 3 recasts the first question from a single outcome to a population because it is focused on a range of comparable outcomes within a single case (the US), defined temporally. Research question 4 defines the population spatially, rather than temporally; this is also a population.

A study focused on a single outcome must, of necessity, interrogate within-case evidence, and may therefore construct comparable observations within the case of primary interest. Thus, in answering research

question 1, a researcher might employ strategies 3 and 4. Similarly, a single-outcome study might incorporate evidence drawn from adjacent cases (at the same level of analysis), i.e. strategy 2. However, if the purpose of this within-case and across-case evidence is to illuminate a single outcome within a single case, it is still appropriately classified as a single-outcome study.

In philosophical work, the fundamental contrast between the case study and single-outcome study may be understood as a distinction between a cause in general and a cause in fact (Hart and Honore, 1959). In the one instance, the American experience is enlisted to help explain something about welfare state development everywhere (or at least within the OECD). In the other instance, what we know about welfare states in general is enlisted to help explain one particular case. Much is superficially the same about these two topics; however, the objective has shifted from macro to micro. As we see, this matters quite a lot.

Things become slightly more complicated when a given study encompasses a *series* of non-comparable outcomes connected with a single case, as many narrative studies do. There is no single topic, in this instance, but rather a set of disparate topics. Thus, a study of the American welfare state that has no clearly defined outcome (presumably there are a range of phenomena that define the American welfare state) is probably best classified as a *multiple single-outcome* study. For heuristic purposes, I speak of the single-outcome study as if there were a central proposition towards which a given study was oriented. Insofar as a study embraces diverse propositions, it embodies multiple single-outcome inferences. This should be clear enough in context.

Similarly, many studies operate at both levels. They purport to say something about a more general subject as well as about the specific contours of the case under study. These sorts of studies are both case studies and single-outcome studies. There is nothing wrong with mixing and matching in this fashion; indeed, it is quite typical. What is essential, however, and often neglected, is a clear separation between these two motives. It must be explicit when an author is generalizing and when she or he is particularizing. Only in this fashion can the reader evaluate an author's claims.

Arguments

With these definitional matters under our belts, we may now proceed. What difference does it make if an author is studying a case (of something broader than itself) or an outcome within a case?⁴ What is the difference between (a) studying the US welfare state as an example of welfare state development more generally and (b) as a topic in its own right?

I begin by discussing the utility of single-outcome studies and the different types of argumentation and causal logic that they embrace. I proceed to discuss the methodological components of the single-outcome study, which may be reduced to three angles: *nested analysis* (large-*N* cross-case analysis), *most-similar analysis* (small-*N* cross-case analysis) and *within-case analysis* (evidence drawn from the case of special interest). The article concludes with a discussion of a common difficulty encountered by single-outcome analysis, that is, reconciling cross-case and within-case evidence, both of which purport to explain the single outcome of interest.

It is important to bear in mind that this article focuses on the distinctive methodological features of single-outcome studies. I leave aside, or treat lightly, issues that apply equally to both genres. Thus, wherever I choose *not* to expatiate on a point the reader can assume that rules applying to case studies (Gerring, 2007) also apply to single-outcome studies.

As with case study research, it is essential for the researcher to specify a clear hypothesis, or at least a relatively clear research question. This point deserves special attention in light of the seeming obviousness of research connected with a particular outcome. If one is studying the First World War it seems self-evident that the researcher will be attempting to explain this historical fact. The problem is that this is a very immense fact, and consequently can be seen from many angles. 'Explaining the First World War' could mean (1) why did the war occur (at all)? (2) why did it break out in 1914? (3) why did it break out on 28 June 1914? (4) why did it break out in the precise way that it did (i.e. shortly after the assassination of Archduke Ferdinand)? (5) why was it prosecuted in the manner that it was? and so forth. Option (1) suggests, but does not mandate, a focus on antecedent (structural, distal) causes, while the other options suggest a focus on proximate causes (of many different kinds). Evidently, the way the research question is posed is likely to have enormous impact on the chosen research design, not to mention the sort of conclusion that the author reaches. While this is true of any study, cross-case, case study or single-outcome, it is particularly true for studies that focus on individual outcomes. Thus, single-outcome analysis requires the author to work hard to define and operationalize the outcome that she or he is trying to explain.

Andrew Bennett (1999: 1) begins his book-length study by outlining a general quest: 'to explain the rise of Soviet military interventionism in the 1970s, its fall in the 1980s, and its reprise in the form of Russia's interventions in the former Soviet republics and Chechnya in the 1990s'. (I assume that these are three discrete and relatively non-comparable outcomes, rather than a range of outcomes along a single dimension. If the latter, then Bennett's study would be more appropriately classified as the study of a population rather than of a series of single outcomes.) Subsequently,

Bennett (1999: 15) lays out more a nuanced indicator of interventionism. This 'escalation ladder' includes the following steps: (1) shipment of arms to the client regime; (2) transport of non-Soviet troops to or in the client regime; (3) direct supply of non-Soviet troops on the front; (4) deployment of Soviet military advisers in the war zone; (5) military aid to allied troops in 'proxy' interventions; (6) use of Soviet troops in combat roles; (7) massive scale in the above activities; (8) use of Soviet commanders to direct the military campaign; and (9) use of Soviet ground troops.⁵ This is a good example of how a general topic can be operationalized in a clear and falsifiable manner.

