

# The Explanatory Power of Historical Institutionalism

*Kathleen Thelen*

## 1 Introduction<sup>1</sup>

The topic I was asked to discuss concerns the explanatory power of historical institutionalism. This is a large topic; it is also one that I am only partly qualified to address, since of course it is only possible to assess the explanatory power of an historical institutionalist or any other approach in the context of the particular empirical puzzles on which it is brought to bear. And, one of the hallmarks of historical institutionalism is that it ranges over a very broad spectrum of empirical phenomena indeed – from social revolutions, to state building, to regime formation, to the development of the modern welfare state, and so on.<sup>2</sup>

Elsewhere (Thelen 1999) I have contrasted historical-institutional approaches with other approaches, including rational choice and sociological institutionalism, and so I will not rehearse those arguments here. Instead, I organize this essay around recent contributions to the literature, as well as ideas based on ongoing research. My discussion is organized into two broad sections. First, I briefly sketch out some of the distinctive features and strengths of an historical-institutional approach. And second, I offer some

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1 I would like to thank the participants in the project on Comparative Historical Analysis organized by James Mahoney and Dietrich Rueschemeyer – results of which are now forthcoming in Mahoney/Rueschemeyer (2002a). Our discussions for that project influenced my thinking deeply and in this article I draw on points that emerged from our collective endeavor. In addition, written exchanges and discussions over the last years with James Mahoney and Paul Pierson have been enormously influential in the evolution of my thinking about the issues addressed below and so I would like to single them out for separate acknowledgement here.

2 The literature is too vast to cite, but for overviews of the general contributions of historical institutionalism to the study of politics see Pierson/Skocpol (2002), Thelen (1999), Thelen/Steinmo (1992).

ideas about how historical-institutional research is contributing to a better understanding of institutional genesis, reproduction, and change. This second section also identifies some of the theoretical frontiers on which historical institutionalists could fruitfully push further.

## 2 Historical Institutionalism as an Approach

As the name suggests, historical institutionalism has something to do with history and something to do with institutions, so perhaps this is an appropriate place to start. Since the literature on varieties of institutionalism (and this line of analysis itself) is by now so pervasive, I will not focus much on the »institutional« part of the historical-institutional enterprise (but see Thelen 1999; Thelen/Steinmo 1992). However, it is certainly worth remembering that there has been a great deal of borrowing and cross-fertilization occurring across different strands of institutionalist thinking (Hall/Taylor 1996). Some historical institutionalists subscribe to a conception of institutions that is quite compatible with rational choice perspectives, in which institutions are defined as the rules of the game in a fairly restrictive sense (e.g., Immergut 1992). Others, however, lean more toward a more expansive, cultural definition of institutions, closer to that embraced by institutional sociologists, in which institutions are seen as embedded in and also reflecting particular kinds of social norms and understandings (e.g., Katzenstein 1996).

These different perspectives and the trade-offs among them have been the subject of a relatively large literature and need not detain us here. However, whichever way that historical institutionalists might lean in these debates, most of them would wish to stress that institutions are important not just in how they constrain individual choice or affect individual strategies, but also in how they affect the articulation of interests, and particularly the articulation of *collective* interests. Thus, historical institutionalists have consistently drawn attention to the way in which institutional configurations »foster the emergence of particular definitions of mutual interest« (Immergut 1998: 339), and how they also often shape political outcomes by facilitating the organization of certain groups while actively disarticulating others (e.g., Skocpol 1992). This is one of the core insights of historical-institutional scholarship, and it applies not just to the question of how institutional arrangements affect the mechanics of coalition formation, but also how they

influence the capacities of groups to recognize shared interests in the first place (e.g., Weir 1992).

The »historical« component of historical institutionalism merits perhaps a bit more comment, for this is an area in which there has been a great deal of important recent theorizing. I will limit my remarks on this to two general aspects – the emphasis in historical-institutional research on context and interaction effects, and the concern on the part of this approach with temporal processes. This discussion sets the stage for the second half of the essay on institutional genesis, reproduction, and change.

