Can One or a Few Cases Yield Theoretical Gains?

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1. The Issue

The crux of skepticism about comparative historical analysis is the "small-
N problem" – the combination of many factors assumed to be causally re-
levant with evidence from only a small number of comparable cases. Ex-
ploring the impact of a large number of relevant factors and conditions
in only a few cases seems to run into insuperable obstacles for learning
anything that is theoretically relevant. In this essay, I will turn a skeptical
eye on these skeptical objections. I go deliberately to the extreme and
ask what can be learned theoretically from the study of a single histori-
case and from comparative analyses of two or very few more cases, a
kind of research that permits close attention to the complexities of historical
developments.

I begin with two opposite positions that I consider problematic and
in their starkest form mistaken. One of these is the most conventional
view, taught in countless classes on the methodology of social research.
It holds that studying a single case yields only one reasonable theoretical
outcome, the generation of hypotheses that may be tested in other, more
numerous cases. And conventional methodological wisdom holds the same

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Study in the Social Sciences (SCASSS) was helpful for giving this essay its final shape, and I am
grateful to Björn Wittrock, one of the directors of SCASSS, for his comments and suggestions.
I dedicate this essay to three historians from whom I learned much over the years: Anthony
Molho, Jürgen Kocka, and Hans-Ulrich Wehler.
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i will argue in this essay that while any explanation requires theoretical premises, the study of single historical cases can do much more than merely generate initial hypotheses. it not only can develop new theoretical ideas, but it can also put them to the test and use the results in the explanation of outcomes. moving beyond the first case yields often particularly powerful new insights. at the same time, cross-case variation presents difficult methodological problems for macrosocial analysis, both quantitative and qualitative. in response to these difficulties, i argue that testable and tested explanatory propositions are not the only gains we can derive from the analysis of a limited number of cases. more modest results still constitute worthwhile cognitive advances.

ii. some first considerations

a prima facie case against the conventional view that little can be learned from single cases is easily made. quite a few single-case studies rank among the most powerful and influential works in social and political analysis. a first example is e. p. thompson's own classic work the making of the english working class (1963). in offering its rich and thoroughly argued account, the book refuted certain marxist accounts of class formation, call them vulgar or naive, and it did so in the historical case that was central to thinking in the marxian theoretical tradition. as a consequence, it changed the way many scholars thought about class formation. theory challenge led to new theoretical formulations that focused on variable experiences of deprivation and conflict in similar structural locations and on memories of the past that shaped these experiences as well as the actions that arose from them. the theoretical orientation of class analysis — what was considered the most reasonable and fruitful approach — turned in a less objectivist direction. thompson's historical account strengthened the view that class formation is a process that cannot be read off from objective conditions, not even in the long run; it is, rather, subject to social construction and cultural circumstance.

another example is robert michels's political parties: a sociological study of the oligarchical tendencies of modern democracy, one of the big success
stories of sociological research. This work took off from the encounter of Michels, a young academic and the son of a Catholic millionaire from Cologne, with the German Social Democratic Party (SPD) before the First World War, a party that represented milieus and interests radically set apart from the bourgeois world of his family and of higher education. True, Michels referred to other European parties as well, focusing on socialist parties as the most prodemocratic, and he developed his ideas within a theoretical frame that was indebted to Max Weber's political and organizational sociology; but the core empirical referent of his analyses was the SPD. Yet the results of this research go far beyond a complex account of structures and processes in the SPD. Michels’s central claim, the “iron law of oligarchy,” is stated as a universal proposition:

It is organization which gives birth to the domination of the elected over the electors, of the mandataries over the mandators, of the delegates over the delegators. Who says organization says oligarchy.

This central proposition is supplemented with an explication of the mechanisms that seem to underlie it. Political Parties, then, develops and makes plausible a theoretical account of oligarchic tendencies in politically relevant associations. Published before the First World War, it proved to be an accurate prediction of countless processes in political parties and voluntary organizations, it cast doubt on the realism of democratic aspirations, it foreshadowed a critique of the Russian Revolution of 1917, and it anticipated Milovan Djilas’s claims in *The New Class: An Analysis of the Communist System* (Djilas 1957).

My third exhibit is a book that responded to Michels’s thesis with the analysis of a deviant case. Seymour Martin Lipset, Martin Trow, and James Coleman, having seen Michels’s proposition confirmed in many cases, chose to examine a union in which democratic processes led repeatedly to leadership turnover. The title of their book, another of the great empirical classics of sociology, is worth quoting in full: *Union Democracy: The Internal Politics of the International Typographical Union. What Makes Democracy Work in Labor Unions and Other Organizations?* (Lipset, Trow, and Coleman 1956).

Against the background of the breakdown of democracy in Europe, the rise and fall of Nazism, and the turn of the Russian Revolution into Stalinism, *Union Democracy* used theoretical arguments that derived from analyses of the breakdown and stability of democracy in national regimes to explore the limits of Michels’s iron law about oligarchy in parties and voluntary associations. The new propositions included prominently claims about the critical role of autonomous intermediary groups, claims that have in the last ten to fifteen years found renewed attention under the heading of “civil society.” *Union Democracy* challenged an established theory and formulated a new theoretical account that could explain both conforming and deviant cases. It thus embraced as well as transcended Michels’s theory, and the detailed analysis of the International Typographical Union (ITU) gave it considerable credibility.

Yet, while even a brief reminder of books such as *The Making of the English Working Class*, *Political Parties*, and *Union Democracy* casts doubt on the proposition that little can be learned theoretically from single-case studies, the conventional view has strong arguments on its side as well and cannot be dismissed by reference to a few extraordinarily successful case studies.

Any explanation, the argument runs, requires propositions with a broader range of application than the phenomena to be explained. Ideally, these propositions should hold – under specified conditions, of course – universally. To be persuasive, the explanatory propositions must be subject to some discipline if one of the many possible explanations is to be chosen over others – that is, if one really cares about one’s explanation. Arthur Stinchcombe has suggested that graduate students who cannot think of three explanations when confronted with a correlation that interests them should choose another career (Stinchcombe 1968, p. 13); and one might invoke a similar belief in the cornucopia of reason when dealing with the explanation of historical sequences. Explanatory propositions, then, have to be explicitly developed and tested before they can claim the credibility that warrants their use rather than that of competing accounts.