Granted, many of the single-outcome studies produced by academics and by lay writers do *not* have clearly defined outcomes. A general history of the First World War is about many things related to the First World War. A general history of Denmark is likely to focus on many things related to Denmark. A biography of Stalin is about many things related to Josef Stalin. There is nothing wrong with this traditional variety of historical, ethnographic or journalistic narrative. Indeed, most of what we know about the world is drawn from this genre of work. (My bookshelves are filled with them.) However, we must also take note of the fact that this sort of study is essentially unfalsifiable. It cannot be proven or disproven, for there is no *argument* per se. It is causal analysis only in the loosest sense. My injunction for a clear hypothesis or research question is applicable only if the objective of the writer is causal-explanatory in a stricter, and more scientific, sense. It is this sort of work, a small minority of single-outcome studies, that concerns us here.

Not only the outcome, but also the causal factor(s) of interest, must be clearly specified. Again, one finds that this is often more complicated in single-outcome analysis than it is in case study analysis, precisely because there is no larger field of cases to which the inference applies. Douglass North and his colleagues (North et al., 1983: 2–3) take note of the following traditional arguments pertaining to US economic history:

1. British policy was vindictive and injurious to the colonial economy after 1763.
2. The railroad was indispensable for American economic growth.
3. Speculators and railroads (through land grants) monopolized the best western lands in the 19th century, slowed down the westward movement, adversely affected the growth of the economy, and favored the rich over the poor.
4. In the era of the robber barons, farmers and workers were exploited.

In looking closely at these arguments, North et al. observe that they are ambiguous because there is no clearly specified counterfactual. The authors therefore revise them as follows (North et al., 1983: 3):

1. British policies were restrictive and injurious to the colonial economy after 1763, compared with what would have taken place had the colonies been independent during these years; or more precisely, income of the Colonies under British rule after 1763 was less than it would have been had the colonists been free and independent.
2. Income in the US would have been reduced by more than 10 percent had there been no railroads in 1890.
3. A different (but specified) land policy would have led to more rapid westward settlement in the 19th century, a higher rate of economic growth and a more equal distribution of income.
4. In the absence of the monopolistic practices of the robber barons, farm income and real wages of manufacturing workers would have been significantly higher.

Here are a set of falsifiable hypotheses. They specify an outcome, an alternative outcome and a causal factor that is held to be accountable for (imagined) variation across those outcomes.

Another point to be aware of is that the way in which an outcome is defined is likely to determine the extent to which it is comparable to other cases. Specifically, the more detailed the outcome, the more it is tailored to the circumstances of a specific country, group or individual, the more difficult it will be to make reference to instances outside the area of interest. If the case of special interest is defined too idiosyncratically it will no longer be a case of anything; that is, it will no longer be comparable (except in the most anodyne and unilluminating way) to other cases. Since one's objective is to explain *that* outcome, and not others, this is not necessarily problematic. However, it does mean that the author will be restricted to evidence drawn from that particular case. This is a serious restriction and limits the falsifiability of any proposition, since it cannot be tested across other venues.

Causal Logic

Before going any further it is important to point out that the causal logic employed in case studies is often quite different from that of single-outcome studies, and this difference stems from their different objectives. Because the case study is focused on developing an explanation for some more general phenomenon it usually focuses on a particular causal factor, X_1 , and its relationship to a class of outcomes, Y . It usually culminates in an X_1/Y -centered hypothesis that explains some, perhaps quite small, degree of variation across Y .

While it is a reasonable objective to seek to explain some small degree of variation across a wide range of cases, it is not a very reasonable objective to seek only to explain some small degree of variation across *one*

outcome. Wherever a study focuses on a single outcome, the reader quite naturally wants to know everything, or almost everything, about the causes of that outcome (leaving aside the obvious background factors that every causal argument takes for granted). Thus, single-outcome studies usually seek to develop a more or less 'complete' explanation of an outcome, including *all* causes that may have contributed to it, X_{1-N} .

Single-outcome studies make extensive use of necessary and sufficient conditions – deterministic ways of understanding causal relations. In case studies, by contrast, researchers usually assume probabilistic causal relations. The simple reason for this is that a general outcome encompassing multiple cases is less likely to conform to an invariant law. While there were undoubtedly necessary conditions for the occurrence of the First World War, there is debate among scholars over the existence of necessary conditions pertaining to wars in general. (Only one such necessary condition has been proposed, non-democracy, and it is hotly contested [Brown et al., 1996].) As a rule, the larger the class of outcomes under investigation the more likely it is that there will be exceptions, in which case the scholar rightly conceptualizes causes as probabilistic rather than necessary and/or sufficient.

Relatedly, because case studies seek general causes they tend to focus on structural causal factors. Because single-outcome studies seek the causes of specific outcomes, whether or not they apply to other outcomes, they often focus on contingent causal factors, e.g. leadership, decisions, or other highly proximate factors. The assassination of Archduke Ferdinand is a plausible explanation of the First World War, but it is not a good explanation for wars in general (Lebow, 2000–1; Lebow and Stein, 2004). It might of course be enlisted as an example of a more general explanation, but the author's emphasis would be on this more general factor, e.g. 'triggering events'. Proper nouns are often embraced in the single-outcome study, while they must be regarded merely as examples of some broader phenomenon in the case study.

However, it would be wrong to conclude that because unique causes are admissible in single-outcome studies, they are also preferable. One does not imply the other. That is, an individual outcome may be the product of a very general cause (a 'law'). Or it may not. Indeed, there is no reason to presume that a case chosen for special study is different from a broader class of cases merely because it happens to form the topic of interest. A study of the American welfare state should not assume, as a point of departure, that the causal dynamics of welfare state development are fundamentally different from those unfolding in Europe and perhaps elsewhere in the world. An inquiry into a murder should not assume that the causes of this murder are any different from the causes of other murders. And so forth. The question of similarity and difference in causal

analysis, the comparability of cases, is thus rightly left open: a matter for investigation.⁶ To clarify, single-outcome research designs are open to idiographic explanation in a way that case study research is not. But single-outcome researchers should not assume, *ex ante*, that the truth about their case is contained in factors that are specific to that case.

