Taking Temporality Seriously: Modeling History and the Use of Narratives as Evidence

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Social scientists interested in explaining historical processes can, indeed should, refuse the choice between modeling causal relationships and studying history. Identifying temporality as the defining characteristic of processes that can be meaningfully distinguished as "history," I show that modeling such phenomena engenders particular difficulties but is both possible and fruitful. Narratives, as a way of presenting empirical information, have distinctive strengths that make them especially suited for historical scholarship, and structuring the narratives based on the model allows us to treat them as data on which to test the model. At the same time, this use of narratives raises methodological problems not identified in recent debates. I specify these problems, analyze their implications, and suggest ways of solving or minimizing them. There is no inherent incompatibility between—but much potential gain from—modeling history and using historical narratives as data.

Across the empirical subfields of political science, we find in recent years a renewed and growing interest in "historical macro-analysis" (Katznelson 1997, 82), which seeks to understand and causally explain processes with an important temporal dimension, such as the formation and evolution of formal and informal social institutions. This trend has given rise to "historical institutionalism" (for overviews see Hall and Taylor 1996; Immergut 1998; Robertson 1993; Thelen 1999), which manifests itself in much of the newer literature on the welfare state and state formation in comparative politics and in the literature on American political development. In international relations, manifestations of this trend toward historical scholarship range from the interest in domestic and international institutions to the *postmortem* debate over the nature of the Cold War. Does this interest in historical processes merely expand the subject matter of political science or does it raise particular methodological problems that require a distinct approach to theorizing and to the presentation of empirical information to test the plausibility or validity of our explanations?[1]

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[1] Throughout this paper I subsume epistemological issues under "methodological" ones.

To advance and extend the methodological debate on this general question, I examine two sets of specific issues. First, what defines "history" as a distinct object of study? What are the implications of such a conception of history for developing explicit theoretical models? Is there an inherent incompatibility between modeling and the quest for explanation and narration of "history," as some observers suggest (e.g., Elster 2000)? Based on an inclusive conception of modeling and an explicit conception of "history" as processes rendered distinctive by the importance of temporality, I argue that modeling such processes is particularly difficult but, nonetheless, possible and desirable. Far from being inherently futile, modeling history is extremely useful, not least because models, by emphasizing the general, help us clarify what is historically and contextually specific when we examine the historical record. Consequently, the "historic turn in the human sciences" (McDonald 1996a) need not lead us away from what scholars of very different persuasions have identified as the particular strength and source of progress of *American* political science: the explicit modeling of the political phenomena we seek to explain, so as to facilitate scrutiny of the deductive logic of the explanation (see, e.g., Milner 1998, and Wæver 1998).

Second, because most historical work in political science is narrative in form, I examine the relationship between models and narratives and, more generally, the strengths and weaknesses of narratives as a type of "presentation of the results" of our analysis (Skocpol 1995, 44). Here, my focus differs from much recent work that concentrates on narratives as a source of empirical information for the analyst, either broadly, conceptualizing all empirical information as "text" to be interpreted (Ricoeur [1971] 1979), or more narrowly, concentrating on specific oral and written "histories" in the form of narratives as constitutive elements of ideas and norms (e.g., Anderson [1983] 1991; Finnemore 1996; Finnemore and Sikkink 1998) with interesting implications for policy (e.g., Van Evera 1994, 36f). These works have been joined by predominantly methodological contributions (e.g., Heise 1993; Lustick 1996), which often seek to raise social scientists' awareness of historians' carefully developed methods to discern

internal validity through the "critical use of sources." All of those works are concerned with narratives as the raw material—as (primary or secondary) *sources*—for the analysis. In contrast, I focus on narratives written by scholars to present the results of their empirical analysis, providing information about actors, institutions, events, and relationships in "a single coherent story, albeit with subplots" (Stone 1987, 74) to provide empirical support for a theoretical argument. Insofar as they are independent of the information used to construct the model, these narratives can serve as data to test the model or as "evidence" to support the model's plausibility.[2]

Using narratives to provide empirical support for one's model has a number of benefits, especially when we seek to examine explanations for historical processes with an important temporal dimension, but it also raises methodological questions that warrant closer attention. Specifically, how do we delineate a sequence of events so as to justify the imposition of a narrative beginning and end onto a continuous empirical record? How does the imposition of a narrative closure affect the generality of our conclusions? What is a narrative's "truth claim?" How useful are narratives for the assessment of alternative explanations? These questions—and how we answer them—matters not only for scholars interested in historical work. The evidentiary status that we attribute to a narrative as a consequence of our conclusions about the relationship between author–scholar and narrative, for instance, affects our reading (and writing) of all work that uses prose to present its empirical results.

I analyze the problems identified by these questions and suggest ways of minimizing their impact on the validity of our findings and the confidence we can have in them. I show how the theoretical model can help structure narratives in ways that make them more suitable as tests and how the use of multiple narratives can increase our confidence in the model, illustrating my arguments with examples drawn from the literature on the state and regime change. I conclude that a careful combination of models and narratives yields clearer insights with greater confidence than a return to untheorized history writing with its often vast hidden causal assumptions and claims. It thus allows the integration of rigorous theorizing and the study of historical processes in a manner that is attentive to, and respectful of, the historical record (cf. Evans 1995, 3f).

[2] The historical narratives discussed here are therefore in many respects similar to case studies, discussed in a number of books and articles on methodology in recent years (e.g., Achen and Snidal 1989; Bennett 1999; Collier 1995; Fearon 1991; Geddes 1990; Jervis 1990; King, Keohane, and Verba 1994; Przeworski 1995; Rogowski 1995; Tetlock and Belkin 1996). However, these narratives are not used as "interpretative" (Lijphart 1971, 692) or "disciplined-configurative" (Eckstein 1975, 99ff) case studies; rather, historical narratives are intended to provide empirical support for original theoretical propositions. Moreover, historical narratives differ from typical case studies in that they trace historical processes over long periods of time to test a theoretical argument with an important temporal dimension—which raises methodological problems not discussed in the case study literature.