The conventional view distinguishes sharply between the “context of discovery” and the “context of validation” of explanatory propositions. Discovering an idea is seen as a methodologically irrelevant psychological process...
that may indeed involve one instance of the phenomena to be explained or may be only as tangentially related to the issues at hand as the falling apple that supposedly inspired Isaac Newton in his work on gravity. Even if the explanatory idea is developed in work on one instance of the explanandum, it has to be put to the test in different cases. Discussing the problems of “testing the theory on the body of data that suggested it,” one formulation of the conventional view held: “These problems are as real and as defeating for the qualitative essays of the knowledgeable political scientist or historian as for the multivariate statistician.... the scholar has an unexplicit but very large number of potential ‘considerations’ which he has, or could have, brought to bear on one instance or another. This, rather than its pattern-recognition characteristics is the crux of the problems of the Verstehen approach – one source of its exquisitely satisfactory fit to particular instances, and its unsatisfactoriness as a reality-testing process” (Raser, Campbell, and Chadwick 1970, pp. 186–7, parentheses omitted).

These arguments make a strong case against taking E. P. Thompson’s formulation of “history as a process as inscribed with its own causation” literally. Yet I will show that single cases can indeed do more than inspire new hypotheses and insights. They can serve the purpose of theory testing as well. And even the explanatory use of theory in the same case in which it was developed is not as unreasonable as it first seems.

III. Theory Development, Theory Testing, and Explanatory Use of Theory in a Single Case

Clearly, a single case can force the rejection of a hypothesis or its modification, provided that the proposition in question was not formulated in probabilistic terms. This is not as rare in social analysis as one might think. Union Democracy broke the iron cast of Michels’s “Who says organization says oligarchy.” And a theory of class formation that insists on predicting the collective organization of workers as a class, as well as the goals of their actions as a class from the class members’ “objective” relation to the means of production in conjunction with a few further conditions such as urbanization, factory work, and repeated conflicts over pay and work conditions, was rendered untenable by the far more complex, contingent, and constructivist account Thompson offered in The Making of the English Working Class.

That single cases can – aside from inspiring theoretical ideas – falsify nonprobabilistic propositions will be readily granted by any neopositivist methodologist. But can a single case history have theoretical implications that go beyond that? This rhetorical question overlooks, first of all, that such falsification may be more complex than the rejection of a single proposition. A long sequence of historical development offers, provided that it is approached with sufficiently specified theoretical expectations, a large number of theoretically relevant observations that may rule out or suggest the revision of a whole series of propositions. Any demonstration of path dependence, dealing for instance with the persistence of a structure of domination that remains stable under conditions that differ substantially from the conditions of its origin, can invalidate – or force the modification of – quite a few claims about the conditions of stable domination. Many hypotheses about the stability of rule are, after all, formulated in a “presentist” way that disregards the interaction of changing current conditions with factors that account for a path-dependent character of the phenomenon.

Aside from falsification, there is also a positive contribution of single cases to theory that many will accept. This is based on the theoretical implications of “least likely” cases, least likely in terms of a widely accepted theory or an implicit theoretical expectation. Such cases, for which Michels’s demonstration of oligarchic tendencies in social democratic organizations (i.e., in the very prime promoters of democracy) is a good example, will greatly increase the plausibility of the alternative theoretical understanding they suggest.\footnote{The powerful theoretical implications of deviant and least likely cases have long been explicitly recognized (see Eckstein 1975; Lijphart 1971, 1975; Smelser 1976); for a sophisticated recent discussion of what she calls “negative case methodology” see Emigh (1997).}

That hypotheses, which are developed (or refined) in one case, can be tested and used as explanations in the same case is more counterintuitive. However, good historical explanations, and especially analytically oriented historical studies, do precisely this. Thompson clearly refers inter alia to his own analysis of class formation between 1790 and 1830 in England, when he talks about the dialogue, in which historical practice is characteristically engaged:

with an argument between received, inadequate, or ideologically-informed concepts or hypotheses on the one hand, and fresh or inconvenient evidence on the other, with the elaboration of new hypotheses; with the testing of these hypotheses against the evidence, which may involve interrogating existing evidence in new ways, or renewed research to confirm or disprove the new notions, with discarding those hypotheses which fail new tests, or refining or revising those which do, in the light of this engagement. (Thompson 1978, p. 43)
This is precisely what fills the pages of The Making of the English Working Class. And the same can be said about Michels’s examination of the implications and corollaries of his main claim in Political Parties. Part of this multiple creation, testing, revising, and retesting of hypotheses may go unrecorded, but it is this complex process that lies behind the assurance of the authors’ claims and the confidence their arguments inspire in the reader. It is above all this dialogue between theory and evidence that constitutes the comparative advantage of comparative historical analysis.

One source of this ever-renewed questioning and testing of hypotheses may be surprising: intense political interest. Given that the standard advice for social and historical analysts urges neutral objectivity, this may be worth a moment’s reflection. It is almost certainly true that the authors of all three books I chose here as exemplars were continuously involved in passionate political discussions, with endless iterations of confronting claims with evidence and counterevidence. Political interest can, of course, encourage wishful thinking and lead to blind spots in perceiving social reality. Yet political interest can also create a very strong urge to know, to understand the conditions under which one’s aims can be realized, what the major obstacles are, which goals must be considered utopian, what compromises will likely impose themselves, what the consequences of the routinization of passionate commitment will be, and so on. The powerful theoretical results of these three works seem testimony to the realism of this second possibility.

Would it be worthwhile to make the complex dialogue between theoretical imagination and empirical evidence that characterizes so much of comparative historical work explicit? From a methodological point of view, the answer must be an unqualified yes. But insisting on full explication is unlikely to meet with resounding success, because it is at odds with the way many, if not most, historical analysts work. For many, the “context of discovery” is intellectually more closely interrelated with the “context of validation” than the textbooks assume. Many also share Max Weber’s distaste for methodological reflection and a bookkeeping of the research process, feeling that it would hamper their work, detract from substantive concerns, and cramp imaginative intuition as well as interesting presentation. Yet, it turns out that authors such as E. P. Thompson — much like Max Weber — are quite capable of methodological argument when controversy requires it, and that in the face of contention about their findings, much of the iterative analytic interrogation of the evidence can be recovered.