Granted, there is an affinity between single-outcome analysis and idiographic explanations insofar as the outcomes that attract greatest attention from social scientists are apt to be outcomes that are non-routine, outcomes that don't fit into standard explanatory tropes. However, the 'uniqueness' of a historian's (or political scientist's or sociologist's) chosen topic is a poor point of departure, encouraging a prejudiced style of investigation into the actual causes of that outcome, which may be more routine than is generally realized.

Analysis

The analysis of a single outcome may be approached from three different angles. The first, which I refer to (following Evan Lieberman [2005]) as *nested analysis*, employs cross-case analysis from a large sample in order to better understand the features of an individual outcome. The second, known most commonly as *most-similar analysis*, employs cross-case analysis within a small sample (e.g. two or three cases). The third, known generically as *within-case analysis*, draws on evidence from within the case of special interest.⁷

The latter two methods are quintessentially case study methods, so my discussion of these topics builds on arguments introduced elsewhere. Gerring and McDermott (forthcoming) argue that all case studies can be understood as variants of the experimental research design. Specifically, we show that case study methods can be classified according to four archetypal methods: the dynamic comparison (which mirrors the paradigmatic laboratory experiment, exploiting both temporal and spatial variation), the longitudinal comparison (which employs only temporal variation but is similar in design to the experiment without control), the spatial comparison (which employs only variation through space, purporting to measure the outcome of interventions that occurred at some point in the past but are not directly observable), and counterfactual comparison (which relies on imaginary variation, i.e. where the researcher seeks to replicate the circumstances of an experiment in her or his head or with the aid of some formal model). This typology forms the backbone of the discussion of most-similar analysis and within-case analysis.

For heuristic purposes, I pursue a single research question, raised at the outset of this article: Why does the US have a small welfare state? This hoary concept is operationalized as aggregate central government revenue,

understood as a share of GDP. As previously, research on this topic may be either exploratory (Y-centered) or confirmatory (where there is a specific X_1/Y hypothesis that the researcher is intending to prove or disprove). How, then, might (1) nested analysis, (2) most-similar analysis and (3) within-case analysis be applied to this classic research question?

Nested Analysis

A nested analysis presumes that the researcher has at her or his disposal a large- N cross-case data set containing variables that measure the outcome and at least some of the factors that might affect that outcome.⁸ With this information, the researcher attempts to construct a general model of the phenomenon that applies to the broader sample of cases. This model, which may be cross-sectional or time-series-cross-sectional, is then employed to shed light on the case of special interest.

Let us begin at the descriptive level. How exceptional (unique) is the American state? The first section in Table 1 lists all minimally democratic countries for which central government expenditure data are available in 1995 ($N = 77$), along with their normalized 'Z' scores (standard deviations from the mean). Here, it will be seen that the US has a moderately low score, 17th out of 77, but not an extremely low score, relative to other democratic polities. Thus viewed, there is little to talk about; the American case is only moderately exceptional.

However, the traditional way of conceptualizing the question of American exceptionalism utilizes an economic baseline to measure countries' welfare states. The presumption is that richer, more developed societies are likely to tax and spend at higher rates ('Wagner's Law'). This way of viewing things may be discerned through an ordinary least squares analysis of government spending, regressed against GDP per capita (natural logarithm), as follows:

$$\text{Expenditure} = 4.89 + 3.10 \text{ GDPpc} \quad (1)$$

$$R^2 = .1768 \quad N = 77$$

The residuals produced from this analysis for all 77 cases are listed in the second column of Table 1. Here, it will be seen that the US is indeed a highly exceptional case. Only four countries have higher negative residuals. Relative to its vast wealth, the US is a very low spender.

Why is the US so poorly explained by this bivariate model? Why does this rich country tax and spend at such low rates? While there are many hypotheses, drawn from a rich and storied research tradition (Marks and Lipset, 2000), I restrict myself here to the role of political institutions. Arguably, democratic institutions that centralize power, strengthen political parties and condition an inclusive style of governance will stimulate

the growth and maintenance of a welfare state (Huber and Stephens, 2001; Huber et al., 1993; Swank, 2002). Hence, one ought to find larger welfare states in countries with unitary (rather than federal) constitutions, list-proportional (rather than majoritarian or preferential-vote) electoral systems, and parliamentary (rather than presidential) executives. To test these propositions across cases, one must code extant democracies on all three dimensions. Since these are complicated institutional features, with many admixtures, I employ three-point scales for each variable. UNITARISM: 1 = federal, 2 = semi-federal, 3 = unitary. PR: 1 = majoritarian or preferential-vote, 2 = mixed electoral system, 3 = proportional. PARL: 1 = presidential, 2 = semi-presidential, 3 = parliamentary.⁹ With this information for each of the world's 77 democracies, one may then regress expenditure levels on GDP per capita plus these additional factors:

$$\text{Expenditure} = -3.92 + 2.52 \text{ GDPpc} + 2.66 \text{ UNITARISM} + 1.19 \text{ PR} + 3.60 \text{ PARL} \quad (2)$$

$$R^2 = .445 \quad N = 77$$

Residuals from this equation are presented in the third column of Table 1. The striking result is that the US case has lost its outlier status. Indeed, it lies extremely close to the predicted value of the multivariate model.

One might conclude from this analysis that one has effectively 'explained' the American case. Of course, any such conclusion rests on lots of assumptions pertaining to the veracity of the general model, the statistical technique, possible measurement error, the homogeneity of the population, and so forth. One can think of many different ways to model this problem, and one would probably want to make use of time-series data in doing so. I have kept things simple with the goal of illustrating the potential of nested analysis, when circumstances warrant.