MODELING

Any attempt to answer the question of whether modeling and the study of history are incompatible should start with explicit definitions of modeling and history. I take a model to be a schematic statement of a theoretical argument, a hypothesized parsimonious abstraction or simplification of "reality" that depicts a deductively sound, systematic, regular relationship between specified aspects of reality and helps to explain that relationship. Modeling to provide causal explanations, we customarily start by designating the *explanandum* as the "dependent variable" and its hypothesized causes as the "independent variables."[3] Modeling, however, must go beyond identifying regularities in a Hempelian fashion by also specifying the causal mechanisms through which the elements are linked in a regular way (Coase [1981] 1994; Elster 1989, 3–10; Tilly 1997). The resulting models allow us, with the help of specified assumptions, to derive specific hypotheses about the *explanandum*, given particular "values" for the independent variables—though the specificity may be restricted to predicting the direction of change in the *explanandum*.[4] Whether employing the logic of instrumental rationality or not, whether assuming perfect strategic and computational capabilities or more limited ones, whether working quantitatively or qualitatively, we model to emphasize what we consider to be the most important, systematic, and in that sense generalizable elements of the phenomena we seek to explain.

Insofar as the nature of the *explanandum* permits, a more formal exposition of the model tends to facilitate checking "a system of causal generalizations . . . for internal consistency and for conclusions that are not intuitively obvious at the outset" (McClelland 1975, 114), as Robert Powell (1999, 99–102) illustrates in his spirited defense of the promise and practice of formal modeling in security studies. The language of mathematics is particularly suited for scrutinizing the deductive logic of an argument—and using it does not require making rational choice assumptions, as Barry O'Neill's (e.g., 2001) work illustrates—but it also imposes costs on author and reader, which may or may not have corresponding benefits for particular questions and individuals. In short, formalization has some advantages, but a model need not be formal. Rather, the defining characteristic of a model is that it provides an explicit, deductively sound statement of the theoretical argument, separate from a particular empirical context.

By forcing us to specify the elements of our arguments, such as actors and their goals, modeling should

[3] To be sure, not all political science and certainly not all historical scholarship seeks to provide causal explanations (Calhoun 1998, 863ff; Latin 1995, 455; Somers 1998, 745ff). But much of contemporary political science scholarship does, and as Max Weber ([1906] 1978) noted long ago, almost all explanations of political phenomena make at least implicit causal claims. I therefore concentrate on the methodological problems involved when seeking causal explanations for historical processes.

[4] Specificity decreases, for instance, when Jean-Laurent Rosenthal, (1998, 89) relaxes the assumption of a unitary elite in his model of early modern regime change.

increase clarity and explicitness, reducing ambiguity and vagueness. That said, in practice, models often are *not* explicit about all elements of the analysis. Particularly when one mode of analysis dominates a discipline, its core assumptions are often dropped from discussion. These assumptions remain a central part of the analysis, but they are no longer mentioned and may be "smuggled in" and thus become "immune to rational criticism" (Barry 1985, 282). Yet, although modeling *can* be a rhetorical device to hide one's values or other assumptions (McCloskey 1998), it need not be; and explicit modeling surely makes it easier to detect hidden assumptions than leaving the theoretical argument implicit.

Modeling as such, then, does not require any particular ontological, epistemological, or substantive assumptions, though any specific model will have to make them and should make them explicit to facilitate their empirical as well as their analytical scrutiny. By definition, assumptions must be "unrealistic" or "inaccurate" in the sense of being incomplete. But if assumptions are manifestly empirically wrong—as in Milton Friedman's ([1953] 1979, 30) (in)famous example of the leaf-growth pattern on a tree—they cause two major problems. First is the familiar problem of the joint hypothesis test: It will be impossible to tell whether empirical disconfirmation of a hypothesis derived from a model with such assumptions is due to a flaw or omission in the model or due to the erroneous assumptions. Second, when predictions that are based on manifestly wrong assumptions are confirmed empirically, such findings are not very useful, because the models based on such assumptions provide little insight into how the outcomes came about (Coase [1981] 1994, 17). Policy that seeks to achieve or avoid these outcomes, if based on such models, is likely to be ineffective and might be outright dangerous.

The final point to be made before turning to a definition of "history" concerns the roles of deduction and induction in modeling. Deduction is of central importance for the ex ante identification of the elements—the types of (f)actors—that constitute the building blocks of a *possible* model. It is also the central heuristic tool for formulating logically sound relationships among these elements, consistent with the model's core ontological assumptions and consistent with the assumptions driving the causal mechanisms, which themselves are usually derived from very general theories of the constraints, motivations, and cognitive processes employed in decision making and thus shaping human agency. But a model based solely on deduction from assumptions is a shell consisting of "empty theoretical boxes" (Bonnell 1980, 162; see also Smelser 1967, 22ff, 32); it needs empirical content in order to make predictions about what we should observe in a particular instance. To be sure, this content could be supplied by further assumptions that are only subsequently subjected to empirical scrutiny (Kiser and Hechter 1998, 799ff, 802ff), but if we truly had no empirical knowledge at all, what would be the basis for such conjectures? In practice, therefore, the empirical content will probably be supplied inductively.[5] Moreover, induction often has a further role in the modeling process, especially when empirical disconfirmation of our initial model leads us to the inductive discovery of additional explanatory factors that can account for the anomalies. Integrating these factors systematically into the model is surely preferable to having them remain extraneous and theoretically irrelevant (cf. Smelser 1967, 33, 35), as long as we keep in mind that the inductive addition of "auxiliary hypotheses" without subsequent separate tests leads only to hypotheses, not conclusions (King, Keohane, and Verba 1994, 21f, 101ff; Lakatos [1970] 1974, 117f).[6] That said, even works that are entirely inductive would benefit in two ways from making their argument explicit and separating it as a model from the particular case on which it is based. First, such a separation forces the author to distinguish between the conceptually abstract elements of causal relationships and their particular manifestations. This distinction is a prerequisite for assessing validity (see Adcock and Collier 2001): Investigating whether we are indeed "measuring what we think we are measuring" (King, Keohane, and Verba 1994, 25) is nearly impossible if the argument is presented exclusively in operationalized form. Second, such a separation induces the author to specify which parts of the explanation are strictly historically–contextually specific and to differentiate them from the parts that constitute a "potentially generalizable model" (Sewell 1996, 270) in the sense of capturing insights that should, under certain conditions, be applicable more broadly. This differentiation, in turn, facilitates the identification of other cases, if any, on which the argument could subsequently be tested.[7]

