

Although emerging streams of historical institutional (HI) analysis have generated substantial insights in the field of comparative politics, this scholarship has lacked a self-conscious approach to methodology. This article specifies the comparative historical methods that many HI scholars have implicitly used for estimating the causal effect of political institutions on key policy and other political outcomes. It demonstrates how various periodization strategies are deployed to sort out the influence of a host of hypothesized and rival explanatory factors. In addition to explicating these methods, the article critically examines recent works of HI scholarship, highlighting the analytical leverage generated through studies that might ordinarily seem to suffer from the problem of small samples. More explicit deployment of these methods would both improve the quality of HI analysis and make its findings more transparent for further evaluation and emulation.

## CAUSAL INFERENCE IN HISTORICAL INSTITUTIONAL ANALYSIS

### A Specification of Periodization Strategies

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**I**n recent years, historical institutional (HI) scholarship has proliferated, providing powerful insights into a broad range of substantive problems, including questions about the sources of variation in labor policy, welfare policy, taxation, and state-labor relations across both time and space. Nonetheless, several scholars who have recognized the theoretical handle this perspective provides for grappling with important questions of political economy and whose own work may be described as HI have called for more careful reflection about research design and methodology (D. Collier, 1999; Immergut, 1998; Katznelson, 1997a, 1997b; Thelen & Steinmo, 1995). Immergut argues that unless better approaches to this research are developed, it stands to be marginalized in the field of comparative politics; she calls for

greater “systematization” (Immergut, 1998, p.27). Skocpol points out that a key problem of much comparative historical research is the lack of exposition of methods (as cited in Kohli et al., 1995, p.44). Absent a clear statement of how inferences were developed, too much of this work is susceptible to the criticisms leveled by scholars such as King, Keohane, and Verba (1994), who bemoan qualitative research that has not been designed in a manner that allows for the development of clear causal inferences. Because the narrative style of reporting historical analysis does not generally lend itself to explicit statements of analytic strategies—or at least not to the extent typically associated with statistical analyses—other scholars may find it difficult to evaluate, replicate, and/or emulate this research.

In this article, I argue that many HI scholars share not only a common theoretical approach but also a common set of methods that provide a powerful basis for making causal inferences. Through the use of comparative historical methods and, in particular, through a set of deliberate periodization strategies, it is possible to generate many degrees of freedom beyond the number of country cases being investigated while being sensitive to problems of measurement validity (Adcock & Collier, 2000) and causal complexity. These methods are appropriate for evaluating general theoretical claims and the more specific empirical explanations advanced by HI analysts. The central goals of this article are to specify these methods explicitly and to demonstrate that they provide a sound basis for making causal inferences.

The body of the article is composed of three parts. First, it identifies the central components of a shared HI theoretical model. Second, it identifies the steps involved in developing and testing the model through comparative historical analysis, emphasizing the deployment of various periodization strategies. Finally, the article demonstrates how this common theory and method have been implemented to understand a range of questions and problems in several noteworthy studies (Immergut, 1992; Rothstein, 1995; Steinmo, 1993).

## HI THEORY

Before turning to the methods employed in HI scholarship, it is necessary to specify some of the core claims that form the basis for a shared theoretical foundation.<sup>1</sup> A central goal of most HI analysis is to estimate the impact of variations in institutional forms and configurations on a particular outcome

1. For a review of the types of historical institutional (HI) analyses that make the theoretical claims I discuss here, see Thelen and Steinmo (1995) and Immergut (1998).

or set of outcomes. It is historical because analysts argue that once constructed at a moment in history, institutions typically endure for significant periods of time, influencing political dynamics and associated outcomes in subsequent periods. Institutions are defined in this literature as the formal organizations and the informal but widely accepted rules of conduct that structure a decision-making or political process. This definition is necessarily vague because analysts working within this framework conceptualize and define institutions in more precise ways according to their own questions and theories. Examples of institutions typically employed in HI analysis include party systems, constitutions, corporatist structures, and financial organizations. HI scholars attempt to demonstrate how the salient characteristics of one or more of these institutions affect specific political dynamics, in turn producing distinctive outcomes.

HI analyses specify institutions as central explanatory variables, but the hypotheses are not simply bivariate relationships. As Thelen and Steinmo (1995) point out,

Institutions constrain and refract politics but they are never the sole “cause” of outcomes. Institutional analyses do not deny the broad political forces that animate various theories of politics. . . . Instead, they point to the ways that institutions structure those battles and in so doing, influence their outcomes. (p. 3)

In other words, HI theories explain outcomes in terms of the joint effect of changing, noninstitutional variables (which I will describe as background variables) and “sticky” institutional factors that tend to change more slowly. A key background variable employed implicitly or explicitly by HI analysts is social structure—often conceptualized as a configuration of economic resources and relationships within society. Virtually all HI analysts assume that important changes in this variable over time or across places will be associated with important differences in the outcome they consider. More interesting, they argue that given similar scores on some measure of social structure or similar patterns of over-time change across places, associated outcomes will differ in the context of different institutional arrangements. Even beyond specifying the ways in which decisions are made, institutions help shape and aggregate the interests of societal actors. In these ways, institutions convert a wide range of possible political outcomes into a much narrower range of probable ones.

Most HI scholars implicitly or explicitly argue that variations in outcomes across time and space result from some specified relationship between institutions and the ways in which they mediate changes in at least one exogenous

variable plus some random error. In more formal terms, this relationship can be written as

$$Y_{i,t} = f(I_{i,t}; \mathbf{X}_{i,t}) + e_{i,t} \quad (1)$$

In these equations, the subscripts  $i$  and  $t$  denote the spatial unit and time, respectively;  $Y$  is a variable representing the outcome under consideration;  $I$  is a variable representing the institution;  $\mathbf{X}$  represents a vector of noninstitutional explanatory variables (for example, industrialization or dummy variables for interventions such as war); and  $e$  stands as an error term for the stochastic model. Equation 1 suggests that outcomes vary across both space and time. As Thelen and Steinmo (1995) point out, institutions themselves may vary over time or their relevance within a society may change, in turn affecting the behavior of political actors and the policy and other outcomes under investigation.