Note that even if the model provides a good fit for the case of special interest, as in Equation 2, there still may be strong reasons for supplementing nested analysis with a more intensive analysis of the case of special interest or of adjoining cases, as discussed in subsequent sections. However, in this circumstance the purpose of a researcher's case-based analysis is likely to shift from (1) exogenous causal factors to (2) causal mechanisms. How might American political institutions have contributed to its welfare state trajectory? Are these the critical causal variables that the general model supposes (or are there reasons to doubt)? If, on the other hand, a case is poorly explained by a general model (i.e. the residual is high), this also offers important clues for subsequent analysis. Specifically, one has strong reason to presume that additional factors are at work, or alternatively, that the outcome is a product of pure chance (something that cannot be subjected to general explanation).

Table 1 *Three Nested Analyses of the US Welfare State*

Descriptive analysis			Bivariate analysis		Multivariate analysis	
Country	Exp.	Z score	Country	Res. 1	Country	Res. 2
1. Colombia	13.37	-(1.71)	1. South Korea	-17.15	1. South Korea	-18.39
2. India	14.84	-(1.57)	2. Argentina	-16.73	2. Thailand	-16.71
3. Dominican Rep.	15.39	-(1.51)	3. Colombia	-15.62	3. Bahamas	-15.97
4. Paraguay	15.41	-(1.51)	4. Bahamas	-15.05	4. Turkey	-14.04
5. Argentina	15.75	-(1.48)	5. United States	-14.85	5. Mauritius	-12.33
6. Thailand	15.78	-(1.47)	6. Mexico	-13.90	6. Paraguay	-9.99
7. Mexico	15.92	-(1.46)	7. Thailand	-13.75	7. Iceland	-8.98
8. South Korea	16.52	-(1.40)	8. Paraguay	-12.81	8. Canada	-8.48
9. Nepal	16.54	-(1.40)	9. Dominican Rep.	-12.25	9. Nepal	-8.25
10. Madagascar	17.39	-(1.32)	10. Venezuela	-11.63	10. Argentina	-7.81
11. Philippines	17.93	-(1.26)	11. Switzerland	-11.33	11. Costa Rica	-7.15
12. Venezuela	18.57	-(1.20)	12. Chile	-11.14	12. Peru	-6.80
13. Bahamas	19.03	-(1.16)	13. Canada	-10.49	13. New Zealand	-6.07
14. Peru	19.07	-(1.15)	14. Australia	-10.32	14. Dominican Rep.	-5.70
15. Chile	19.85	-(1.08)	15. Peru	-9.72	15. Madagascar	-5.47
16. Bolivia	21.10	-(0.95)	16. Malaysia	-8.82	16. Lithuania	-5.12
17. United States	21.72	-(0.89)	17. Philippines	-8.60	17. Malaysia	-5.04
18. Malaysia	21.98	-(0.87)	18. India	-8.45	18. Colombia	-4.98
19. Turkey	22.23	-(0.84)	19. Costa Rica	-7.66	19. Greece	-4.71
20. Costa Rica	22.43	-(0.82)	20. Turkey	-7.23	20. St Vincent/Grenad.	-4.33
21. Pakistan	22.80	-(0.79)	21. Mauritius	-6.82	21. Trinidad & Tobago	-4.15
22. Mauritius	23.26	-(0.74)	22. Nepal	-4.99	22. India	-3.41
23. Mongolia	24.38	-(0.63)	23. Panama	-4.98	23. Panama	-3.36
24. Panama	24.71	-(0.60)	24. Bolivia	-4.87	24. Grenada	-3.25
25. Canada	25.04	-(0.57)	25. Madagascar	-4.44	25. Latvia	-3.17
26. Lithuania	25.22	-(0.55)	26. Iceland	-3.46	26. Australia	-2.99
27. Australia	25.33	-(0.54)	27. Russia	-3.44	27. Chile	-2.79

Table 1 *Continued*

Descriptive analysis			Bivariate analysis		Multivariate analysis	
Country	Exp.	Z score	Country	Res. 1	Country	Res. 2
28. Russia	25.39	−(0.53)	28. Germany	−3.10	28. Fiji	−2.44
29. Switzerland	26.64	−(0.41)	29. Lithuania	−2.83	29. Ireland	−1.99
30. Grenada	28.11	−(0.27)	30. Uruguay	−2.78	30. Mexico	−1.64
31. Trinidad/Tobago	28.13	−(0.26)	31. New Zealand	−2.67	31. Denmark	−1.24
32. Fiji	28.61	−(0.22)	32. Trinidad/Tobago	−2.60	32. Luxembourg	−1.19
33. Uruguay	28.88	−(0.19)	33. Grenada	−1.49	33. Mongolia	−0.97
34. St Vincent/Grenad.	29.16	−(0.16)	34. Cyprus, Greek	−1.40	34. Norway	−0.95
35. Papua New Guinea	29.21	−(0.16)	35. Pakistan	−1.31	35. Albania	−0.80
36. Sri Lanka	29.33	−(0.15)	36. Greece	−1.07	36. Bolivia	−0.77
37. Vanuatu	29.34	−(0.15)	37. Fiji	−0.61	37. Venezuela	−0.48
38. South Africa	30.57	−(0.03)	38. South Africa	0.11	38. Cyprus, Greek	−0.17
39. Albania	31.01	(0.02)	39. St Vincent/Grenad.	0.20	39. United States	−0.14
40. Romania	31.78	(0.09)	40. Spain	0.58	40. Vanuatu	−0.07
41. Latvia	32.20	(0.13)	41. Mongolia	1.00	41. Estonia	−0.03
42. New Zealand	32.32	(0.15)	42. Ireland	1.36	42. Switzerland	0.04
43. Cyprus, Greek	32.60	(0.17)	43. Luxembourg	1.66	43. Spain	0.05
44. Greece	32.70	(0.18)	44. Norway	1.77	44. Uruguay	0.39
45. Congo, Rep.	32.82	(0.19)	45. Vanuatu	2.13	45. Papua New Guinea	0.50
46. Iceland	32.88	(0.20)	46. Papua New Guinea	2.87	46. Slovenia	0.59
47. Germany	33.72	(0.28)	47. Latvia	3.85	47. Czech Rep.	1.11
48. Nicaragua	34.09	(0.32)	48. Sri Lanka	3.93	48. Portugal	1.14
49. Lebanon	35.18	(0.43)	49. Denmark	4.12	49. South Africa	1.45
50. Spain	35.22	(0.43)	50. Romania	4.45	50. Philippines	1.58
51. Namibia	35.43	(0.45)	51. Czech Rep.	4.93	51. Botswana	1.74
52. Jamaica	35.44	(0.45)	52. Finland	4.97	52. Finland	1.80
53. Estonia	35.51	(0.46)	53. Austria	5.18	53. Germany	2.09
54. Moldova	35.72	(0.48)	54. Estonia	5.49	54. United Kingdom	2.25