[5] For instance, in a paper on popular support for European Union membership in the 1994 Austrian referendum (Büthe, Copelovitch, and Phelan 2002), we start deductively from general theories of European integration and political economy to model support as a function of, among other factors, the economic interests of industrial sectors. We then work inductively from a qualitative analysis of the public debate before the referendum to specify those industrial sectors whose interests had salience for the public and might therefore be expected to have affected the outcome of the referendum. We test the resulting specified model quantitatively, using referendum results from Austria's 121 districts as the dependent variable.

[6] The implied oversight of this caveat renders problematic the following methodological advice by Bates et al. (1998, 16): "The authors derive [testable hypotheses] from theory; but when the case materials do not confirm their expectations, they do not respond by rejecting their models. Rather, they respond by reformulating them and by altering the way in which they think about the problem."

[7] This is not to say that historical works that do not use models cannot be very insightful. Barrington Moore's (1996) classic, *Social Origins of Dictatorship and Democracy*, for instance, presents an argument that is so highly contextualized that Victoria Bonnell (1980, 170) finds that "his generalizations . . . cannot be reduced to . . . models." And yet, it has provided insights and inspiration to several strands of research on regime change, class formation, and processes of social transformation. Similarly, the theoretical model remains largely implicit in Philip Nord's (1995) *The Republican Moment*. Yet its rich and vivid account of the transformation of Parisian civil society from the 1860s to the 1880s, focusing on the emergence of democratic cultures and norms of conflict resolution in autonomous spheres of civil society prior to the successful political democratization of 1871, is surely very insightful for theorists and practioners of democratization

When success breeds success, when variables feed back into themselves, we have an exciting story to tell, but unless we know its metaphors [its model] . . . we have no way to tell it.
McCloskey (1991, 36)

MODELS AND HISTORY: THE PROBLEM OF TEMPORALITY

Several scholars have suggested in recent works that the social scientific study of "history" raises particular methodological problems (e.g., Bates et al. 1998; Elster 2000; Lustick 1996; Pierson 2000). Yet none of them defines what he means by history, not even Terrence McDonald (1996b), in his programmatic essay, "What We Talk about When We Talk about History." So what is it that makes history distinctive?

Conceptualizing history as "what historians do" seems not very fruitful. Notwithstanding some real differences in stance and approach to evidence separating political scientists and political historians (Ingram 1997), what historians study is often studied with similar methods by political scientists, sociologists, economists, and others. Whether or not the study of history raises particular methodological problems for modeling or the presentation of empirical information would then become a matter of contested disciplinary conventions rather than of characteristics of the subject matter (Tilly 1990).

A conception of history as the study of "things of the past" is also not very promising. Studying past events may—but need not—require the researcher to work with sources that call for particular methodological, historiographic tools (McNeill 1998, 4f). Moreover, everything that we can study empirically must already have happened and, in that sense, is a "thing of the past." If "history" is distinctive, then it must be conceptualized as a set of phenomena with distinct characteristics.

I submit that history—as an object of study that may require a distinct approach to theorizing and to the presentation of empirical information—must consist of macroprocesses that cover an extensive temporal space. How does this definition raise particular methodological issues? If the process itself is our *explanandum*, then isolating events from the historical process within which they occur risks depriving us of understanding because any one event in such a temporal sequence, far from being an independent observation, is meaningful only if seen as part of the larger process (Elias [1937] 1997, 80ff, [1939] 1997, 390; Mink [1966] 1987, 64f, 67, 80, 82f). Temporality thus becomes the defining characteristic of "historical" *explananda*. There are, to be sure, several ways of conceptualizing temporality (Aminzade 1992; Griffin 1992; Sewell 1996), not least since time itself can be understood as a social construct; and the significance of the passing of time depends upon the level of abstraction at which we work.[8] But whichever specific conception of temporality we adopt, it calls our attention to the issue of sequence, and it injects a dynamic element. Dynamics complicate the modeling task; sequence enables it.