## 2.1 »Context« in Historical-Institutional Research

First, to the issue of attention to context. In a recent review of historical-institutional research, Pierson and Skocpol note that when one looks at the titles of books written by historical institutionalists, one finds that most of them contain a fairly explicit specification of the places and dates that are dealt with between the covers (Pierson/Skocpol 2002: 11). This practice signals a close attention to aspects of the empirical context that define the »scope conditions« of the propositions advanced in these studies. One reason for this is that, by and large, historical institutionalists define their research agendas in terms of empirical puzzles that emerge from observed events or comparisons. For instance: Why did the politics of the advanced industrial countries differ so much in response to the oil shock of 1973 (Katzenstein 1978)? Or: Why do some developed democracies tax and spend more than others (Steinmo 1993)? This is not the only way to go about things, of course. A fair amount of rational choice scholarship proceeds differently, and derives puzzles from situations in which observed behavior appears to deviate from what the general theory would predict: Why do unions sometimes lead workers into hopeless battles (Golden 1997)? Why would individual citizens ever volunteer for war (Levi 1997)?

Another reason for the emphasis on context in much historical-institutional research may run a bit deeper, however, and reflect a somewhat different orientation to theory building – whether more oriented toward »universal« or middle-range theory. A good deal of historical-institutional scholarship shows that the impact of institutions is often heavily mediated by features of the overarching political or historical context, a point that Charles Ragin's work has repeatedly emphasized and underscored (Ragin 1987; also Katznelson 1997). To give one simple and obvious example: A

strong professional bureaucracy in the context of a one party state occupies a very different role and has a very different impact on politics than a strong professional bureaucracy in the context of a democracy. Therefore, to answer many research questions, there are often real advantages to focusing on particular well-chosen clusters of cases that share some features while also differing on other dimensions of interest.

In some instances, comparisons will focus on cases that share some regional and/or temporal similarities, but differ on outcomes. Gregory Luebbert's 1991 book, for example, provides an explanation of the origins of liberalism, fascism, or social democracy across a range of countries in Europe in the interwar period. Luebbert articulates his theory using concepts and categories that are closely linked to the empirical cases, and it is partly the contextualized nature – and resulting specificity – of the concepts he employs that puts the theory at risk – making it easy for scholars coming after him to test his propositions against new evidence. This kind of testing and the possibility for falsification is not at all straightforward for many theories that employ highly pliable concepts formulated at great distance from the cases. At the same time, however, it should be clear that the formulation of contextualized propositions does not preclude scholars interested in similar outcomes elsewhere from adapting Luebbert's explanation to other cases. It just means that one would not expect the specific propositions to apply without revision to other historical contexts or epochs.

This more empirical and contextualized approach to theory construction is something for which historical institutionalists have been criticized, particularly by proponents of »general theory-building« (see, especially, the discussions in Green/Shapiro 1994). If we are interested in explaining, say, democracy and development, then – it is sometimes argued – we should strive to understand patterns of democratization *generally*. If we confine our analysis to explaining democratization in particular historical or regional contexts – for example, Western Europe in the nineteenth and early twentieth centuries, or Latin America in the twentieth century – the results will not cumulate into a general theory. On the contrary, the identification of context-specific variables and patterns is thought to detract from the real task of discovering general laws that hold across time and space.

In many ways, the debate about universal versus contextualized theory building may ultimately be rooted in fairly deep-seated difference in terms

of where different scholars believe the real payoff lies.<sup>3</sup> As Ian Shapiro has put it, those who strive to develop universal theory operate on the assumption that the general part of an explanation will capture a very big part of the story. Therefore, if we can identify the overarching rule, we will know a great deal about what we want to know about, say, democracy and development (to stay with the previous example). On this view, the rest is more or less idiosyncratic, and simply specifies the German, or Mexican, or Czech variant on the same basic story. I think it is safe to say that most historical institutionalists would share with Shapiro a high degree of skepticism on this point, and be more inclined to think that what you might be able to discover at the level of universal laws may be a rather small and maybe even trivial part of the story. The search for middle range theory is thus driven less by a disdain for theory than the conviction that deeper understanding of causal relationships (i.e., good theory) can often be achieved through a more intense and focused examination of a number of carefully selected cases.