HI theories also tend to recognize that the effects of change on an explanatory variable may not be realized until a subsequent period, and so the effect is “lagged.” Outcomes may also become resistant to change over time, as they are themselves the product of outcomes during prior periods—what is known as an autoregressive process.<sup>2</sup> By implication, this suggests that a better model may be

$$Y_{i,t} = f(Y_{i,t-1} \dots Y_{i,t-p}) + f(I_{i,t} \dots I_{i,t-k}; \mathbf{X}_{i,t} \dots \mathbf{X}_{i,t-k}) + e_{i,t} \quad (2)$$

Equation 2 implies that  $Y$ , for a given time and place, is a function of the institutional variable and other explanatory variables, measured in the current period and periods up to  $k$  time periods prior to the period  $t$  under consideration. Prior values of  $Y$ , as far back as  $p$  periods, also strongly influence  $Y$  at time  $t$ .

The underlying theory implied by the specifications in Equations 1 and 2 is that changes in a particular outcome are caused by the institutionally mediated impact of some exogenous factor(s). HI scholars argue that similar values for  $\mathbf{X}$  will produce different outcomes when the value of the institutional variable differs across cases. Because the value of  $I$  is sticky and tends not to change very much over time for a particular place, the value of  $Y$  will tend to vary in a range narrower than if we considered the impact of changes in  $\mathbf{X}$  in all places with different institutional settings.<sup>3</sup> Thus a central claim of HI theory is that the stability of certain types of institutions effectively constrains

2. See Cromwell, Labys, and Terraza (1994); Mills (1991); Greene (1997); or other basic time-series texts for discussion of time-series models and autoregressive processes.

3. See Thelen (1999) for a discussion about identifying the mechanisms of reproduction in analyses of path dependency in HI research. Also see Mahoney (1999).

the range of outcomes on the dependent variable, suggesting that moments of institutional origination and institutional change are critically important.<sup>4</sup> Finally, HI scholars generally advance their arguments in light of other rival explanations, identifying the inability of a vector of rival explanatory variables (**Z**) to account for important variation on the outcome under investigation.

### COMPARATIVE HISTORICAL METHODS OF HI ANALYSIS

HI scholars tend to pursue their empirical research with the dual purpose of specifying and elaborating theoretical claims such as the ones previously described and of answering specific empirical problems in light of particular cases. Although some analysts rely largely or partially on statistical methods, most employ comparative historical analysis to develop and test their arguments. Unlike statistical analyses, which benefit from a shared language for describing technique, including labels for estimators and shared standards for reporting estimates, comparative historical analyses are not reported in a standard manner. However, the absence of explicit statements about method and a common language for reporting findings does not imply an absence of deliberate methods. Rather, comparative historical investigations of HI theories often combine a mixed deployment of cross-sectional and longitudinal analyses that parse out the various testable implications of the theory. This section attempts to specify these methods in general terms, and the next section highlights examples of these methods.

#### CROSS-SECTIONAL ANALYSIS

HI scholars tend to begin their analysis with static, cross-sectional investigations of contemporary outcomes. More than anything, such analysis helps to motivate the empirical problem by identifying unexplained variance on the dependent variable. Either multivariate regression analysis or Mill's (1843/1961) methods of similarity and difference, or a combination of these approaches, is generally employed as the entry point into a research question. Both provide some basis for dispensing with rival hypotheses and help to determine if the institutional hypothesis is plausible. For example, level of

4. See Haydu (1998) for a discussion of the different causal mechanisms that can relate historical developments, including the creation of political institutions, to outcomes over time. In particular, see the discussion of repeated problem solving.

development is almost always considered as a possible explanatory variable. If cases with similar values on this variable vary on the outcome under investigation, this suggests the need for alternative explanations—that is, that modernization or capitalist development cannot account for this variation. That is not to suggest that such factors are not associated with the outcome—because such development may indeed have given rise to the outcome under investigation—but that other explanations are needed to account for cross-national variation. Although some variables may account for some differences across cases, the static analysis helps to identify the amount of variation left unexplained by noninstitutional factors.

These are quite reasonable first cuts at the analysis, which help scholars to develop more focused comparisons across just a few cases, but on their own, these strategies generally provide a relatively weak basis for causal inference because of the small number of cases and variables considered. Because the model involves the historical creation of a particular institution and its impact on certain outcomes during a period of time, the static analysis tends to raise almost as many questions as it addresses. As John Stuart Mill (1843/1961) himself recognized, the methods of difference and agreement, like static regression analysis, are useful only for evaluating constant causes. Meanwhile, HI arguments are specified in terms of dynamic processes in which outcomes are often the accumulated responses to particular stimuli or other dynamic changes. Moreover, although institutions endure, they are recognized as having historical origins and as being capable of change, suggesting that they are not permanent. When such theoretical concerns are joined with comparative static analysis, one is left to wonder whether some other influence prior to the development of those institutions may have given rise to the variations on the dependent variable. Is the causal arrow pointing in the right direction? Moreover, because the theory argues that institutions do not give rise to outcomes as a sole independent variable but merely influence those outcomes through the intermediation of other pressures, the static analysis is unable to make sense of this dynamic relationship.