Dynamics

The institutions within which actors interact are social constructs, as are the aggregate actors that populate so many of our models in political science. Due to factors such as uneven growth, increasing or diminishing marginal utility, and accumulation or ratcheting effects (e.g., Fearon 1998; Pierson 2000; Thelen 1999, 392ff) as well as the tendency of actors to attempt to manipulate or escape constraints (Almond and Genco 1977, 493, 518f), the passage of time makes it, ceteris paribus, more likely that institutions, actors themselves, and their preferences may change. Recognizing this dynamic aspect of temporality does not mean that everything is constantly in flux. In fact, institutions and aggregate actors can be extremely stable for a long period of time (Sewell 1996, 264). But the possibility of change implies that explanations of temporally large processes must *allow* for change in the constitution of actors (e.g., Ertman 1997; Spruyt 1994; Tilly 1998, 7ff) as well as for change in their preferences. In the sense of the inherent dynamism of temporality, then, history is "the study of changes of things that change" (Herbert Butterfield, as quoted in Schroeder 1997, 67). Models of "history" must explain stability rather than assume it.[9]

Because changes in preferences are all too easily invoked to explain changes in behavior and to claim the unsuitability of models that hold preferences constant, it is important to specify what I mean by preferences.[10] I follow Jeffry Frieden in defining preferences as those interests of a given actor that determine how the actor rank-orders the possible outcomes (1999, 42). Preferences thus must be differentiated from the actions that the actor may undertake to achieve any particular outcome ("strategies" in game theory parlance), but they should also be sufficiently specific to the situation that they can unambiguously yield a rank-ordering of the outcomes that would result from the conceivable actions in that situation. Consequently, "what are considered preferences in one [context] might be strategies in another" (Frieden 1999, 41).

A change in preferences becomes more likely over time for two reasons. First, new ideas arise over time. While Mark Blyth (1997) rightly warns against a facile attribution of causal force to ideas, ideas can indeed

alike. I submit, however, that those insights would be clearer and more accessible if the authors had also presented an explicit theoretical model.

[8] The conception and increments of time that are relevant when the process we want to explain is a vast sweep of state formation (e.g., Hintze [1906] 1975; Tilly 1975, 1992) will surely differ from those that might be relevant when we are concerned with the decision-making process that leads to a particular policy decision.

[9] This is one of the strengths of Barry Weingast's chapter in *Analytic Narratives*: His model of U.S. federal institutions allows him to explain why policy remained stable over several decades despite major economic and demographic changes that significantly increased the numerical strength of groups opposed to the status quo and changed some of their preferences (Weingast 1998, esp. 161ff, 184f, 188).

[10] I thank one of the anonymous reviewers for thoughtful suggestions for clarifying this issue.

cause a change in preferences by making altogether new outcomes available, which forces a reordering of the outcomes. The rise of Keynesianism provides an illustration of this phenomenon. At the outset of the Great Depression, certain social actors failed to rank first among their preferences a Keynesian demand-side stimulus policy and failed to influence policymakers accordingly, even though with hindsight the adoption of such a policy would have been the outcome that maximized their material utility. Later, similarly situated actors indeed preferred such a policy over alternatives and employed various strategies to achieve the adoption of Keynesian policies by governments. This finding does not suggest that, at the earlier point in time, the actors were less instrumentally rational in ranking the outcomes and selecting their strategies accordingly—the "outcome" of Keynesianism just had not been formulated yet (Hall 1989).

Second, ideas and especially norms, being social constructs, can change (Ball, Farr, and Hanson 1989; Ruggie 1983; Wendt 1999, esp. 113ff). An example from the literature on regime change illustrates the resulting change in preferences nicely: Assume that policies are at least in part a function of regime type, and assume further that regime type has no effects on the citizens other than through policy (there are no normative commitments to a particular regime type). Social actors (individuals or groups) may, under these conditions, be expected to have preferences over (i.e., rank-order) the regime types based on which regime type affords them the greatest impact on policy (e.g., Bracher [1955] 1971; Hallgarten 1952; Lepsius 1978; Przeworski 1991, passim, esp. 51ff). If actors now develop over time a normative commitment to a democratic form of government, they will probably rank-order the possible outcomes of a regime change differently because the utility that they assign to the outcome "democracy" has increased relative to all other outcomes (e.g., Bermeo 1992; Di Palma 1990; Weingast 1997). The actors may still rank-order their strategies as before and may therefore choose the same action (for instance, acquiescence to the rule of the current nondemocratic regime if the perceived risks and costs associated with doing anything else render other possible strategies prohibitive), but their preferences have changed.[11]

The dynamic quality of temporality suggests that models based on assumptions of stable institutional contexts, stable preferences, and constant units for which we record variable, independent attributes at any given point in time would be unsuited if we are concerned with explaining history, understood as a macroprocess. Yet taking temporality seriously does not require abandoning modeling as such. In fact, standard game theoretic models can incorporate dynamic elements (for an introduction, see Brams 1994, and Gibbons 1992, 55ff, 173ff), and the extensive form used to depict and analyze such dynamic games indeed "take[s] sequence into account" (Bates et al. 1998, 14)—although it is based on a truncated conception of temporality in that, within such game models, "actual chronology is important only insofar as it influences what one player knows about the actions of the second" (Kreps 1990, 18). Models of historical processes, in contrast, need to derive the constitution of the actors and explanatory variables such as actors' preferences within the model in order to allow for change (cf., e.g., Jackson and Nexon 1999, 302ff). At the same time, such endogenization of explanatory variables to capture dynamic change (and explain stasis where it occurs) does not require a fundamentally different approach to theorizing. Some recent work in economics, for instance, relies on only minor modifications of rational choice assumptions to develop dynamic models of preference formation (e.g., Becker 1996), which could be used as a building block of a larger model of an historical process. Evolutionary models developed in biology seem to be well suited to being adapted to explain the sociopolitical processes of persistence and change in the knowledge, values, and habits to which we customarily refer as "culture" (Boyd and Richerson 1985). In sum, endogenizing explanatory variables does not require a fundamentally new approach but can be achieved through building on, or adapting, various existing types of models.

Endogenizing explanatory variables, however, comes at the expense of parsimony or worse: Scholars who seek causal explanations usually frown upon endogenization because when the dependent variable is not only explained by, but also (partly) explains the independent variables, we run the risk of circular reasoning. Can we avoid this problem? Sequence provides the answer.