Table 1 Continued

Descriptive analysis			Bivariate analysis		Multivariate analysis	
Country	Exp.	Z score	Country	Res. 1	Country	Res. 2
55. Botswana	35.98	(0.50)	55. United Kingdom	5.57	55. Malta	2.33
56. Czech Rep.	36.21	(0.53)	56. Albania	5.57	56. Austria	2.38
57. Ireland	36.67	(0.57)	57. Lebanon	5.74	57. Namibia	3.35
58. Norway	38.93	(0.79)	58. Botswana	6.10	58. Romania	3.50
59. Malta	39.06	(0.81)	59. Malta	6.13	59. Jamaica	4.69
60. Luxembourg	39.67	(0.87)	60. Jamaica	6.59	60. Lebanon	4.89
61. Slovenia	39.92	(0.89)	61. Namibia	6.70	61. Sri Lanka	4.99
62. Poland	40.35	(0.93)	62. Slovenia	6.70	62. Russia	5.04
63. Portugal	40.81	(0.98)	63. Portugal	7.17	63. Sweden	5.54
64. Bulgaria	40.96	(0.99)	64. Congo, Rep.	7.18	64. Bulgaria	6.16
65. United Kingdom	41.04	(1.00)	65. France	9.51	65. Moldova	6.49
66. Finland	41.25	(1.02)	66. Moldova	10.49	66. Israel	6.53
67. Denmark	41.36	(1.03)	67. Nicaragua	10.54	67. Pakistan	7.48
68. Austria	41.91	(1.08)	68. Poland	10.81	68. Congo, Rep.	8.28
69. France	45.98	(1.48)	69. Sweden	11.04	69. Netherlands	9.15
70. Israel	47.18	(1.60)	70. Belgium	11.09	70. Nicaragua	9.83
71. Sweden	47.54	(1.64)	71. Netherlands	11.99	71. Poland	10.27
72. Belgium	47.61	(1.64)	72. Israel	12.34	72. France	12.64
73. Italy	48.13	(1.69)	73. Italy	12.70	73. Hungary	13.18
74. Netherlands	48.46	(1.73)	74. Bulgaria	13.31	74. Italy	13.50
75. Hungary	49.39	(1.82)	75. Hungary	18.55	75. Belgium	13.56
76. Lesotho	49.53	(1.83)	76. Seychelles	20.56	76. Lesotho	19.97
77. Seychelles	52.75	(2.15)	77. Lesotho	25.40	77. Seychelles	23.83

Exp. = central government expenditure/GDP. Z score = standard deviations from the mean. Res. 1 and 2 = residuals from Equation 1 and 2, respectively.

Most-Similar Analysis

The most-similar method, which hails back to the work of J. S. Mill, refers to the choice of a few cases that share similar background conditions that might affect an outcome of interest but are different on the outcome itself, and perhaps also on a variable of theoretical interest.¹⁰ This method is no different in the context of a single-outcome analysis, with the exception that one of the most-similar cases is preselected. It follows that the chosen comparison case (or cases) should be that which is most similar to the case of special interest in all respects *except* the dimension(s) of interest to the researcher.

If the researcher has no hunch about the possible causes of American welfare state development then the search for a most-similar case involves matching the US to another case that has a higher level of government expenditure and is fairly similar on various dimensions that might affect this outcome. Britain or Canada might fit the bill since both have similar political cultures and larger welfare states. The research then consists of examining these cases closely to try to identify some contrasting feature that might explain their different trajectories.

If the researcher has a hunch about why the US has low levels of welfare spending then the task of finding a paired comparison is more determinate. In this situation, one searches for a country with higher welfare spending, a different status on the variable of interest, and similarities on all other factors that might affect the outcome. Let us say that the researcher's hypothesis concerns the constitutional separation between executive and legislature, the American doctrine of 'separation of powers'. In this circumstance, Canada might be the most appropriate choice for a most-similar analysis since that country has a parliamentary executive but shares many other political, cultural and social features in common with the US.¹¹

Note that if an offhand survey of available cases is insufficient to identify a most-similar case, either because the number of potential candidates is large and/or because the similarities and differences among them are not well-known, the researcher may resort to truth-tables (with comprehensive listings of attributes) or matching techniques (Seawright and Gerring, forthcoming) as a way of identifying appropriate cases.