Thompson — other than Michels or Lipset, Trow, and Coleman — was only in a limited way interested in making theoretical claims beyond an elucidation and explanation of the particular historical developments he studied. True, he sought to reject an "economistic" theory of class formation; but that aside, his theory-evidence dialogue remained confined to component developments in the rise of working-class consciousness in four decades of English history, seeking to establish, for instance, that and how earlier cultural patterns (and not only new employment relations or the distribution of economic resources) shaped the experience and the meaning of exploitation in different groups of workers.

The distinction between analytically oriented history, which focuses on the explanation of particular developments, and a history-conscious social science primarily interested in the propositions usable in various historical explanations marks important differences in intellectual style; but one must not overestimate its significance. The theoretical core of both kinds of work consists in the development and validation of explanatory causal hypotheses. Yet in order to bridge the gap that separates the self-understanding of many historians from that of other social scientists, it may be worthwhile to focus a little longer on the more implicit analytic work of the historian than on the self-conscious theorizing of historical social scientists.

Thompson’s focus on a bundle of historical explanations, which together account for the emergence of a coherent working class with a particular consciousness and organization, has been understood as a general hostility toward theory. And since he was very much concerned with people’s experience of changing objective conditions and their understanding of that experience, his work has been construed as interpretive rather than causal analysis. 6 Neither reading takes sufficient notice of Thompson’s abiding interest in developing, testing, and using causal and structural hypotheses. A footnote to the text quoted earlier explains:

By “concepts” (or notions) I mean general categories — of class, ideology, the nation-state, feudalism, &c., or specific historical forms and sequences, as crisis of subsistence, familial development cycle, &c. — and by “hypotheses” I mean the conceptual organization of the evidence to explain particular episodes of causation and relationship. (Thompson 1978, p. 43)

His choice of words makes it clear that he wishes to stay close to the actual process of historical analysis and keep his distance from the formal prescriptions of social science methodology; but there is no ambiguity about his interest in explanatory propositions.

6 See Trimberger (1984, pp. 225–30), who reports on the first critique and argues the second.
Even if confined to microexplanations within the overall historical account of the “making of the English working class,” these propositions are different from sheer narrative, and they are so distinguished by Thompson. Here is how he describes the construction of narrative by historians:

The discrete facts may be interrogated ... as links in a linear series of occurrences, or contingent events - that is, history “as it actually happened” (but as it can never be fully known) - in the construction of a narrative account; such a reconstruction (however much it may be despised by philosophers, by sociologists, and by an increasing number of contemporary historians who have been frightened by the first two) being an essential constituent of the historical discipline, a pre-requisite and premise of all historical knowledge, the ground of any objective (as distinct from theoretic) notion of causation, and the indispensable preliminary to the construction of an analytic or structured account (which identifies structural and causal relations), even though in the course of such an analysis the primitive sequential narration will itself undergo radical transformation.7

Yet Thompson remains skeptical about full-scale causal accounts: “History is not rule governed, and it knows no sufficient causes.” It can learn “how things turned out ... not why they had to turn out that way” (Thompson 1978, p. 49). This reservation about sufficient causes may at first surprise. Yet it seems often - though not always - reasonable, and this on two grounds. First, it corresponds to the sense of historical actors that the future is open. To recover this sense about historical processes, even though we know the outcome, is critical for the historical imagination. Whether such a sense of openness is actually realistic depends, of course, on the processes in question. Some outcomes may be quite determined - reasonable

7 Thompson (1978, p. 29). Looking at the same issue with a primary interest in explanatory theory, Arthur Stinchcombe comes to a very similar view: "All the books I choose to analyze are narratives of a sequence of events, and this choice is central to my argument. I believe that the test of any theory of social change is its ability to analyze such narrative sequences, and that the poverty of the theory of social change is due to paying no attention to that narrative detail... for the purpose of advancing causal understanding, the unique sequence... has to be broken up into theoretically understandable bits. When those bits get back into the narrative, having been theoretically interpreted, the narrative will also be improved by being grounded in general ideas" (Stinchcombe 1978, p. 15). John Stephens and I discussed the same point more recently: "Causation is a matter of sequence. One needs diachronic evidence, evidence about historical sequences, to explore and test ideas about causation directly. This remains true even if it is also true that simple historical narrative is not the same as causal explanation. Post hoc does not any more translate directly into propter hoc than correlation demonstrates causation. Explanation can indeed proceed without analytic hypotheses; but causal explanation ordinarily needs hypotheses about sequence which can best be tested against evidence about sequences" (Rueschemeyer and Stephens 1997, pp. 55-72).

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examples are the reproduction of institutions such as marriage or private property in many known societies - while others, such as the outcomes of wars or such achievements “against the odds” as large-scale working-class organization or the unification of independent political units, are or were indeed open. Close historical examination can often yield some reasonable estimate on these alternatives. It appears, however, that many outcomes are not just determined but overdetermined. The continuation of private property rights over large sections of productive capital in most Western political economies and the persistence of many lines of ethnic division are probably good instances of such overdetermination. So many phenomena seem, in fact, to fall into this category that a functionalist might be tempted to draw the mistaken conclusion that all really important outcomes tend to be overdetermined. Ironically, this phenomenon of overdetermination offers another reason for reserve about claims of sufficient causes: if more than one condition is sufficient to account for an outcome, it becomes clearly more difficult (though not impossible in principle) to identify different singly sufficient causes.

Whether the primary interest is in historical explanation or in the generation of theoretical propositions, detailed case analyses often entail the generation, testing, revising, and restating of explanatory propositions within the same complex material. The discipline that in such endeavors is imposed on willful interpretation and speculation derives from the often large number of theoretically relevant observations and from the fact that, for each of these, analytic intent and empirical evidence can be fairly closely matched, more closely than is possible in many studies with large Ns. This is also the gist of Donald Campbell's "revisionist" article on the value of anthropological case studies:

In a case study done by an alert social scientist who has thorough local acquaintance, the theory he uses to explain the focal difference also generates predictions or expectations on dozens of other aspects of the culture, and he does not retain the theory unless most of these are confirmed. In some sense, he has tested the theory with degrees of freedom coming from the multiple implications of any one theory. The process is a kind of pattern-matching ... in which there are many aspects of the patterns demanded by theory that are available for matching with his observations on the local setting.

Campbell likens this mode of analysis to knowledge construction in everyday life, and he concludes his article with an observation that hints at the evolutionary basis of human cognitive efficiency: “after all, man is, in his
ordinary way, a very competent knower, and qualitative common sense is not replaced by quantitative knowing.  