### **LONGITUDINAL ANALYSIS: PERIODIZATION STRATEGIES**

Comparative analysis of sequences of events, processes, and outcomes provides opportunities to generate much higher levels of confidence that observed correlations are truly causal relations because, by definition, causes must precede effects. “One needs diachronic evidence about historical sequences to explore and to test ideas about causation directly”

(Rueschemeyer & Stephens, 1997, p. 57). Specifically, HI scholars employ careful cross-period analyses—described here as periodization strategies—within and across country units. These analyses provide an opportunity to examine the impact of over-time change in key explanatory variables on over-time change in the dependent variable.

The move from comparative static to comparative historical analysis of HI theories requires the dissection of the historical chronology of places into analytically useful periods. Periodization is a cornerstone of virtually all historical analysis that involves the simplification of history through the recognition of certain types of events or processes as more “important” than others (Katznelson, 1997a) and that uses the dates of those events as dividing lines for a chronology. Periods are bounded by important events, changes, or turning points that can be conceptualized as markers of variation in a potentially important explanatory variable.<sup>5</sup> I will characterize these turning points as moments, realizing that some moments may endure for several years. Wars, presidential elections, external price shocks, and regime changes are all examples of important events that may be useful period markers because of their potentially important impact on the outcomes under investigation. The analyst must identify what such events signify for the general model—that is, does a particular event or observation reflect change in an institution, a noninstitutional explanatory variable, or some historical contingency that may be treated as a rival explanatory variable? As in any study, certain facts and observations are privileged over others because they are assumed to have greater relevance to the question under consideration. Within a mass of historical observations, only a few events define a period, whereas most other events and processes are explained as taking place during a period.

HI analysts attempt to find evidence of both immediate and delayed causal effects by assessing the impact of change in one variable on another across cases. This technique is what Mill (1843/1961) describes as the method of concomitant variation, asserting “that every modification of the cause is followed by a change in the effect” (p. 263). This practice is analogous to Campbell and Stanley’s (1966) time-series and multiple time-series designs, which involve examinations of an outcome for periods prior to and subsequent to a particular “treatment” or change in a variable. The analyst assesses the causal relationship by comparing the degree to which there is observed variation on the outcome across the identified moment of variation on the hypothesized explanatory factor.

5. See Abbott (1997) for an in-depth discussion of turning points. See Aminzade (1992), Griffin (1993), and Pierson (2000) for important discussions of conceptual problems related to time.

The critical question of period analysis is which periods should be compared. Not all historical observations are created equal, and some provide much more analytical insight than others. To draw causal inferences, the analyst deliberately identifies moments of variation on various explanatory variables. Moments of institutional change or change in any of the possible explanatory variables provide opportunities to evaluate the impact of such variation on the dependent variable.<sup>6</sup> Moreover, in studies comparing the historical development of more than one place, analysts attempt to select comparable moments of variation on explanatory factors.

Unlike in statistical analyses in which estimates of causal effects are presented as numerical parameter coefficients, comparative historical analysis is generally presented in terms of narrative descriptions of the nature and magnitude of effects, often through the specification of counterfactuals.<sup>7</sup> As in statistical analyses, comparative historical estimates of institutional effects tend to be stated probabilistically, as we could never be certain of how outcomes would have unfolded under different circumstances. The value added of comparative historical analysis is to provide assessments, with as much specificity as possible, of the types and magnitudes of change that are caused by particular institutions and other influences.

I identify four sets of periodization strategies that have been deployed in comparative historical analyses: the institutional origins strategy, the institutional change strategy, the exogenous shock strategy, and the rival causes strategy. As summarized in Table 1, each periodization strategy involves the identification of important moments of variation on explanatory variables to test their influence on the dependent variable, measured across places and time periods. Unlike in statistical analyses of pooled time-series data, which estimate the theoretical model in one integrated analysis of all available data, periodization strategies are used iteratively and opportunistically. Analysts sort through a rich historical database of information on the relevant variables with observations that span a set of countries and a relatively long period of time. The final presentation of most comparative historical analyses is rather lengthy because multiple analyses are conducted to parse out various aspects of the underlying model, and this requires more extensive explication. Moreover, because of the vast historical scope and macro-level concepts that are employed, questions of measurement and unit heterogeneity are particularly problematic, and to maintain transparency in scoring the cases, scholars gen-

6. As King, Keohane, and Verba (1994) argue, this is a sound strategy for research design.

7. When possible, statistical analyses of time-series data may be usefully employed as a complement to the more qualitative, historical analysis. For an example, see Swank (1996).

Table 1  
*Four Strategies for Periodization in Historical Institutional Analysis*

Steps	Identify Moments for All Country Cases	Identify Controls (necessary conditions for making causal inferences)	Specify Institutional Hypothesis	Estimate Causal Effect Through Over-Time Comparison
Institutional origins strategy	Institutional origination	Over-time change in background variables should be similar across cases.	Change in outcome predicted to be different across cases with different institutions, and similar across cases with similar institutions.	Measure change in outcomes relative to institutional variation across cases.
Institutional change strategy	Institutional change	Over-time change in background variables should be similar across cases.	Outcomes predicted to converge across cases of institutional convergence and to diverge across cases of institutional divergence.	Measure divergence or convergence of outcomes across cases relative to institutional convergence or divergence.
Exogenous shock strategy	Major change or shocks in background explanatory variable(s)	There should be no institutional change within cases, no change in rival explanatory variables, and similar shocks across cases.	Outcome dynamics predicted to be affected by shocks but in nationally distinctive ways.	Measure change in outcomes relative to institutional variation across cases.
Rival causes strategy	Major change or shocks in rival explanatory variable(s)	There should be no institutional change within cases and no change in background explanatory variables.	No noticeable change predicted in outcomes within or across cases.	Measure change in outcomes relative to shock in rival explanatory factor(s) within and across cases.

erally feel compelled to provide substantial evidence to defend their classification of cases.