Thus far, I have discussed the spatial components of most-similar comparison, where variation across cases is essentially static (there is no change, or at least no change of trend, in the variables of interest across the chosen cases). This conforms to a spatial comparison, as introduced earlier. Wherever comparative cases embody longitudinal variation they offer an additional dimension for causal analysis. Thus, the best choice for most-similar analysis is a pairing that provides a dynamic comparison, replicating the virtues of a classic experiment (but without a manipulated

intervention). Here, one can compare the outcome of interest before and after an intervention to see what effect it may have had in that case, a sort of pre- and post-test. Unfortunately, for purposes of exploring the role of separate powers in US welfare state development there are no obvious comparison cases of this nature. That is, no countries that are reasonably well-matched with the US have instituted constitutional changes in their executive (e.g. from presidential to parliamentary, or vice versa).¹² Nor, for that matter, has the US. As a rule, cross-case dynamic comparisons are more difficult to identify than spatial comparisons.

Within-Case Analysis

Regardless of how informative cross-case evidence (either large-*N* or small-*N*) might be, one is unlikely to be satisfied that one has satisfactorily explained an outcome until one has explored *within-case* evidence. If there is a specific hypothesis that organizes the research, research designs may be dynamic, spatial, longitudinal, or counterfactual. Let us return to the previous hypothesis, the structure of the executive in conditioning a weak welfare state in the US, to see how these research designs might play out in a given case.

Since states within the Union also pursue welfare policies, and their causal relationships may be similar to those that obtain at a national level, one might exploit variation within and across states to illuminate causal factors operative at national levels, where our ultimate theoretical interest lies. Suppose that a state decides to abolish its executive office (the governor), creating what is, effectively, a parliamentary system at the state level. Here is a terrific opportunity for a dynamic comparison. That state's welfare levels can be measured before and after the intervention and it may also be compared to another state(s) that did not undergo constitutional change.

Suppose that at least one state in the Union has always possessed a parliamentary system of government (from 1776 to the present). Under this circumstance, there is no intervention that can be studied, and no pre- and post-test may be administered. Still, one might compare levels of spending in that state with other similar states that have separate-powers constitutions in an attempt to judge the effects of constitutional structure on social policy and political development. This constitutes a spatial comparison.

A longitudinal comparison might be established at the national level. The American welfare state has been growing for some time and whatever causal dynamics are at work today have presumably been operative for some time. Granted, there has never been a parliamentary executive in the US, so there is no change on the variable of theoretical interest. However, there have been changes in the relative strength of the president and Congress, and this may offer some leverage on the question.

There are also periods in which both branches are controlled by the same party, and periods of divided party control. These may be compared according to the level of new legislation that they produce (Mayhew, 1991). This provides a picture of what the US welfare state might look like today if *all* periods of American history had been periods of single-party rule. To be sure, it may be doubted whether temporary periods of unified party control approximate the political circumstances of parliamentary systems; this is, at best, a poor substitute for a change in constitutional status, the actual variable of interest.

Evidently, with this particular research question, the opportunities for within-case empirical analysis are quite limited. As a consequence, the writer who wishes to apply a 'parliamentary' explanation to the American welfare state is likely to lean heavily upon counterfactual comparison. What course would the American state have taken if the US possessed a parliamentary system? This is a difficult matter to reconstruct. However, intelligent speculation on this point may be highly informative (e.g. Sundquist, 1992).

The employment of counterfactual reasoning in the analysis of individual outcomes is well established. Yet, it also raises dicey questions of causal logic, for, in principle, virtually any event lying prior to the outcome and having some plausible causal connection to it might be invoked as a necessary antecedent cause. The laggard American welfare state might be attributed to the American Revolution, the Civil War, early democratization, weak and porous (and generally corrupt) bureaucracies, the failure of the Knights of Labor in the 1880s, Progressive-era reforms, the First World War, the 1920s, the New Deal, the compromise between representatives of capital and labor after the Second World War, the Cold War and so forth. Each of these prior developments has been considered critical to the subsequent development (i.e. non-development) of the American welfare state. And all of these arguments are more or less plausible.

This is not an atypical situation, for most outcomes can be traced back in time to a wide variety of prior 'turning points'. The causal regress is, in principle, infinite, as is the number of possible counterfactual scenarios. (What if Gompers had failed to maintain control over the AFL? What if businesses had not been so hostile to the organization of labor unions in the interwar period?) Recall that in a *generalizing* case study, one's consideration of causal factors is limited to those that might plausibly explain variation across a broader population of cases. Yet, no such restriction applies to single-outcome studies. As such, this genre of endeavor is virtually intractable for the outcome is radically overdetermined. There are too many possible, and probable, causes. The options are, quite literally, infinite.

Mitigating this problem is a special restriction that applies to the counterfactual analysis of individual outcomes. Philosophers and social scientists have come to agree that the most sensible counterfactual within a field of possible counterfactuals is that which demands the smallest alteration in the course of actual events, as they really did happen. This principle of causal reconstruction has come to be known as the *minimal-rewrite* rule.¹³ The author should play God with history with as light a hand as possible. All other things being equal, when deciding between two explanations of a given event the researcher should choose the cause that is most contingent. It is the turning point, the critical juncture, which rightly deserves the label 'cause', not the factors that probably could not have been otherwise. The effect of this criterion is to eliminate rather absurd conjectures about the course of history, e.g. the American welfare state would have developed differently if the Europeans had never discovered America. While perhaps true, this counterfactual is not a very useful reconstruction of history because it envisions a scenario that departs radically from the actual course of events. Of course, the minimal-rewrite rule will not discriminate among all the hypotheses that might be generated through the counterfactual analysis of a single outcome. But, properly applied, it will narrow the field.¹⁴

Putting Cross-Case and Within-Case Evidence Together

Having reviewed three fundamental methods of single-outcome analysis, namely, nested analysis, most-similar analysis and within-case analysis, the easy conclusion is that all three of these methods ought to be employed, wherever possible. We gain leverage on a causal question by framing the research design in different ways and evaluating the evidence drawn from those separate and independent analyses. To the extent that a particular explanation of an outcome is confirmed by nested analysis, most-similar analysis and within-case analysis, one has successfully triangulated.