To claim that anthropological interpretations of cultural patterns and the explanations of historical processes involve a complex interrelation of theory development, theory testing, and the explanatory use of theoretical propositions runs counter to a standard maxim of conventional methodology: disregard confirming observations that were known at the time the proposition was formulated. Some such “peaking” is unavoidable in historical sociology. It is after all less than likely that people who are truly ignorant of the history to be analyzed will do a worthwhile job. Small wonder that Arthur Stinchcombe opens his argument in *Theoretical Methods in Social History* with an invective “...against the fashion of discussing the relation of theory to facts, sired by Kant, foaled by the Vienna School, and raced past us in our statistics textbooks.... the whole Kantian idea contradicts our everyday experience of research. We do not form a historical interpretation before finding out what was going on. Only respect for philosophical appearances could generate such an outcome” (Stinchcombe 1978, p. 4). It is also common knowledge that the maxim is routinely violated in quantitative research as well. Yet unavoidable and common in normal research experience or not, does peaking make confirmation too easy? That may be a danger in relatively simple studies. In much historical work of quality, however, an explanatory or structural proposition has, together with its various corollaries and implications, to meet a very large number of diverse “data points,” so that it is often not easy to settle on any one account, no matter how much of the evidence was—in some sense—known in advance. Arthur Stinchcombe may be right that any intelligent person can respond to isolated correlations (as well as simple sequences of events) with three, four, or five accounts, but that view does not apply to more complex patterns of evidence.  

8 Campbell (1975, pp. 181-2, 191). Substantively, Campbell’s article is somewhat awkward testimony for the case I am making. His central example is E. H. Eriksen’s interpretation of Yurok culture and personality in terms of psychoanalytic notions of fixation, a mode of analysis that is perhaps less plausible today than it was in 1943, when the study was published, or in 1975, when Campbell referred to it (“... its initial implausibility is made worse in the following oversimplification: rather than an oral or anal fixation, the Yurok were fixated on the whole alimentary canal” [p. 184]). Yet surely the rejection of this example would not invalidate the revised methodological argument. 

9 Campbell notes: “Even in a single qualitative case study the conscientious social scientist often finds no explanation that seems satisfactory” (1975, p. 182). In our discussion with John Goldthorpe, John Stephens and I took a certain pleasure in pointing out that Goldthorpe himself was unable to offer an adequate theoretical account for his very impressive comparative quantitative findings about social mobility (Rueschemeyer and Stephens 1997, p. 69).  

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If it is true that historical research often involves theory development as well as theory testing and explanatory theory use, it is also true that the studies that have yielded the most analytic insight were informed by intensive advance theoretical reflection. The results of this reflection may have remained largely implicit or they may have been stated as an explicit theoretical framework of questions, concepts, orienting ideas, and central hypotheses.  

Such reflection not only shapes the questions and the premises of the case analysis, it also links them to earlier scholarship and thus to analytic work on other instances of the issues under investigation. It therefore increases—if indirectly—the number of cases on which conclusions are built. 

As in everyday life we can gain powerful insights from a few encounters because these are assessed against the experience of a lifetime, so the theoretical framework—when informed by previous thought and research—provides the background against which the picture of the cases studied yields more telling results. (Rueschemeyer et al. 1992, p. 38)  

The role of such a theoretical framework must be clarified a little further, and Arthur Stinchcombe offers a good foil. He opens his book on *Theoretical Methods in Social History* with the assertion that what is usually called “theory” is irrelevant for analytically oriented historical investigation: “When it comes down to analysis of specific cases, I would argue that when they do a good job of historical interpretation, Marx and Weber and Parsons and Trotsky and Smelser all operate the same way” (Stinchcombe 1978, p. 2). He rejects grand meta-theories as “dross” and looks for propositions that actually can explain historical processes. Does this mean that the theoretical frameworks I just referred to are in Stinchcombe’s judgment also just dross? The theoretical frameworks I have in mind are not empirical theories; they are not primarily ensembles of testable propositions, though they may contain some of those. To a large extent, they consist of problem formulations, conceptualizations, and reasons given for these. These reasons, which prepare causal analysis by pointing to factors likely to  

10 In Barrington Moore’s *The Social Origins of Dictatorship and Democracy* (1966), this remained largely implicit. By contrast, Theda Skocpol opened her *States and Social Revolutions* (1979) with a review of the literature and a formulation of her structuralist approach. In *Capitalist Development and Democracy*, Evelyne Huber Stephens, John Stephens, and I made the formulation of a theoretical framework an explicit part of our procedure that informed the country analyses, which of course allowed for additional hypotheses and which could—and did—lead to a partial reformulation of the framework (Rueschemeyer, Stephens, and Stephens 1992).
be relevant for different outcomes, often constitute what Thompson calls "theoretical expectations." In a formal sense, theoretical frameworks are largely meta-theoretical in character, as they contain few directly testable hypotheses; but – and this is critical – these frameworks are meta-theory that is problem-specific, dealing with class formation, organizational democracy, or the growth of welfare state policies. As focused meta-theory, theoretical frameworks are in their specificity very different from the grand theoretical schemes of, say, functionalist systems theory or historical and dialectical materialism. They are immediately preparatory to what Stinchcombe looks for, testable explanatory propositions, and I believe that they greatly advance the cause of such explanatory theory.

The result of these reflections about inquiries into single historical cases can be summed up simply. Such case studies can do more than generate theoretical ideas. They can test theoretical propositions as well, and they can offer persuasive causal explanations. Skepticism about this claim rests ultimately on the mistaken identification of a single case with a single observation. Good historical analysis that is analytically oriented goes through frequent iterations of confronting explanatory propositions with many data points. If this confrontation does not proceed with the quantitative use of standardized items but typically works in a qualitative way, examining many different implications of the explanatory propositions entertained, it nevertheless involves many such empirical checks. And it gains its credibility precisely from the fit between theoretical ideas and their complex implications, on the one hand, and the best empirical evidence, on the other. In this confrontation of theoretical claims with empirical evidence, analytical history enjoys two significant advantages compared to all but the most exceptional quantitative research: it permits a much more direct and frequently repeated interplay between theoretical development and data, and it allows for a closer matching of conceptual intent and empirical evidence.