Each periodization strategy is associated with certain conditions and limitations for making causal inferences, which is why the repeated deployment of all four strategies is a more powerful basis for exploring an HI model than the use of just a single strategy on its own. Detail within the historical database provides opportunities for the analyst to test various claims. Most important, each of the strategies is associated with a falsifiable hypothesis—

and to the extent that they “pass” multiple hypothesis tests, the central theoretical argument emerges as a relatively robust model of political life. If other rival explanatory factors provide greater explanatory power, the HI model should either be respecified or rejected.

### **Institutional Origins Strategy**

The most important starting point for estimating the impact of institutions on outcomes is through the comparison of periods prior to and subsequent to the creation of the relevant political institutions central to the HI argument. Simply put, if HI scholars want to claim that a particular set of institutions matters for a particular set of outcomes, it is necessary to show that such outcomes were not already in place prior to the establishment of such institutions. Thus the identification of moments of institutional origination is a preliminary point of departure for the analysis. If cross-national differences in the outcome are already evident prior to the moment of institutional origination, it would be hard to infer that institutions are responsible for such patterns of variation.<sup>8</sup>

To assess the impact of cross-national institutional variation, the analyst must control for the levels of over-time variation in various background variables across the periods of institutional origination. If these conditions are not met, it may be difficult to assess the extent to which variations in institutions were responsible for cross-place variations or if different magnitudes of variation on other explanatory variables were actually responsible for that variation. The analyst may incorrectly accept the institutional hypothesis if the moment of institutional origin coincides with other factors that really caused variations on the outcome. In other words, because the historical moment seems to mark a disjuncture on the outcome, the analyst may falsely assume that the institution was responsible for that change when some other factor was truly the causal influence. Because institutional origins are generally associated with change in other important factors in political and economic development, the potential for these errors can be high. Satisfaction of this hypothesis test should be seen as a necessary but far from sufficient condition for establishing the robustness of the theory.

Ultimately, the goal of this analysis is to estimate the impact of institutional variations on outcomes. Again, it is worth repeating that this effect cannot be understood as wholly exogenous to other explanatory variables such as

8. For an alternative formulation of a similar concern in institutional analysis, see R. B. Collier and Collier (1991), in particular, the discussion of comparing the legacy with the antecedent system. They caution against the misinterpretation of apparent disjunctures and apparent continuities.

the process of industrialization. Yet the institutional impact can be measured by comparing the quality and magnitude of change on the dependent variable from postinstitutional to preinstitutional periods across countries in which patterns of change in other background variables are broadly similar.

### **Institutional Change Strategy**

A second strategy involves periodization according to moments of institutional change. In this case, the turning point dividing periods is the moment at which existing institutions are reconfigured in concrete ways: For example, when constitutions are revised, electoral rules are changed or technological innovations render certain financial institutions obsolete. By comparing outcomes across such historical thresholds, the analyst can explore the ongoing explanatory power of an institution as a mechanism of reproduction for the outcome under consideration. Of course, for many studies, no such moment may exist within the historical record, and this strategy cannot be employed.

The logic of inference and the potential pitfalls of the analysis are similar to those specified in the institutional origins strategy. When comparing across cases, if institutional change produces greater institutional convergence (or divergence), we should expect that—if the other explanatory variables are at similar values—there will be greater convergence (divergence) on the dependent variable. To make causal inferences, it is necessary to control for other important exogenous shocks that may have occurred contemporaneously with periods of institutional change.

Of course, it may be unreasonable to expect that institutional convergence will produce complete convergence on the outcome in question. Institutional change at one level may not influence change in secondary institutions that developed in response to the major (explanatory) institution for quite some time. Moreover, because outcomes are heavily influenced by the past, they tend to change more at the margins than with complete disjunctures. The question that the analyst must answer is whether the trajectory of change becomes more similar to or different from the period prior to the institutional change. If the magnitude of the difference on the outcome across places persists following moments of institutional change, the institutional hypothesis can be rejected.

### **Exogenous Shock Strategy**

In the first two strategies, analysts attempt to control for exogenous shocks, but it is also possible to gain analytic leverage from looking closely at

major historical disjunctures in noninstitutional background variables. A central aspect of most HI arguments is the claim that major changes in certain critical background explanatory factors will affect change in the dependent variable but that similar causal forces will produce different outcomes across institutional environments. The exogenous shock strategy is the most powerful basis for inference when the explanatory variable is a process or influence that transcends the borders of the particular places under investigation—for example, major wars or the propagation of new ideas. In such situations, the analyst can explore whether the over-time variation produces over-time changes on the dependent variable and whether its effect is different depending on the political institution in place.

By comparing the relative change in outcomes across periods, the analyst estimates the impact of institutions as intermediating factors in a relationship between the explanatory variable(s) and the outcome. To make inferences, the direction and magnitude of over-time variation on this explanatory variable must be similar across places. Moreover, to assess their mediating influence on the explanatory variables, institutions must vary across countries but not within countries across periods on either side of the “shock.”

If the value of change in the noninstitutional explanatory variable is not similar across country cases, it is more difficult to make strong inferences one way or another about the impact of institutions on the outcome. The analyst cannot assess whether the different outcomes were the result of different institutional settings or simply the different values on the explanatory variables. When a noninstitutional factor varies in one country and not another during a particular period of time, this does not mean the institutional hypothesis should be rejected. Such difference is merely a product of a nonexperimental setting, and the analyst can make inferences only with respect to periods with adequate control for such factors. Of course, the occurrence of a particularly important change or event affecting the outcome in question in one place and not the other may impede the ability to make causal inferences in subsequent periods.