However, it is not always possible to employ all three methods. Or, to put it more delicately, these three methods are not always equally viable. Even when possible, and viable, sometimes the conclusions reached by these three methods are not consonant with one another. For example, cross-case evidence may suggest one causal factor and within-case analysis another. The three methods reviewed here might even suggest three different causal factors.

This sort of dissonance is mildly problematic in the generalizing case study, where the purpose of the investigation is to shed light on cross-case causal relationships. Here, one can reasonably dismiss idiosyncratic findings drawn from a single case as 'noise', stochastic variation or variation that is unexplained. However, in single-outcome studies the purpose of the study is to explain *that particular case*. Here, varying results from

cross-case and within-case analyses cannot be treated lightly. And here, because the objective is to provide a reasonably complete explanation, it is not permissible to dismiss evidence as part of the error term (noise). (An error term pertains exclusively to phenomena that have a distribution, and a single outcome does not have a distribution.)

Even more common than contradictory sets of evidence is the situation in which certain hypotheses garnered from the within-case analysis are untested, and perhaps untestable, in a cross-case setting. Consider the following arguments that the research team of Acemoglu et al. (2003: 113) provide to explain Botswana's good policies and institutions and, from thence, its extraordinary economic success in the post-independence era:

1. Botswana possessed precolonial tribal institutions that encouraged broad-based participation and placed constraints on political elites.
2. British colonization had a limited effect on these precolonial institutions because of the peripheral nature of Botswana to the British Empire.
3. Upon independence, the most important rural interests, chiefs and cattle owners, were politically powerful, and it was in their economic interest to enforce property rights.
4. The revenues from diamonds generated enough rents for the main political actors, increasing the opportunity cost of, and discouraging, further rent seeking.
5. Finally, the post-independence political leaders, in particular Seretse Khama and Quett Masire, made a number of sensible decisions.

All these factors may have contributed to explain why Botswana adopted good (market-augmenting) policies and institutions. But few are easily tested across a wide range of country cases. Does the existence of certain Tswana-like tribal institutions lead to broad-based political participation and constrained elites in other polities? Does 'light' imperial control lead to better post-independence politics? Is it advantageous for rural interests to dominate a country politically? Are diamond revenues a good thing? Each of these statements, if generalized to include a broad set of country cases, is plausible. But few are easy to test. The final argument, having to do with leadership, is true everywhere almost by definition (good leadership is usually understood as leadership that leads to good policy outcomes), and therefore does not tell us very much. To be sure, the authors present these five arguments as conjoint causes; perhaps all must be present for salubrious results to ensue. If so, then the argument is virtually incapable of broader application. This means that we must accept the authors' claims based largely on within-case evidence (plus a smattering of two-case comparisons that address different elements of the story).

It is quite common in single-outcome analysis to rest an inference or a set of inferences upon evidence drawn from that case alone, and there is

nothing in principle wrong with this style of argumentation. Nonetheless, Acemoglu et al. would be able to make a more convincing argument if they could show more cross-case evidence for their various propositions. In a few instances, statements made with reference to Botswana seem to fly in the face of other countries' historical experiences. For example, while the authors credit Botswana's success to its light-handed, non-interventionist colonial history, it seems likely that growth rates in countries around the world are positively correlated with the length and intensity of colonial control, particularly if the colonial power is British, as it was in Botswana (Grier, 1999; La Porta et al., 1998). It is possible, of course, that the effect of a rather crude variable like colonialism is not uniform across all countries. Indeed, there is no reason that we should accept uncritically the results of a cross-country regression that tells this particular story. But we have no cause to dismiss it either.

The point of this discussion is not to argue for or against either style of evidence but rather to point to a vexing methodological problem that affects virtually all single-outcome analyses. Cross-case and within-case evidence often tell somewhat different stories and there is no easy way to adjudicate between them. About all that one can say is that the strength of each sort of evidence rests upon the particulars of the evidence. Thus, if the cross-case analysis is sketchy, if, for example, the author is suspicious of the heterogeneity of cases in the sample, the operationalization of key variables, or the specification of the model, then she may choose to place less emphasis on these results. Likewise, if the within-case evidence is sketchy, if, for example, the case might be reconstructed in a variety of different ways, each of which provides a plausible fit for the theory and the evidence, then she may choose to place less emphasis on these results. In short, it all depends.

Conclusions

At this juncture, the reader may have come to the conclusion that single-outcome analysis is singularly difficult and case study analysis correspondingly easy. This is not the message I wish to convey. Indeed, I indicated at the outset that single-outcome arguments are often more conclusive than the corresponding cross-case arguments (for which the case study might be employed as a mode of analysis). A murder may be easier to solve than general problems related to criminal activity.

However, the sort of single-outcome studies that social scientists focus on explaining are also typically the sort that are difficult to parse. And this, in turn, rests upon their singularity. It is the unusualness of the outcome, not the method applied to the single-outcome study, that makes these studies so recalcitrant. The American welfare state will never have

a conclusive explanation. The US is too different from other nations, and there are too few other nations, to allow for this degree of certainty. Likewise for the First World War and the French Revolution. The more unique an outcome, the more difficult it is to explain because we have fewer comparison cases, and those few cases that present themselves suffer problems of causal comparability.