Before I move to some consideration of the analytic gains to be made by going beyond a single case, one implication of the argument developed up to this point should be made explicit. A good deal of the extant quantitative research is confined to a single country or a single community. This research, though proceeding with different techniques of data collection and analysis, is as much confined to a single case as is good analytic historical work that deals with, say, the emergence of a partially organized working class in one country.

11 See footnote 3.
The upshot of these mere glances at our three initial examples is an answer to the questions that is simple in principle yet quite complex in actual use: What is a case? and Where are its boundaries? are questions whose answer depends on the theoretical problem posed. Even E. P. Thompson's work can be taken as a number of cases in which past cultural patterns shape the work and life experiences of different working-class groups, and these repeated causal accounts within the overall story give the analysis much of its credibility. Or it can be read as what it presents itself as: the account of one complex historical development. Whether we deal with one case or a complex of instances, then, is another issue to be decided by the theoretical framework that defines problems, proposes conceptualizations, and gives reasons for these decisions, whether that framework is only implied by the analysis or made explicit, and even whether the framing is offered by the original author or is later imposed by a secondary analyst.

Anybody who has ever engaged in macrocomparative research knows of the impressive gains that can be reaped when one moves across boundaries within which causally important but theoretically perhaps unrecognized factors are held constant. This is quite often the case when we move across societal, national, and/or cultural boundaries. One can conceptualize this move as a shift from within-system analysis to system-level analysis (cf. Przeworski and Teune 1970; Schriewer 1999, pp. 56–8), though I would prefer not to put it in the terms of system theory language. The critical point here is simple: only by going beyond the first case does the impact of factors on the outcomes of interest come into view that does not show up in within-case analyses because these factors are – completely or largely – held constant.

A recent example is found in Robert Putnam's attempts to explain his findings about a decline of civic engagement in the United States since the 1970s. Putnam first emphasized the increased labor force participation of women, while later giving greater weight to the impact of television. The second explanation was made plausible with a complex analysis of the development of television viewing and the generational aspects of the decline in social participation in the United States (Putnam 1995a, 1995b, 1998). Though not uncontested in the light of American evidence (e.g., Norris 1996), these explanations run into much more potent problems when viewed in the light of elementary cross-national comparison. Norway and Sweden have higher women's labor force participation rates than the United States, yet they have experienced no significant decline in social and political participation. Similar and equally strong objections arise from

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even a superficial cross-national comparison linking increasing television viewing to a decline in civic engagement.

At a minimum, these comparisons suggest further research questions about contextual factors that appear salient when Northwestern European patterns and developments are contrasted with those in the United States. Institutions and regulations supporting women's labor force participation are more developed in Scandinavia, and these may well support both work outside the house and civic engagement. More broadly, contrasts in the associations and parties appealing to the public as well as in the overall pattern of political opportunities may be as important for civic engagement as aggregate changes in individual needs and interests. In Scandinavia, unions and political parties are organizationally stronger and more involving than they have been in the post–New Deal United States, and a wide range of associations are more closely interrelated with public institutions and public resources than all their counterparts in the United States. It is quite possible that the major explanations for the differences in social and political participation are found at the mesolevel of associations and parties and at the macrolevel of national political opportunities rather than at the aggregate micro level of changed individual preoccupations (Rueschemeyer, Rueschemeyer, and Wittrock 1998).

Max Weber's work on law and the rise of capitalism provides another example of the striking effect of moving the analysis beyond a single case. Weber developed a pure type of "formally rational" law that guarantees a maximum of the calculability that he considered necessary for the sustained development of a capitalist order. In his formally rational formula, the formal character of a legal order refers to its differentiation from the substantive concerns of religion, morality, and political interest, while rationality means in Weber's sociology of law the universal and systematic character of legal rules. If the "ideal type" of formally rational law is in the first place a complex pure construct, to which the multiplicity of historical phenomena can be compared, it represents at the same time an embryonic theory, because it is joined to the claim that economic calculability is the greater the more closely a particular legal order approximates the pure type. This claim appeared to be substantiated by the character of continental legal systems, especially the German legal system, which became highly rationalized, even before unification, in the course of capitalist development. However, the case of England presented a challenge. The English legal order exhibited a much lower degree of both formalism and rationality. Yet England was the first country to develop vibrant capitalist growth.
Weber never came to a firm solution of this "English problem"; but he considered a series of alternative hypotheses, which demonstrate the powerful effect of moving comparatively beyond the macrocontext in which the original proposition was developed:

(1) The English legal system offered a low degree of calculability but assisted capitalism by denying justice to the lower classes. (2) England was unique in that it achieved capitalism "not because but rather in spite of its judicial system." The conditions allowing this, however, did not prevail anywhere else. (3) The English legal system, while far from the model of logically formal rationality, was sufficiently calculable to support capitalism since judges were favorable to capitalists and adhered to precedent....

[Another hypothesis was developed later:]... the English judiciary was to a significant degree independent of the state, so that autonomy in this sense remains part of the model. Because of this latter aspect of English legal life, some observers have argued that England did develop a truly "rational" legal system before the rise of capitalism, and that the major flaw of Weber's analysis was the false distinction he drew between English and continental law. (Trubek 1972, pp. 747-8, notes omitted)

Going beyond boundaries within which causally relevant factors may be held constant, then, brings substantial gain. Both Putnam's explanation of the decline of civic engagement in the United States and Weber's account of the relation between the character of the legal system and the chances of capitalist development can be substantially improved by examining intensively one more case that seems to challenge the first version. (In the interest of space, we have to leave the substantive questions raised by the two examples behind; such is the annoying disregard of methodological discussions for the real world.)

The contrast in the explanandum may sometimes be due to a major new factor not previously considered, as in one combination of Weber's alternative hypotheses about England: what made capitalist development possible was not predictable law, but the more direct favors of the judicial system to capitalist entrepreneurs. At other times, an empirically substantiated explanation of the different outcome in the second case may not so much reveal new factors as a different interaction among otherwise well-known factors that are equally visible in both cases; our conjecture about the differences in the impact of labor force participation of women on civic engagement between the United States and Scandinavia offers an -- as yet unsubstantiated -- example of this.

Of course, dual case studies very rarely can settle questions about the impact of factors that differ across cases. This would be possible only in the rare constellation where theoretical predictions come to definite stipulations that are contradicted by the second case -- the second-case equivalent of the falsification of nonprobabilistic claims by a single case. Considerable increases in the plausibility of theoretical claims can be gained if the second case is in light of theory and for specified reasons least likely to confirm it -- the second-case equivalent of Michels's choice of a social democratic party as least likely to be undemocratic.