This strategy is most useful when the exogenous shock is truly a shock in the sense that a dramatic change occurs in a well-defined manner during a well-defined and relatively short period of time. Lengthier processes that get conceptualized as shocks may indeed have important causal implications, but the relationship between such processes and the outcomes under investigation are much more difficult to evaluate because other unrelated factors may also influence the outcome during such periods.

### **Rival Causes Strategy**

A final periodization strategy considers the impact of factors associated with rival explanations. The analyst identifies moments of variation on such variables through identification of major events that rival explanations would predict to influence change on the outcome under investigation.

In this case, the HI analyst hypothesizes that these factors do not cause important change. Thus the most powerful use of this strategy is when significant exogenous shocks are identified and relative continuity can be demonstrated in the outcome under investigation. Too often, HI and other comparative historical analysts rely almost exclusively on the static deployment of Mill's (1843/1961) methods of difference—that is, matching on variables across places at a moment in time—to address rival explanations. Although this is a valuable strategy, it cannot address important characteristics that differ across places and may be hypothesized to explain these variations. For example, the potential explanatory value of varied sequencing and patterns of change in development across places is often a critical consideration. The timing of industrialization across Organization for Economic Cooperation and Development countries was quite distinctive across places, and the simple invocation of level of development as a control ignores the potential impact of timing on the outcome in question.

Of course, the exploration of the impact of rival factors may lead to their inclusion in the model. If similar levels of variation on the rival factor produce varied levels of change on the outcome, it may be possible that those factors should be included in the model as institutionally intermediated factors. If they produce similar levels of variation on the outcome, it is possible that they should be included as nonintermediated variables in the model. If over-time variation on rival explanatory factors is not associated with over-time variation on the outcome, the rival hypothesis can be rejected. As in the exogenous shock strategy, the analyst can only make inferences about the impact of hypothesized factors during periods of relative stability on the values of other institutional and explanatory variables.

### **EXAMPLES OF HI RESEARCH**

Having specified the comparative historical strategies used for making causal inferences about HI models or explanations, it becomes possible to discuss the use of such strategies within published examples of HI scholar-

ship. Steinmo's (1993) study of tax policy and Immergut's (1992) study of health policy are both book-length projects, and Rothstein's (1995) study of union strength is a chapter-length contribution to an edited volume on historical institutionalism. Together, these studies demonstrate a shared approach to the simultaneous deployment of cross-sectional and periodization strategies with which they draw causal inferences with high levels of confidence.

Through a review of these works, it is indeed possible to see that a common theoretical model undergirds this brand of scholarship and that very similar methods have been used to develop the causal arguments. In comparing the works, it is possible to highlight some of the trade-offs associated with different applications of these strategies, including why a mix of strategies is necessary in the wake of complex and "messy" historical data that are a far cry from an experimental setting and why geographic breadth may not be more desirable for making causal inferences than historical depth. Unfortunately, in all cases, these analysts tend not to be as explicit as they could be about the types of over-time comparisons that serve as the guiding logic for making causal inferences, and skeptical critics may see them as "storytelling" when, in fact, these analyses are characterized by structured comparisons and involve the identification of specific variables, relationships, and causal inferences.

#### STEINMO (1993): *TAXATION AND DEMOCRACY*

Steinmo's (1993) study, *Taxation and Democracy*, employs an HI argument to account for divergent patterns of tax policy. His work is notable for its heavy and effective reliance on inferences derived from cross-period analyses in his exploration of why the tax policies of the United States, Sweden, and the United Kingdom are so different from one another. Steinmo begins by recognizing significant differences in the contemporary tax policy mixes and tax revenues of various Organization for Economic Cooperation and Development countries, particularly the United States, the United Kingdom, and Sweden. He motivates his puzzle by employing Mill's (1843/1961) methods of difference, pattern matching on several variables that might explain differences in tax systems. Because these countries do not differ in terms of their level of development, regime type, or societal desire for public goods—they are all rich democracies with large demands for public goods—none of these factors can account for differences on the dependent variable. Rather, employing HI theory, Steinmo identifies political institutions as the key explanatory variable and argues that when this variable intersects with a series of other background variables, including industrialization, ideas, technology, social structure, and international shocks, they combine to produce

nationally distinctive policy outcomes. Simply by matching scores on several control variables for the three countries and by identifying covariation between institutional forms and tax policies, he generates a plausible hypothesis concerning the relationship between taxation and varied forms of democracy.

Because this static analysis of only three cases provides a weak basis for causal inference, the institutional hypothesis is considered and developed through an analysis of the historical development of tax policy in the three countries. As summarized in Table 2, various cross-period comparisons are employed to test the idea that institutions mediated similar pressures in different ways, producing varied outcomes.

Steinmo begins by employing the institutional origins strategy to rule out the hypothesis that the tax systems were merely remnants of some predemocratic legacy or some long-standing cultural practice associated with each society by demonstrating the amazing transformation in tax systems that took place between the 19th and 20th centuries. Taxation in all three countries in the 19th century included “a wide assortment of levies, fees, duties, excise taxes and charges” (Steinmo, 1993, p. 63). In other words, the score for tax policy was quite similar across the three cases in premodern times, and this finding allows Steinmo to rule out the hypothesis that modern tax policy was simply a holdover from the past.<sup>9</sup>

Steinmo found it was possible to employ the institutional change strategy with respect to the Swedish case when he identified an important change in that country’s political institutions. A 1970s constitutional reform resulted in the abolition of the two-chamber system—a critical feature of the Swedish political institution that had produced a model tax system in the 1950s and 1960s. He “selected” on this variation and points out that subsequent tax reform in that country—although still partially influenced by the old patterns—came to be characterized by new patterns of policy making that were the result of this constitutional change. Specifically, he points out that although this change led to an historically unique bourgeois majority in the Riksdag, it was the constitutional change itself that had much greater ramifications in the long term. Among other things, the change brought about a more confrontational and aggressive style to politics that had the effect of generating a much more complicated and onerous tax system. Following the theoretical model presented in Equations 1 and 2, in the Swedish case,

9. In the Swedish case, however, Steinmo (1993) does not quite reconcile his hypothesis about the impact of institutions with his own observation that early 20th century policy making was “dominated by a bureaucratic oligarchy largely derived from the traditional landed aristocracy” (Steinmo, 1993, p. 63). A clearer statement relating postwar taxation to this early period would have suggested some of the limits of the institutional argument.