Sequence

Sequence allows us to endogenize the explanatory variables without having to abandon modeling and scientific aspirations because it enables us to avoid circular reasoning. Endogenization involves incorporating into the model some variation of causal feedback loops from the *explanandum* to the explanatory variables. In a static model, such feedback loops make the argument circular. Determining causality then becomes impossible. The sequential element of temporality, however, gets us around this problem, because it allows us to have causal feedback loops from the *explanandum* at one point in time to the explanatory variables at a *later* point in time only.[12]

The enabling effect of sequence is nicely illustrated by an example of a causal feedback loop in the chapter on education in Abram de Swaan's (1988) *In Care of the State*, where the author seeks to explain the historical process by which elementary education, once seen

[11] Higher-level preferences surely remain the same (in this case, for instance, the preference for longer life or safer possessions, which may be threatened by the current regime's sanctions against prodemocracy activists). But—and this is the key to the differentiation between preferences and strategies used here—those unchanging higher-level preferences are insufficient by themselves to explain the rank-ordering of outcomes in the situation at hand.

[12] Note that anticipated reactions can undermine the assumption that events at time t are independent of events at time $t+1$.

as an individual and local responsibility, came to be provided through nationwide collective and compulsory arrangements, administered or at least regulated by the state. In de Swaan's model, an initial, partial success in increasing the scope and raising the quality of public education increases the opportunities for cross-regional commercial activities and the effectiveness of the state bureaucracies by raising the uniformity of language and knowledge. Over time, both of these consequences of increased and improved education should be expected to swell the ranks of the "metropolitan elites" (supraregionally trading entrepreneurs and central state bureaucrats) who favor the provision of elementary education as a public good. A change in the *explanandum*, education, thus leads to a change in the relative power of the actors in the conflict over the scope and quality of education at a *later* stage. It strengthens the proponents of widened and improved elementary education at the expense of the opponents, such as local elites and clergy, and in turn should lead to a further increase in the scope and quality of education.

Time itself thus becomes an element of the causal explanation, a factor in the model. But time does not function as a standard explanatory variable that directly affects the *explanandum*—otherwise, its effect could easily be expressed through a linear differential equation (McCloskey 1991, 22ff). Rather, it operates in the background to affect several explanatory variables in a variety of ways. Models that seek to help us explain historical processes *qua* processes therefore must explicitly incorporate a temporal dimension and consider carefully how each explanatory factor is affected by the passage of time in the process that we are trying to explain.

In sum, the importance of temporality is the distinctive characteristic of "historical" phenomena as objects of study, which raises particular methodological issues. The dynamic element of temporality complicates the modeling task by demanding that our models allow for the possibility of change (and hence explain rather than assume stability), but the incorporation of a sequential element enables us to do this without running the risk of the circular reasoning often associated with endogenizing the independent variables. Yet temporality raises additional problems when we move to empirical testing.

> *Model building is important for working out the internal logic of a chosen set of assumptions and relationships. But rigorous empirical analysis is needed to ensure the* relevance *of those assumptions and relationships.*
>
> Lazonick (1991, 303, emphasis added)

CONFRONTING MODELS WITH DATA: HISTORICAL NARRATIVES

Ways of confronting a model with data range from showing the consistency of the theoretical argument with one empirical observation of the *explanandum* (Eckstein's [1975, 108ff] "plausibility probe") to a test on a large sample that meets the criteria of statistical analysis (irrespective of whether we employ a quantitative or qualitative methodology). Where on this continuum a given work falls does not affect its usefulness or insightfulness but should affect our confidence in its conclusion, keeping in mind that any single work usually exists in the context of larger theoretical and empirical literatures. At any given point along the continuum, multiple analytical techniques are at our disposal. From the viewpoint of the presentation of empirical information, we can differentiate broadly between a primarily quantitative presentation, based on statistical analysis, and narratives, based on qualitative techniques such as process-tracing. In this section, I spell out what makes narratives a particularly suitable form of presenting empirical information when we want to test models about historical processes, and I suggest ways of structuring narratives to attain this benefit.

Why narratives? Practical constraints, such as too few instances of a given macrohistorical process, may inhibit the use of statistical techniques. Yet time series analysts have long used lagged variables, and in recent years some scholars have begun to develop more sophisticated procedures to control for time dependence in political phenomena (Beck and Katz 1996; Beck, Katz, and Tucker 1998). In short, we have very suitable statistical tools for the empirical analysis of models with dynamic and sequential elements, although if the nature of the *explanandum* indeed is such that all stages within each instance of the historical process (the *explanandum*) are interrelated, one would need to incorporate into the statistical model a lag for each variable at each stage of the process prior to the final stage, with a corresponding rapid decrease in the degrees of freedom.[13] Moreover, various elements of the model and additional implications may be separately tested using statistical techniques. Statistical methods can thus be used in the empirical analysis of historical processes and may be particularly valuable when complementing and reinforcing insights gained from qualitative techniques (cf. Beck, Katz, and Tucker 1998; Berg-Schlosser and Quenter 1996; Mahoney 1999).

A preference for narratives, then, is due not to the unavailability of analytical techniques that lead to other forms of presenting our results, but to particular strengths of the narrative form. The most important of these strengths is that narratives, in addition to presenting information about correlations at every step of the causal process, can contextualize these steps in ways that make the entire process visible rather than leaving it fragmented into analytical stages. Moreover, narratives allow for the incorporation of nuanced detail and sensitivity to unique events, which may be necessary to understand the particular manifestation of an element of the model but which are beyond the model.

[13] The problem here is neither a lack of information, since information about the pertinent variables at each stage of the process is equally required for qualitative-narrative techniques such as process-tracing, nor quantification per se, which, at least at the basic level of more versus less, is usually possible. Collaborative database projects show that such complex information can be gathered for many cases. All of these techniques of course require carefully specifying the functional equivalents across different instances of the process.