Beyond this, increasing the number of cross-case comparisons without losing the advantage of close familiarity with the complexity of cases becomes imperative. Hypotheses about factors that differ across large-case boundaries -- that is, all hypotheses about macrosocial phenomena -- require testing against a larger, though not necessarily a very large number of cases. Increasing the number of macrocomparative cases raises complex questions of case selection and of defining the most appropriate total set of comparable cases. For both, full-scale hypotheses and the expectations embodied in focused theoretical frameworks are of critical importance, though extensions of the analysis beyond a single case often operate with a great deal of ignorance about the dynamics of the phenomenon under study outside the first context of analysis. In this essay, I will not go further into these matters of case selection and domain definition.

The issues of macrocomparative analysis are normally discussed in an order that I reversed in this essay. The usual starting points are cross-case comparisons and the problems of such comparisons raised by the small number of cases. I will not rehearse here again the considerable methodological refinements that have been achieved in dealing with these problems, since they are treated in the essay by Mahoney in this volume. But I do wish to emphasize that the perspective gained by focusing first on the theoretical gains achieved through within-case analysis puts the problems of cross-case analyses in a different light. As Mahoney (2000 and in this volume) makes clear, within-case analyses complement important ways different modes of cross-case exploration. In particular, a procedure he calls "causal narrative" directly combines within-case causal sequences with cross-case identification of likely causal factors.

13 For an excellent treatment see Collier and Mahoney (1996).
14 "A good example of the use of causal narrative to compare event structures is found in Skocpol's (1979) work on social revolutions. Many of Skocpol's key explanatory variables are actually made up of numerous causally linked processes. Likewise, the outcome of social revolutions is itself composed of a series of causally connected events. These constituent processes represent an event-structure pattern that could be formally diagrammed and compared across cases (see Mahoney 1999). Although Skocpol does not carry out a formal
A final point about comparative advantages that analytically oriented historical work on a small number of closely understood cases enjoys over quantitative cross-national studies employing multiple regression for its causal inferences should at least be adumbrated. Even though the procedures used in analytic historical research have remained until recently largely implicit, comparative historical analysis has shown itself better able to deal with certain problems that cause trouble for multiple regression analysis when it deals with macrocausal issues and therefore relatively small numbers of cases. Thus, comparative historical work that uses both within-case and cross-case analysis can explore more complex interactions among causal factors, it can better trace multiple paths of causation, and it does not make the assumption of a linear relation between independent and dependent variables that—in the absence of historical information suggesting other relations between causal factors and outcomes—multiple regression analyses often adopt. 15

V. And Yet There Are Limits to the Theoretical Results of Single and Small-N Case Studies

It is one thing to dispel myths about single-case and comparative historical analysis; it is quite another to claim that qualitative macroanalyses encounter few or no serious methodological problems. Despite considerable advances in making explicit what shrewd and careful comparative historical scholars have practiced for a long time, there is little reason for a triumphalist assertion of the value of small-N/close-acquaintance studies.

Comparative historical analysis does not and cannot follow a radically different logic from other investigations of reality, be it in the social or the natural sciences. The two major recent publications on the methodology of qualitative research rightly insist on this point: both The Comparative Method by Charles Ragin (1987) and Designing Social Inquiry by Gary King, Robert Keohane, and Sidney Verba (1994) hold that qualitative and quantitative research follow in principle the same underlying logic. 16 This means above all that the hypotheses developed and used in analytic historical work must be checked systematically and in complex multiple ways against empirical evidence. I have argued that this testing in fact takes place in many historical case analyses, though both the raw fact of massive empirical testing and the various sophisticated modes of hypothetically identifying, tracing, and comparing causal sequences have often remained implicit.

Yet comparative historical macroanalysis of cross-case similarities and differences does run into difficulties that are related to the small number of cases available for study. The point is not only the immediate issue of too few cases chasing too many causal factors. This is a disabling problem for many cross-case analyses, though it may not loom as large as many critics assert. Combinations of within-case and cross-case comparison can alleviate it, at least to some extent. Furthermore, nominal comparisons based on deterministic assumptions about the underlying causes do not require large numbers of cases to acquire statistical significance (Mahoney 2000, pp. 395–6, citing Dion 1998). And under some circumstances, it is possible to examine a fairly large number of cases without losing a close acquaintance with each of the histories (see Rueschemeyer et al. 1992). Still, the small number of cases is a serious handicap of macrocomparative research.

Some are inclined to argue that with various assumptions about the underlying structures of reality and thus about causality (see Hall, this volume) and with much increased methodological refinement, small-N historical studies have reached a level of validity comparable to that of the larger-N model of mainstream methodology. Thus, comparative historical analysis should be granted the same standing as quantitative research. The main problem I see here concerns precisely the different underlying assumptions about causal patterns in social reality. While we have guesses about these “ontological” patterns, it is difficult to find out more about the realism of

15 I learned much on these issues from an article on limits of multiple regression in comparative research on advanced welfare states that Michael Shalev made available to me, even though he does not yet want to have it cited in any detail (Shalev 1998–9). I should note, however, that the alternatives Shalev favors are not comparative historical analyses but simpler forms of quantitative analysis that frankly acknowledge the consequences of the limited number of cases, which are at the disposal of comparative studies of welfare states.

16 Ragin’s book is largely a defense of qualitative methods and an attempt to upgrade the level of methodological reflection and practice among qualitative researchers. King, Keohane, and Verba take methodological reflections on quantitative research as their starting point and draw from these conclusions for qualitative research. It is true that greater methodological reflection is one of the strengths of the quantitative research tradition. Yet as indicated, methodological reflection has greatly advanced in comparative historical analysis. A review of King et al’s book that acknowledges both, yet points out that much can be gained by a more equal dialogue, is Munck (1998).
such causal models because of the limitations of the evidence, including very importantly the numerical limitations of cross-case analysis. At the same time, it is worth noting that these limitations are ultimately shared by quantitative work as well, because multiple regression analysis also presupposes models of causality that may very well not be realistic.17

When, for example, and on what grounds can we make the assumption that a set of causes can reasonably be conceived of as deterministic, thus making nominal comparison the procedure of choice? In nominal comparisons, a single deviation leads to the rejection of a hypothesis, but a quite limited number of strategic confirmations increases the credibility of the analysis dramatically. Similar questions arise from any other set of assumptions about causal patterns. It is in this broader sense that we also have to view the age-old arguments that derive from a causal logic embodied in experimental designs, that in macrocausal analysis it is not only impossible to “rerun history” but also extremely difficult to approximate the experimental causal logic with randomization. I consider the limitations an inevitably small number of cases imposes on our exploration of these underlying assumptions ultimately as the most serious of the different small-N problems.