Table 2  
*Examples of Periodization Strategies in Recent Works of Historical Institutional Analysis*

	Sven Steinmo (1993) <i>Taxation and Democracy</i>	Ellen Immergut (1992) <i>Health Politics</i>	Bo Rothstein (1995) <i>Labor Market Institutions and Working-Class Strengths</i>
Institutional origins strategy	Demonstrates that taxation structures were largely similar in the premodern era, suggesting that preinstitutional legacies cannot explain variation.	Demonstrates that health outcomes were largely similar in the premodern era, suggesting that preinstitutional legacies cannot explain variation.	Demonstrates that union densities did not influence selection of insurance scheme during the early part of the century, confirming the causal order of the argument.
Institutional change strategy	Demonstrates that institutional change in Sweden leads to change in the Swedish tax system.	Demonstrates that institutional change in France leads to change in the French tax system. In particular, increasing institutional convergence with Swedish political institutions was associated with convergence on health policy.	Not used
Exogenous shock strategy	Demonstrates that World War II influenced tax outcomes in all countries and that nationally distinctive institutions were associated with the adoption of nationally distinctive policies.	Demonstrates that World War II influenced health policy formation in all countries and that nationally distinctive institutions were associated with the adoption of nationally distinctive policies.	Not used
Rival causes strategy	Demonstrates that a shift in the partisan orientation of the U.S. president does not lead to significant shifts in the U.S. tax system.	Demonstrates that a shift in the composition of the Swiss parliament does not lead to significant shift in health policy.	Not used

changes in  $I$  covary with changes in  $Y$  across periods. Although these variables also covary with changes in  $\mathbf{X}$ , similar change in the vector of background variables did not effect change on the outcome in the other two cases, suggesting that institutional change was the key causal factor. Moreover, he

argues that as a result of the institutional change, new political dynamics emerged in Sweden, producing a shift in the taxation outcome. Sweden went from being a country with an average tax burden as a share of gross domestic product to being the most heavily taxed nation in the world, with many of the complicating features that characterized the American tax system. This evidence provides a very solid confirmation of the hypothesis associated with the institutional change strategy.

Perhaps the strongest use of periodization strategies as a basis for causal inference is Steinmo's use of the exogenous shock strategy, identifying multiple turning points associated with his key background variables. Ideas (about the need for democratization, demands for greater equity, and Keynesian economic policy), structural change in the global economy (high growth of the 1950s and 1960s, the oil crisis, and the globalization of capital), and other external factors (notably, the two World Wars) were the basis for tax reform in all three countries, but the response to each was different. This method of repeated evaluation lends solid support to the argument. For example, he shows that although the "premodern" tax systems of the three countries were basically similar, all of them changed along with the process of modernization. The advent of Keynesian theory was also mediated in different ways. When certain background variables are not at similar values across cases—for example, Sweden's war neutrality—he recognizes that the analyst simply cannot control for all variations in a nonexperimental setting. He points out that this difference did have some influence on cross-national variation while maintaining that even for nonwarring powers, certain economic and other pressures were still fairly similar across countries. An in-depth knowledge of the cases allows Steinmo to assess apparent dissimilarities across country cases and to score cases appropriately.

Clearly, there are limitations to how well-specified the HI theoretical model can be. For example, it would be virtually impossible to predict a priori the range of ideas that could influence tax policy and to specify exactly how such ideas would affect different outcomes. On the other hand, the design does allow the analyst to consider the more generally stated hypothesis—that ideas can affect the formulation of policies in multiple places—but that ideas are filtered through particular domestic institutional arrangements.

Finally, Steinmo does use the rival causes periodization strategy, but this is done in a matter perhaps so subtle that it may be almost undetectable to many readers. One example is his refutation of the idea that policy is a direct product of the ideology of the party or leader in power. For example, he points out that in the United States, the progressive President Roosevelt was surprisingly moderate on tax policy and pushed few changes his first 3 years in office. In terms of analytical variables, he finds that observed variation in **Z**

(the vector of rival explanatory variables) does not covary with  $Y$  across periods, suggesting that  $Z$  does not have an independent causal influence on  $Y$ . Similarly, in the case of Sweden, he argues against the possible theory that the country's tax system was merely a product of socialist government interests. By pointing out that a 1976 to 1981 "bourgeois" government was no different from the socialists in its inability to resist spending demands and thus was forced to raise taxes to a similar degree, he rules out this potential hypothesis. Yet in relegating this piece of information to a parenthetical clause (p. 131), most readers are prone to miss this potentially important piece of information.

In sum, Steinmo's analytically insightful historical analysis provides enormous opportunities for making causal inferences, particularly because he deploys all of the periodization strategies. However, his lack of explicit disclosure of some of his analysis may provide a misleading portrait of undertheorized narrative.