In de Swaan's narrative of the introduction of state-provided education in The Netherlands, for instance, he notes how the Napoleonic invasion made the central metropolitan elite suddenly exceptionally independent of local elites and clergy, thus strengthening them in the societal group conflict over the expansion of the state, including the expansion of public education. This unique event affected the speed of change and thus the particular manifestation of the process, but it did not change the general dynamic of the process as captured by the model.

At the same time, narratives must not revert to untheorized historical accounts, invoking extraneous factors in an ad hoc fashion, because such accounts are not useful as a test of the causal propositions. How can we avoid this problem? The model itself can help us write narratives that are useful as a test of the argument.

The theoretical model can be used to structure the narratives. As we know from Arthur Danto's (1965, 149ff) thought experiments, even the imagined "ideal chronicler" who records every action and event in perfect chronological order cannot provide a complete history, let alone a causal explanation. Any historical narrative therefore must simplify "reality" by designating some elements as salient and omitting many more as not significant (McClelland 1975, 75ff). The model can help by providing the criteria for what is salient: The actors identified in the model constitute the actors of the narrative, which traces their goals, beliefs, and actions. Within each narrative we thus employ process-tracing (Bennett 1999; George and McKeown 1985). The influence of other elements of the model, such as how temporal progression affects the actors and their preferences, should be systematically described.[14] Beyond the elements identified in the model, however, additional context-specific information should be minimized. Information that is extraneous to the model should be provided only insofar as it affects salient elements and is needed either to understand the relationship between these elements or to appreciate the contingencies of a particular historical process. If the narrative cannot be written in terms of the model, something is wrong with the model.

Using the model in this way to discipline the narrative ensures equivalence in the sense that each narrative contains the same (or at least functionally equivalent) elements. Each narrative thus becomes a unit or "observation"—a "plausibility probe" to test the causal argument, though we may not be able to assume independence for testing purposes (Sewell 1996, 258f).[15] And most importantly, the model allows the analyst to overcome the problem of deciding what matters for the narrative (cf. Mink [1978] 1987, 187f)—the problem that leads to the ad hoc-ness of many inductive historical explanations.

HISTORICAL NARRATIVES AS DATA: FOUR PROBLEMS AND (PARTIAL) SOLUTIONS

Notwithstanding the above strengths of narratives, their use as data brings to light four problems and limitations not identified in recent methodological work and debates on history, narratives, and social science theory.[16] The first two problems are particular to the use of narratives as data in conjunction with models of historical processes; the third and fourth apply to any use of narratives as data.

The first problem concerns defining or delineating historical sequences as distinct, or what Bearman, Faris, and Moody call "casing" (1999; see also Mink [1966] 1987). If we conceive of history as a continuous stream of interrelated events, then at the logical extreme there is no beginning and no end. A narrative, however, inherently imposes a beginning and end onto the historical record. To be sure, all empirical work—whether large-*N* statistical or small-*N* case study work—needs to justify the boundaries of its units of analysis, especially when proceeding on the assumption of independence of observations (cf. King, Keohane, and Verba 1994, 222). But the emphasis on the interrelatedness of events across time makes this problem particularly acute for the empirical testing of models of history, while the literary and "aesthetic" (Topolsky 1998) qualities of the narrative tend to obscure it.

The model can provide a deductive, albeit only partial, solution to this problem. By specifying the *explanandum* in general terms and theorizing temporality explicitly, the model delineates a sequence as distinct—based on our research objectives—in the potentially infinite space of time. Note, however, that the *explanandum* itself provides a justification for choosing the starting and ending points of the narrative only insofar as the historical process we seek to explain can plausibly be said to have had a clear starting point (e.g., an exogenous shock) and to have "run its course." A number of sociologists have in recent years developed alternative *inductive* procedures, often grounded in the boundary specification approach of network theory, for delineating an event sequence as a distinct process and hence justifying a distinct narrative with clear end points: "event structure analysis" (Heise 1993), "abstraction and generalization of interactions" (Abell 1993), "interaction process analysis" (Kosaka 1993), and "bicomponent analysis" of event populations (Bearman, Faris, and Moody 1999). However, questions remain about the logic underpinning these procedures (see Abell et al. 1993), and I doubt their general applicability to macrosocial phenomena. This leaves us with a clear specification of the *explanandum* as the only generalizable, methodological justification

[14] Carefully tracing the positions taken by each of the actors identified in his model, de Swaan shows, for instance, that over time (for reasons consistent with the model, such as a growing cohesion of a group-social actor), the demands of working-class parents intensified, whereas "time" *reversed* the preferences of industrialists.

[15] I address the issue of independence below, when discussing the use of multiple narratives as a remedy for what I identify as the "third problem" of historical narratives as data.

[16] For prominent contributions to those debates see Abell et al. 1993, Elman et al. 1997, King, Keohane, and Verba 1994, Kohli et al. 1995, Laitin et al. 1995, Mahoney 1999, Ragin, Berg-Schlosser, and Meur 1996, Schroeder 1994, and Somers et al. 1998.

for making choices about where to begin and end a narrative.

De Swaan (1988), whose use of historical narratives is exemplary, achieves such a delineation of a temporal sequence by defining his *explanandum* as the process by which elementary education was transformed from a private and local affair into a responsibility of the state, provided at a basic level throughout the territory within its reach, according to certain standards (e.g., a uniform language) and financed through compulsory measures. This definition of the *explanandum* suggests that the narrative must start with the initial moves (ideational or practical) away from the previous local and private system of schooling but need not be concerned with that system's prior history. And the narrative can end when uniform, state-financed elementary education has been established in the country in question, without needing to concern itself with the subsequent evolution of the educational system or other related aspects of the welfare state. The specification of the *explanandum* thus provides the criteria for choosing the beginning and end of the narrative.