With crimes it is different. This is why judges and juries charged with rendering verdicts generally achieve a higher level of certainty and confidence than the sociologists charged with explaining crime (in general). But academics do not write case studies of individual crimes, unless, of course, those cases are sufficiently unusual to warrant individual treatment (e.g. crimes with immense political repercussions such as the Watergate burglary).

It will be seen that the initial choice of terms at the outset of this article, nomothetic and idiographic, correctly describe much of the work that falls into the two genres of case study and single-outcome study research. What I have shown, however, is the nature of the difficulties that these two genres of investigation stumble against. The single-outcome study is problematic not by reason of any inherent methodological difficulty but rather by virtue of the situations in which it is typically deployed. There is less need for single-outcome studies of typical outcomes. Consequently, single-outcome studies tend to be *singular*-outcome studies. In sum, it is the choice of topic, not the method, that renders this genre methodologically intractable. Unusual subjects will always be difficult to reach firm conclusions about.

Notes

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1. Burawoy (1998), Campbell (1988), Eckstein (1975), Feagin et al. (1991), George and Bennett (2005), Lijphart (1975), Platt (1992), Ragin and Becker (1992), van Evera (1997) and Yin (1994). See also the symposium in *Comparative Social Research* 16 (1997) and the annotated bibliography of works, primarily in sociology, in Dufour and Fortin (1992).
2. Contrast McAdam (1988) with McAdam et al. (2001). On this general point, see Davidson (1963).
3. Occasionally, where there is a very small population, the researcher may be able to study every case in the population intensively. In this rare circumstance there is no inferential leap from sample to population.
4. For discussion of the methodological issues arising in the attempt to explain a single event, see Goertz and Levy (forthcoming).

5. The book focuses at the high end of this scale; the entire scale is reproduced here to illustrate how sensitive indicators may be developed in single-event contexts.
6. Granted, some single-event analyses have, as their primary objective, a search for distinctiveness. Thus, the researcher's question might be 'What is different about X (Denmark, the US et al.)?'. However, this is an essentially descriptive question, while our concern is with causal inference.
7. Note that the task of case selection in single-event analysis is already partially accomplished: one has identified at least one of the cases that will be subjected to intensive study. It should also be noted that case analysis may be assisted by formal models (e.g. Bates et al., 1998) or statistical models (e.g. Houser and Freeman, 2001). Here, I am concerned only with evidentiary basis (the sorts of evidence that might be mustered) for such an analysis.
8. See Lieberman (2005). Coppedge (2002) employs the term 'nested induction', but the gist of the method is quite similar.
9. Details on these coding procedures are explained in Gerring et al. (2005) and Gerring and Thacker (forthcoming).
10. Sometimes, this method is known as the 'method of difference', after its inventor (Mill, 1872). For later treatments see Cohen and Nagel (1934), Eggan (1954), Gerring (2001: Ch. 9), Lijphart (1971, 1975), Meckstroth (1975), Przeworski and Teune (1970) and Skocpol and Somers (1980).
11. The US/Canada comparison is a fairly common one, though not all scholars reach the same conclusions (e.g. Epstein, 1964; Lipset, 1990).
12. France adopted a semi-presidential system in 1958. However, the primary locus of legislative sovereignty still resides in parliament, so it is not a good test of the theory.
13. See Bunzl (2004), Cowley (2001), Einhorn and Hogarth (1986), Elster (1978), Fearon (1991), Hart and Honore (1959: 32–3; 1966: 225), Hawthorn (1991), Holland (1986), Mackie (1993: 39), Marini and Singer (1988: 353), Taylor (1970: 53), Tetlock and Belkin (1996: 23–5) and Weber (1949). Also known as 'cotenable' (Goodman, 1947) and 'compossibility' (Elster, 1978).
14. Note that the minimal-rewrite rule has the additional effect of nudging single-event analysis away from general, structural causes and toward unique, proximate causes, which are (almost by definition) more contingent. Compare two classic explanations of American exceptionalism, (1) the great frontier (see Turner, 1972) and (2) the failure of the Knights of Labor in the 1880s (Voss, 1993). Evidently, the second event is more likely to have turned out differently than the first.

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Biographical Note: John Gerring is currently Associate Professor of Political Science at Boston University, where he teaches courses on methodology and comparative politics. His books include *Party Ideologies in America, 1828–1996* (Cambridge University Press, 1998), *Social Science Methodology: A Criterial Framework* (Cambridge University Press, 2001), *Case Study Research: Principles and Practices* (Cambridge University Press, 2006), *Global Justice: A Prioritarian Manifesto* (under review), *Good Government: A Centripetal Theory of Democratic Governance* (with Strom Thacker; forthcoming), *Concepts and Method: Giovanni Sartori and His Legacy* (with David Collier; under review), and *Democracy and Development: A Global Synthesis* (with Strom Thacker; in process). His articles have appeared in *American Political Science Review*, *British Journal of Political Science*, *Comparative Political Studies*, *International Organization*, *Journal of Policy History*, *Journal of Theoretical Politics*, *Party Politics*, *Political Research Quarterly*, *Polity*, *PS: Political Science and Politics*, *Social Science History*, *Studies in American Political Development* and *World Politics*. He was a fellow of the School of Social Science at the Institute for Advanced Study (2002–3). He is currently editor of *Qualitative Methods: Newsletter of the American Political Science Association Organized Section on Qualitative Methods* and he is currently president-elect of the *Qualitative Methods* organized section of the American Political Science Association.

Address: Boston University Department of Political Science, 232 Bay State Road
Boston, MA 02215, USA. [email: jgerring@bu.edu]