**IMMERGUT (1992): *HEALTH POLITICS: INTERESTS AND INSTITUTIONS IN WESTERN EUROPE***

Immergut's (1992) study of national health policy demonstrates how decision-making institutions affected the development of health policies in various countries. She carries out comparative historical analysis of three West European countries and leverages a series of periodization strategies as the basis for strong causal inferences. Like Steinmo (1993), she engages her empirical puzzle with Mill's (1843/1961) method of difference: She asks why France, Sweden, and Switzerland wind up having such different state roles in the provision of health care despite similar levels of economic development, similarly strong medical professions, and similar policy proposals for health insurance. She argues that different sets of constitutions and decision-making processes—in particular, different "veto points"—ultimately explain this variation. These institutions mediate a common conflict between two actors: governments and medical associations. Her comparative historical analysis considers this hypothesis in a quite rigorous manner, allowing her to draw causal inferences far beyond the logic implied by the static comparison, which serves simply as a touchstone for the project.

Aware of the often unproblematic characterization of national models of welfare provision, which correspond to the outcomes she observes in health policy, Immergut uses cross-period analysis to establish the historically specific creation of these policies. By comparing contemporary policy mixes with those of a much earlier period, she rules out—via the institutional origins strategy—cultural or other factors that predate those institutions as the

causal factors behind contemporary, nationally distinctive policies. She shows that the health care systems of the three countries during the 1920s were in fact quite similar to one another and that all of those systems were quite different from what they would become several decades later. With this analysis, we are assured at least of the plausibility of her political-institutional explanation.

What about institutional change? Immergut identifies an important moment of change in the French case during the moment of the 1958 constitution, ushering in the Fifth Republic and a change in the powers of the French executive. As this move tended to give greater autonomy to the executive, she argues that the veto point came to resemble the Swedish case more than the Swiss case and that the associated outcome of health policy also became more like the Swedish case to a greater degree than before. This use of the institutional change strategy is reminiscent of Steinmo's (1993) and emerges as a quite powerful test of the institutional hypothesis. She also briefly considers the impact of change in Swedish political institutions as discussed by Steinmo.

Immergut also identifies several background variables that are the engines of over-time change for health care policy. Some, such as the interests of doctors and workers, are relatively consistent over time and serve as controls. Others, such as war and economic shocks, do change over time, but the extent to which these are really common pressures mediated differently by different institutional environments is somewhat difficult to establish because of the organization of the book. The factors that trigger new health care policy proposals seem to be generated more locally and at nationally distinctive moments than as the result of major external shocks or pressures. Yet she argues that over the long term, the pressures for new policies were basically similar. For example, she uses several measures of the growing strength of unions and of medical associations in all three countries to demonstrate that these factors cannot explain differences in the French and Swiss cases, in particular—although these groups are clearly important to the development of health care policy in general. Most important, she points out that unlike in Sweden, where organized labor succeeded in securing greater health coverage, in Switzerland, even despite substantial labor organization and enthusiasm for more extensive health policies after World War II, the referendum ultimately doomed those proposals to failure.

To nail down her argument, Immergut employs important within-country, over-time comparisons to rule out several rival explanations. Unfortunately, she is also not nearly as explicit about use of the rival causes strategy as she could be, but she manages to rule out several rival explanations by demonstrating the consistency of national patterns of policy making, even in the face

of changing leaderships, the emergence of new groups within society, the changing composition of legislatures, and other historically contingent factors. For example, she shows that in Switzerland, despite swift passage of an 1889 policy proposal through a parliament clearly dominated by a single party, the proposal was rejected in a referendum (p. 145). In other words, the party composition of the legislature does not explain the variations in the outcome—either within countries or across countries over time.

**ROTHSTEIN (1995): “LABOR-MARKET INSTITUTIONS AND WORKING-CLASS STRENGTH”**

Rothstein’s (1995) account of cross-national variation in worker organization reveals a contrasting mode of analysis for specifying the HI model, particularly in light of the Steinmo (1993) and Immergut (1992) studies. Rothstein’s essay places the most emphasis on cross-sectional analysis and the least emphasis on cross-period analysis. He gains additional analytical leverage by increasing the number of country cases under analysis from 3 to 18 but effectively employs only a single cross-period comparison. Given the very similar theoretical logic of his argument, it is worth considering the trade-offs associated with this alternative approach. Greater geographic breadth provides a stronger basis for claiming that the findings have general application, but less depth in the historical analysis provides a weaker basis for making causal claims.

Rothstein begins with an important empirical question, asking, “Why are some working classes more organized than others?” (p. 33). Although he does not dispute Karl Marx’s observation that class formation stems from changes in the mode of production, his basic research design questions the undifferentiated implications of that argument. Rather than narrow his analysis to a smaller group of country cases, however, he proceeds to analyze 18 cases of advanced industrial development.

As in the other studies, he employs Mill’s (1843/1961) method of difference to motivate the problem. He argues that the 18 countries are at very similar levels of development and yet are characterized by very different rates of unionization, so something else must be at work. Similarly, using two paired comparisons with a case from each of the “higher” and “lower” union density groups, he rules out a cultural explanation. Although he does not specify operational measures of the concept of culture, he makes the intuitively plausible case that Belgium and the Netherlands share a very similar culture and that Sweden and Norway are also culturally very similar but that within each of these pairs, union density varies widely, suggesting that culture is not likely an explanatory variable.

Employing HI theory, he argues that the institutionalization of unemployment insurance schemes had an important influence on union organization by providing different levels of selective incentives to encourage individuals to join. On one hand, compulsory unemployment schemes had a negative effect on unionization because these tended to take power out of the hands of unions, whereas union-administered “Ghent” systems facilitated union power and worker organization. In both cases, these institutions mediated the process of modernization and changes in the mode of production, generating different levels of working-class organization over time.