The second problem concerns the need to "conclude" the narrative while the process may be ongoing, which restricts the "generality" of our conclusions (King, Keohane, and Verba 1994, 137). If we use feedback loops in our models, through which a change in the value of the dependent variable is hypothesized to change the value of some or all of the explanatory variables at a later point in time, time itself acquires an explanatory role (in interaction with the original explanatory variables). As long as only the explanatory variables, and not the *explanandum* itself, are affected by time in this way, truncating time by setting an end point for the narrative does not introduce the selection bias that is caused by truncating the range of the dependent variable (King, Keohane, and Verba 1994, 128ff). But more than with conventional independent variables, of which we may restrict the range, we have to be very careful about assuming consistent continuity of the relationship between the explanatory variables and the *explanandum* beyond the investigated period. When time itself becomes a factor in the model, as discussed above, we have to consider carefully how each explanatory factor is affected by the progression of the process that we are trying to explain. The extent to which we can expect the effect of time on the *explanandum* (through the other independent variables) to continue as observed during the time period covered by the narrative depends on the tenability, beyond this time period, of the assumptions that we had to make to model the effect of time. This limitation of historical narratives affects the confidence we can have in the generalizability of the insights we gain from them. The solution, however, lies not in modestly claiming that our conclusions cannot be generalized but, rather, in paying careful attention to temporality in both the model and the narratives and specifying the implications for generalization accordingly.[17]

Third, what is the status of any narrative's *truth claim*? As Mink ([1978] 1987, 199) put this "dilemma of the historical narrative":

> . . . As historical it claims to represent, through its form, part of the real complexity of the past, but as narrative it is a product of imaginative construction, which cannot defend its claim to truth by any accepted procedure of argument or authentication.

The problem here, as Andrew Norman (1991, 131) points out, is not that "a discursive representation has a structure that that which it represents does not." There is no necessary link between discursive structure and *mis*representation. Moreover, some objective criteria for assessing a narrative's truth claim exist, such as the extent to which an author is able to provide from the historical record evidence that, after having been subjected to standard historiographic procedures, supports the author's "story." It is therefore hardly necessary to equate historians with novelists or fiction writers (Gaddis 1992/93, 56). Rather, the problem is that, because facts never speak for themselves (Lustick 1996), there is an interpretative element that cannot be evaluated from within the narrative without circular reasoning. To assess this aspect of the narrative, the reader will have to draw on knowledge from sources *external* to the narrative at hand; i.e., the reader will need to know something about the historical period in question from other sources—sources that themselves are bound to have narrative qualities. Consequently, it is more meaningful to endorse good narrative work as "plausible," "persuasive," or "compelling"—as seems to be the practice among historians—rather than "true" or "right," though we certainly may find some narrative work that is poor and even plain "wrong," such as when its interpretation is marred by logical inconsistencies or makes incorrect assertions about the chronology of events.

Moreover, that the truth claim of a narrative cannot be assessed from within the narrative itself also has important implications for the utility of narratives as data on which to test hypotheses derived from models. Presumably, no one will consciously publish a model with empirical information that directly contradicts it. Except for discarded alternative explanations (see below) and possible oversights, then, the vulnerability of the model to empirical disconfirmation is ultimately also external to the narrative.

However, the use of multiple narratives may increase our confidence that the model indeed captures the key dynamics of the process. Using multiple narratives is appropriate because models should be applicable to more than a single instance if they have the benefits

[17] The applicability of a historical model by no means needs to end with the time period on which it was empirically tested. Otto Hintze ([1906] 1975), for instance, long ago provided a fascinatingly simple example of what we would today call a "second image reversed" (Gourevitch 1978) model of state formation, capturing an essential element of a causal mechanism that might explain not only the various transformations of the "organization of the state" over the thousand-year stretch of European history to which he applies it, but also phenomena that occurred many decades after the formulation of his model, such as European integration and the later parts of the "third wave of democratization" (Huntington 1991, esp. 85ff).

of capturing what is generalizable. Having assured that each narrative contains the same elements, we can employ each narrative as a unit or "observation" to test the causal argument. To be sure, we are unlikely to be able to furnish sufficiently many historical narratives to perform statistical tests, and by virtue of treating each historical narrative as a self-contained unit so as to incorporate temporal progression into each "observation," we have to reject any attempt to make "many observations from few" (King, Keohane, and Verba 1994, 217ff). Moreover, there is the problem of independence.[18] As William Sewell and others point out, if separate historical instances of a process are treated like ideal-typical scientific experiments, such treatment assumes that they are independent of each other. This assumption is problematic because, unless there is a perfect informational separation between the instances, actors in one (later or perhaps contemporaneous) instance will have knowledge of the constitutive actions and the outcome of the other instance(s) (Sewell 1996, 258f). Such knowledge will violate the assumption of independence—for purposes of testing a model—only if it changes elements of the process as modeled (e.g., the preferences of actors), which is by no means a necessary, though a possible and indeed quite likely consequence of having knowledge of prior or other instances.[19] Where empirical analysis suggests such an effect, we therefore cannot rely upon the statistical logic of the traditional comparative method (Lijphart 1971). But as "plausibility probes" multiple narratives are very useful. Increasing the number of confirming narratives does not in any way "prove" the model (Mohr 1996, 118ff), but in light of the temptation of inductivist modifications of a given model, the ability of a model to withstand the difficult test of application to different occurrences of the *explanandum* without ad hoc alterations makes more plausible that it has captured the central, generalizable dynamics rather than unique elements of a particular case.