I will conclude these reservations about the theoretical claims that can be derived from historical macrocausal comparisons with an even broader reflection. The revival of comparative historical analysis a generation ago was largely a rebellion against the vacuous conceptual schemes of grand theory, of functionalist modernization theory, and of a neo-Marxism that had turned highly scholastic. This rebellion was also a retreat from the ideal standard for comparative social inquiry that demands replacement of proper names of phenomena that are located in a particular time and space with variables that are specified independently of a particular time and place (Przeworski and Teune 1970, pp. 24-6). In the ideal world of logic this is a persuasive standard. It was not attained by the grand research programs that dominated the first long generation after the Second World War. But if we had to give up on it in order to gain the advantage of nontrivial substantive explanations of important outcomes, it may be useful to be clear about the loss. Historically delimited explanations – historically

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17 Another, more trivial but no less telling reason why quantitative macrostudies often share the limitations of small-N comparative historical research is that for many issues a large number of comparable cases simply does not exist. These limitations are accentuated when, as is often the case, quantitative cross-case analyses confine themselves to the examination of standardized case features and are not complemented by within-case causal tracing.
that while we should not lose sight of the desirability of fully specified and tested hypotheses, we have to come to appreciate a much less impressive but also more subtle and nuanced learning as well. Comparative historical analysis has yielded some powerful theoretical propositions, as well as many plausible explanations of interesting historical developments that use more specific, albeit often historically circumscribed, explanatory hypotheses. Yet it has also contributed much more to our understanding of large and important processes of change. These contributions range from specifying conceptual equivalencies across political, social, and cultural boundaries through the identification of universal or quite general problems that occur in varied historical contexts to the development of highly focused theoretical frameworks that are indispensable for analytic historical research even if they themselves are not (yet) testable theories. I will discuss briefly some of these more limited theoretical advances. In each, comparative historical analysis has been of great importance, and it was so primarily for two reasons: because of its insistence on pursuing important issues comparatively even against the odds of methodological difficulties and because of the lively theoretical engagement with the evidence that is characteristic of this work.

Half a century ago, Robert Merton argued for “middle range theories” against the grand conceptual constructions of Talcott Parsons (Merton [1949] 1968), and T. H. Marshall made a similar plea for what he called “stepping stone theories.” Yet if we inspect more closely a paradigmatic specimen of middle range theory, namely, reference group theory, we make an interesting discovery. Both in its version that deals with cognitive estimates, explaining for instance the perceived chances for promotion in different branches of the military, and in its application to normative orientations, pointing for instance to the role of anticipatory socialization in the course of career advancement, reference group theory instills a good sense of explanation but it is not able to predict. The reason is clear: it does not specify sufficiently what is “referred to, by whom, and under which conditions. It represents a causal hypothesis, but one whose conditions are insufficiently specified. Such underspecification is characteristic of many theoretical propositions that have recently found much attention under the label of “mechanisms.” These are often, in the ironic formulation of James Coleman and Arthur Stinchcombe, “bits of sometimes true theory.”

Incomplete theoretical propositions of this kind are commonplace in contemporary social science. Most models of rational action theory have the same character, as they make underspecified assumptions about what people or organizations are after, how they understand their situation, and by which norms and values they are guided and constrained. And many of the – implied or explicated – propositions that analytically oriented historians deploy in their causal accounts are similarly underspecified.

And yet, it is hard to deny that such bits of sometimes true theory constitute real advances in knowledge. Reference group theory, arguing that cognitive assessments tend to be informed by references to certain kinds of experience, prevents the naive substitution of the observer’s estimate of the “objective” situation for the understandings and assessments of the people observed. The rational choice analysis that identified the “collective action problem” prevents us from assuming that interests shared by a large number of people or organizations will inevitably or almost always lead to coordinated action. If neither is sufficiently specific to be tested directly, they do give a useful new orientation to the study of common problems.

A second category of theoretical results that are easily underestimated if our attention centers exclusively on testable and tested hypotheses are the focused theoretical frameworks for the study of particular substantive problem areas to which I have referred a number of times. These are of profound importance. Theoretical frameworks that identify research problems and offer useful conceptualizations, as well as give reasons for problem choice and the conceptual identification of relevant factors, shape the analysis of a given set of problems and are in turn reformulated in light of the results. They are in one sense meta-theories, but in contrast to traditional grand social theory, they are subject- and problem-specific meta-theories. I characterize them as meta-theories, because they are not directly testable but seek to establish the most fruitful intellectual framework for the investigation. As such, however, they have been of critical importance in comparative historical

research. Thompson (1978, pp. 45–6) speaks of the same thing when he characterizes the use of explanatory expectations in historical investigation:

They do not impose a rule, but they hasten and facilitate the interrogation of the evidence, even though it is often found that each case departs, in this or that particular, from the rule. The evidence... is not rule-governed, and yet it could not be understood without the rule, to which it offers its own irregularities.

Thompson's own reformulation of Marxist ways of framing class formation, emphasizing the lasting effect of socially grounded cultural patterns and the important element of social construction in the formation of class organization and class goals, is a good example of such a subject-specific meta-theory. The most important advances in social and political theory, I submit, are not found in empirically testable and tested specific propositions of universal applicability but in theoretical frameworks of tested usefulness for the study of particular areas of analysis, say of class formation, of revolutions, or of constitutional change.

A much less accomplished and yet cognitively very respectable advance that may result from the dialogue between theoretical expectations and evidence in historical case studies is the identification of universal or quite general problems, the variable responses to which are still poorly understood. The collective action problem offers one of many examples: both its severity and the variety of successful and unsuccessful responses by different actors in historical specific situations are ill understood in spite of the significant advances in theoretically framing the problem in such different areas as the analysis of social movements or state-society relations. Another, more specific problem of this kind that has been examined in a number of varied comparative historical investigations is the social organization of expert services that deploy superior knowledge to the solution of urgent and important problems of a clientele that is less informed and thus limited in its ability to control the experts. Jürgen Schriewer (1999) offers other examples from comparative research on education. Schriewer makes the variability of responses to common problems (together with the interrelations of all social phenomena in a global system) the basis for a proposal to reorient comparative research that diverges much more radically than the ideas I present here from the positivist vision of social analysis. I am not inclined to follow his line of argument, but I do wish to argue that the identification of common and perhaps universal problems should be ranked among the major achievements of comparative case research.