To consider this hypothesis, Rothstein uses ordinary least squares regression analysis to estimate the influence of his institutional variable as well as two other variables—the “political color” or left-right dimension of governments and the size of the work force—on his dependent variable, union density. His analysis of 18 cases provides much greater leverage for comparative static analysis than the other two studies considered in this article, and this allows him to consider these alternative explanations. Indeed, he finds that even when controlling for these other two variables, the variable measuring the type of unemployment insurance scheme in operation (scored as 1 for Ghent, 0 for compulsory, and .5 for Belgium’s mixed system) is a robust predictor of unionization rates. Rothstein’s cross-sectional analysis provides a stronger basis for making generalizable claims than does the cross-sectional analysis of the other two studies.

Rothstein does acknowledge the possibility that the observed correlation between union density and unemployment scheme may have resulted from the opposite causal relationship—in other words, that strong labor movements select Ghent systems and vice versa. To consider this possibility, he employs the institutional origins strategy and moves backward in time to assess causal order. By looking at data from 1930 for 10 of the countries, he observes that there is no correlation between the two variables. He makes the plausible argument that the selection of which system to introduce was largely a matter of historical contingency and that once in place, institutional variation had an impact on worker organization. Rothstein argues that the “effect of the Ghent system on union density seems to be considerably delayed” (p. 43), suggesting that the impact took place between 1930 and the 1980s. Given this hypothesis, perhaps more important than the actual rank orderings of countries in the 1980s is the relative change in union membership during this period. Indeed, on average, the results help to confirm Rothstein’s hypothesis. In the three Ghent systems for which Rothstein had historical data, there were net increases in worker organization of approximately 45% of the workforce. By contrast, in the seven compulsory systems, over-time change ranged from a net loss of 17% to a gain of 35%. On average,

in Ghent systems, similar long-term patterns of industrialization were associated with higher levels of union organization.

Although Rothstein's argument and evidence are initially persuasive, many questions remain because the historical records of the country cases are treated only superficially in this analysis. Rothstein does not probe further into the dynamics of building union strength over time with respect to the particular country cases. Unlike in the other two (much longer, book-length) studies, there is very little presentation of historical process, and scrutiny of these results suggests some good reason to be skeptical: Of the 11 countries for which Rothstein has comparative data, six of the countries more or less maintained their relative rank orders (Sweden, Austria, Netherlands, the United Kingdom, the United States, France). Norway significantly increased its place in the rank order, which is a disconfirming case because it had a compulsory scheme. That leaves only four cases with truly supportive results: Germany, Australia, and the Netherlands declined in their rank orders and had compulsory schemes, whereas Denmark increased its rank order and had a Ghent system. Absent a more careful historical approach, he is not able to locate periods of important increase in union organization, linking how the Ghent system facilitated this process when compared with similar countries using the compulsory system. Within his set of cases, he might have explored other potential explanatory variables in over-time comparisons. For example, by selecting on changes in governments over time, within governments, or across periods of economic boom or bust, he might have been able to make a more precise specification of this causal relationship. Moreover, he might have explored other factors, including specific changes in the mode of production, to understand what drove union organization over time and whether certain moments were more or less important for the development of union strength than others. If certain technological changes were introduced in some places and not others or at different times, these factors might have been identified as potentially powerful complementary or rival explanations. In short, more extensive historical analysis, deploying the various periodization strategies, would have provided greater analytic leverage for making causal inferences.

## CONCLUSION

By specifying a general HI model and a set of comparative historical methods that have been used to evaluate that model, this article has provided a framework for more critical evaluation of this brand of scholarship. Most

important, the article demonstrates that comparative historical analysis is not simply storytelling but rather a nuanced and iterative process that involves consideration of various implications of the theoretical model with structured comparisons drawn from a rich database of historical records.

In exploring and comparing three works of HI analysis, certain important similarities and differences were revealed that suggest a common enterprise as well as a range of options for carrying out future research. The most important similarity is a theoretical one: Although the specific institutions and outcomes differ, in all three, institutions mediate background variables such as the process of modernization to produce nationally distinctive outcomes. Second, all of the studies are motivated by contemporary problem-driven questions highlighting important differences on a variable of interest. All use a common comparative logic typically associated with Mill's (1843/1961) methods of difference and agreement to rule out rival explanations. Finally, all proceed by engaging in more elaborate comparative analyses across places and over time.

Yet there were also important differences highlighting trade-offs in the design and analysis of such research. Most important, the contrast between Steinmo's (1993) and Immergut's (1992) studies on one hand with Rothstein's (1995) on the other reveals how different emphases with respect to the analysis of the dimensions of time and space affect the reader's level of confidence in the types of inferences being made. The Rothstein study emphasizes the cross-sectional component of the analysis, generating higher levels of confidence that this institution has a generalizable relationship with the outcome under investigation, but the exact nature of the causal relationship is less clear. By contrast, the Steinmo and Immergut studies emphasize the longitudinal dimension of the analysis by looking at multiple historical episodes, if only in three countries. As a result, one has greater confidence that the particular institutions found in each of those countries actually caused the outcomes we observe in those countries, and the process by which the institutions affected the outcomes is very well specified. The shortcoming is that we have less confidence that if those institutions were to be transplanted in other countries, we would observe the same effect.

The specification of these strategies should help analysts to make more informed decisions about how to approach their own HI analysis and to gain maximum analytic leverage out of their historical research. Contrary to conventional wisdom, the addition of country cases may be less valuable than the inclusion of more nuanced historical analysis for drawing causal inferences. Indeed, the powerful deployment of multiple periodization strategies in conjunction with comparative static analysis provides a means for addressing

what is often seen as the key problem with the comparative method—namely, the inability to sort out a host of rival explanations.<sup>10</sup> By looking across both places and time, employing a range of analytical strategies, HI scholars have developed novel approaches to making strong causal inferences, and these approaches are worthy of emulation. In the future, this scholarship would benefit from a more explicit and self-conscious reporting of these strategies and methods.

10. See D. Collier (1993) for a review of this form of analysis, particularly as compared with statistical, case study, and other approaches.

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