In fact, it is the use of multiple narratives that makes Theda Skocpol's (1979) *States and Social Revolutions*, Margaret Levi's (1998) "Conscription: The Price of Citizenship" (1998), and, especially, de Swaan's (1998, 52–117) "The Elementary Curriculum as a National Communication Code" so convincing. To be sure, the extensive methodological debate about Skocpol's book has produced "little consensus" about the status of her narratives (Mahoney 1999, 1156), and her consciously inductive approach makes it questionable to treat her narratives as data to test a model, but the book derives most of its persuasive power precisely from her ability to narrate three instances of social revolutions in the same terms. Multiple narratives work even more effectively in Levi's "Conscription." Though it is sometimes difficult to see how the various parts of her complex model structure the three narratives, each of her three accounts of the abolition of "buying one's way out of military service" (Levi 1998, 109), in France, the United States, and Prussia, shows the same, separately theorized process at work. Finally, de Swaan uses five separate narratives (for the United States, France, Britain, Germany, and The Netherlands) to test his—separately theorized though not fully explicit—model of the historical process by which elementary education becomes the domain of the state. In all of these works, what persuades the reader of the validity and usefulness of the model is that for each of the countries, as Sewell (1996, 262) puts it with respect to Skocpol, the specific historical process "can be narrated convincingly in terms of the operation of analogous causal processes, which. . .[thus] make sense of numerous details that otherwise would seem purely accidental." In this sense, providing multiple "plausibility probes," far from being "fruitless[ly] repetiti[ous]" (Skocpol and Somers 1980, 191), should enhance confidence in the explanation. This way of combining models with narratives allows us to provide a "scientific" causal explanation of historical processes, without depriving them of their process character.

Fourth, due to the limited truth claims of narratives, those who use historical narratives as empirical evidence for a causal explanation will probably fail to assess alternative explanations and, if they try, will fail to convince skeptics. Trying to assess alternative explanations would entail providing a second set of narratives, similarly structured by the underlying model of an alternative explanation for the same phenomenon. To be sure, such alternative narratives could conceivably be supplied by the same author just as those working quantitatively often operationalize and test alternative explanations. In fact, Graham Allison (1971) employed three separate narratives to test three alternative, competing models of foreign policymaking in *Essence of Decision*.[20] But alternative narratives by the same author serve primarily as a rhetorical device in support of the primary, favored explanation. The interpretative freedom of the author makes it unlikely that less convincing alternative narratives would be accepted as sound evidence of the failure of the alternative explanations. This, however, is not a serious problem. As Morris Fiorina notes, meaningful alternative explanations are much more likely to be advanced by others whose "perspectives and commitments" allow them to argue as strongly as possible in support of those alternatives. All such explanations must then be subjected to the collective assessment of the scholarly community at large (Fiorina 1995, 92).

CONCLUSION

I have pursued two objectives: (1) I have sought to clarify, based on a broad conception of modeling and an explicit definition of "history," the difficulties—and possibilities—of modeling historical processes; and

[18] I thank one of the anonymous reviewers for thoughtful comments on this issue.

[19] Statisticians therefore speak of seeking "independence of the error terms," which is a purely empirical matter, rather than "independence of observations."

[20] I thank Steve Solnick for reminding me of Allison's use of alternative narratives.

(2) I have sought to make the case for narratives, identify four overlooked problems with their use as data to test models about historical processes, and offer at least partial solutions to these problems.

I have argued that modeling is rendered more difficult when dealing with "history" due to the importance of the dynamic and sequential aspects of temporality for processes that can be methodologically meaningfully distinguished as "historical." But social scientists interested in the study of such "history" need not return to narratives that, detached from any explicit causal model of the historical process they seek to capture, implicitly make—often sweeping—causal claims based on hidden assumptions and unspecified causal mechanisms. There is no inherent incompatibility between the deductive modeling of causal relationships and the study of history. Quite to the contrary, for social scientists—and historians—who seek causal explanations of historical processes, making their models explicit and subjecting them to separate theoretical examination have several advantages. Most importantly, the greater transparency of a model (compared to a purely "operationalized" argument) facilitates the scrutiny of the argument's assumptions and internal logic, which is particularly desirable for inherently complex historical phenomena. Further, models help clarify what insights the author suggests to the reader, by inducing the author to specify which elements of the argument are, in the author's mind, uniquely specific to the historical setting and which parts of the argument are potentially generalizable. In fact, the model should be epistomologically prior to, and independent of, the narrative(s) used to "test" it, in order to enable the model—rather than the narrative—to do the explanatory work (cf. Somers 1996, 79ff).

Regarding the benefits of using narratives, I have emphasized the ability of narratives to contextualize any given stage in the process, which is essential when we seek to capture empirically the process for which the model stipulates an explanation. To be useful as a test of a deductively sound model, a narrative should be structured by the model in that the presentation of empirical information follows the model's identification of actors, their preferences, etc., so as to minimize the ad hoc character of the empirical account.

At the same time, using narratives as data to test a model that seeks to capture an entire historical process raises four problems not identified in recent debates. I have specified these problems and analyzed their implications for the usefulness of narratives as data. I have suggested ways of solving or minimizing the problems, including the use of multiple narratives to increase our confidence that the model indeed captures the key elements of a causal explanation of the processes that we are seeking to explain.

So does the study of history, understood as the analysis of processes that span a large temporal space, require a distinct approach to theorizing and to the presentation of empirical information? I conclude that it indeed engenders particular problems and requires us to be attentive to issues that may not arise in analyses of phenomena where temporality plays no role. But the study of history does not require a fundamentally different approach. Notwithstanding some remaining limitations, historical narratives can provide strong support for the plausibility of a historical model, maintaining a basic commitment to a scientific approach while overcoming several of the problems of the compatibility of historical narrative and social science identified by critics of the "protoscientific" ambitions of historical analyses. And it may be that at the level of broad historical phenomena, where the temporal dimension is crucially important to any explanation, providing such support for the plausibility of the argument is the best we can hope for, at least from any *one* scholar. Science is, after all, a collective enterprise (Weber [1919] 1946).

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