Finally, there is a gain from comparative historical work, even from dual case studies, that is much more modest and yet of critical importance for future comparative studies. Any comparison of similar phenomena in two different societal, national, or cultural settings has to determine conceptual equivalences that cut across the two contexts. This is a far more daunting task than it may appear to the majority of sociologists who either work empirically within their own society or argue theoretically at a level of abstraction that leaves all historical particularity out of view. Yet this astounding dualism in the work of a discipline that claims to study societies and the paucity of comparative work on many of the most common subjects of sociological investigation may be due precisely to the elementary difficulties of establishing such conceptual equivalencies that do justice to two or more macrosocial and cultural contexts and at the same time represent faithfully the analytic intent of the hypotheses and theoretical frameworks involved. Even concepts as simple as medical or legal work require translating local understandings and conceptualizations into a more universal conceptual lingua franca. And this can be done only with a painstaking examination of two or more social and cultural contexts. Such research may actually go beyond isolated conceptual equivalencies and also lead to subject-specific meta-theory or the identification of common problems, but the conceptual equivalencies by themselves are certainly a nontrivial gain.

20 We benefited from this reorientation of class analysis in our work on Capitalist Development and Democracy (Rueschemeyer et al. 1992, pp. 53–7 passim).

21 For a systematic consideration of a large variety of mechanisms involved in collective action see the important pair of books by Mark Lichbach (1995, 1996).

22 Comparative research broadened these studies from a market-based understanding of the delivery of expert services to a more comprehensive understanding that included state-based and other forms of social control (Rueschemeyer 1986, ch. vi). Of the extended literature I cite here only Freidson (1970), Johnson (1972), Rueschemeyer (1973), and Larson (1977).

23 See Schriewer (1999, pp. 63–83). I should add that I was far more inclined toward a dismissive criticism of such research results when some time ago I reviewed the comparative historical work of Reinhard Bendix (Rueschemeyer 1984).

24 In Lawyers and Their Society (Rueschemeyer 1973), I had to deal with the specific conceptual equivalencies concerning legal work. In a later article I focused specifically on the equivalence issues concerning legal work but now ranging across many more countries (Rueschemeyer 1989).
VII. What Can Be Learned from One or a Few Cases?

Comparative historical analysis can do much more than its detractors allow. Yet it falls short – as does its cousin, macrocomparative quantitative research – of the ideal inspired by neopositivism: the standard of a universally applicable social theory whose propositions are substantively meaningful and hold under specified conditions independent of time and place. It is quite possible that the distance between this ideal and actual macrosocial analysis cannot be closed.

Even analytically oriented analysis of single historical cases, however, can yield significant theoretical gains. These gains go far beyond the rejection of determinist propositions from which the case deviates and the increased credibility a proposition receives from a confirmation under least likely conditions. They include the generation of new hypotheses, as well as their testing and retesting against the multiple data points a thoroughly analyzed historical case offers. Much of the skepticism about the theoretical value of single historical case studies derives from the mistaken equation of a single case with a single observation.

Yet single historical processes are not literally “inscribed with their own causation.” The sheer inductivism suggested by this formulation is as impossible in analytically oriented history as it is in other modes of inquiry. In fact, the best analytic history is characterized by a high degree of theoretical reflection that embodies a wide range of previous observations and analyses in theoretical frameworks. The expectations derived from these frameworks focus and guide the inquiry, whether it aims primarily at causal explanation of particular developments or is interested foremost in the formulation and testing of the theoretical propositions used in such explanations.

Analyses that are confined to single cases – whether they are qualitative historical or quantitative and present-oriented in character – cannot deal effectively with factors that are largely or completely held constant within the boundaries of the case (or are simply less visible in that structural or cultural context). This is the reason why going beyond the boundaries of a single case can put into question seemingly well-established causal accounts and generate new problems and new insights. Cross-case analyses are critical for understanding variations in macrophenomena, which frequently modify the dynamics of meso- and microphenomena.

While methodological reflection and the refinement of practices in comparative historical analysis have made great strides in recent years, the limitations of an inevitably small number of cases remain a serious problem.

Can One or a Few Cases Yield Theoretical Gains?

This does not only – or even primarily – relate to the fact that cross-case comparisons involve often too many relevant factors; an equally important problem is that a limited number of large cases does not allow us to come to reasonable conclusions about our assumptions regarding underlying causal patterns. It is here that I see virtually insurmountable obstacles blocking the ability to come close to the ideal of a universally applicable social theory that is at the same time of substantive interest.

Yet reading the best comparative historical work or being involved in such research gives us a sense of insight and understanding that seems at odds with this negative conclusion. In the closing paragraphs, I tried to explicate this sense of intellectual accomplishment. I argued that side by side with convincing causal explanations and the partially generalized propositions these involve, comparative historical analyses yield also other results that constitute real theoretical gains, even if they do not constitute directly testable and substantively powerful propositions. Perhaps the most important of these are the theoretical frameworks that guide analytic historical work and are in turn revised by it.

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Scholars who write about comparative historical methods sometimes make it appear that the research tradition has a single basic approach for identifying patterns of causation. Yet, in fact, comparative historical analysts employ a wide range of strategies of causal assessment in their substantive research. These strategies encompass both methodologies for juxtaposing cases with one another and methodologies for analyzing processes that take place within individual cases. And they include both techniques of causal assessment designed to identify the necessary or sufficient causes of an outcome and tools for locating causal factors that covary with outcomes in linear patterns. Rather than narrowly limiting themselves to any one approach, then, comparative historical researchers are eclectic in their use of methods.

In this essay, I attempt to analyze systematically these different strategies of causal analysis. My main objectives are to specify the concrete procedures entailed in the strategies, discuss their underlying assumptions about causality, and assess their comparative strengths and weaknesses. Along the way, I engage the long-standing debate about small-\(N\) versus large-\(N\) research. I devote particular attention to the ways in which different comparative historical methods are or are not compatible with the assumptions that guide the research.

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