Beyond this, and as a number of scholars have pointed out, almost all research strategies involve some kind of trade-off, and historical institutionalists are generally willing to sacrifice some generality in terms of the propositions they advance in exchange for the gains that they make in other areas. Prominent among these is a higher degree of conceptual validity (see especially Collier 1998; Coppedge 1999; also Locke/Thelen 1995). Proximity to the empirical cases under investigation – a hallmark of historical-institutional research – has the distinct advantage of ensuring that the concepts with which the analyst is working capture what he or she is trying to get at, and, especially, that they capture the same thing across all cases under consideration. This is important for avoiding problems of causal heterogeneity when very diverse cases are compared (see also Mahoney/Rueschemeyer 2002b: 6–8).

Moreover, most historical institutionalists would strongly contest the idea that middle range theories developed for particular historically or regionally bounded phenomena do not »add up« to anything.<sup>4</sup> Indeed, historical-institutional research has generated a very lively research tradition in many

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3 This point comes from remarks made by Ian Shapiro at a workshop on »Political Science: The State of the Discipline,« Washington DC, December 8–9, 2000.

4 This challenge is taken up specifically in Mahoney and Rueschemeyer's volume (Mahoney/Rueschemeyer 2002b); see the chapters by Mahoney, Goldstone, and Amenta for summaries of the findings of cumulative research in the historical-institutional tradition on democratization, revolutions, and social policy.

areas, which has also resulted in a very considerable accumulation of knowledge across studies undertaken by scholars based on different historical periods and/or sets of countries (Mahoney/Rueschemeyer 2002a; Pierson/Skocpol 2002). To take one example, Barrington Moore's analysis of democracy and development inspired a number of studies from an historical-institutional perspective that in the meantime have taken us a great distance in specifying the conditions under which the bourgeoisie has been a liberalizing force, or not. Since Moore's classic study, there has developed a large literature that explores these very issues across a wide range of historical and contemporary cases (ranging from Luebbert's work on Europe between the two world wars, to Rueschemeyer, Stephens and Stephens' and Ruth Collier's studies encompassing a wider range of country cases and a longer historical time span). As a result of this scholarship, we now know a huge amount about the relationship between class politics and democratization and the conditions under which the bourgeoisie is more likely to promote or impede political liberalization.

It may ultimately be more fruitful to think of the different approaches to theory building cited above as complementary rather than competing. Comparing the results of empirical research based on different perspectives can be very instructive since discrepancies between findings will likely lead scholars to problems (in the conceptual or causal framework) that can then lead to refinements and corrections (on this point, see also Zysman 1994: 277). Constructive engagement with the »other side« can lead to better results, for example, as historical institutionalists move from defining the scope conditions for their theories in terms of empirical time and place to more abstract formulations of the conditions under which factor  $x$  is seen to lead to outcome  $y$ , or as proponents of universal theorizing move away from broad but often somewhat vacuous general propositions to more precise formulations of causal relationships that can then be subjected to further empirical research.

## 2.2 »Time« in Historical-Institutional Research

The second point about the »history« part of historical institutionalism has to do with temporal processes, a subject that has been explored in detail by Aminzade, Abbott, and, more recently, Pierson (e.g., Abbott 1990; Aminzade 1992; Pierson 2001). Historical institutionalists, both the classics and contemporary scholars, have always attached a great deal of importance to is-

sues of sequencing and timing in the analysis of political processes (e.g., Ertman 1997; Gerschenkron 1962; Gould 1999; Lipset/Rokkan 1968; Shefter 1977). In addition, a large literature on »critical junctures« is precisely concerned with the interaction effects among different processes as they unfold over time, and often as they unfold differently in different contexts (e.g., Collier/Collier 1991).

The attention to sequencing in historical-institutional research is partly motivated by the old truism that in order to establish causality you have to demonstrate not just a correlation between two variables, but also provide some mechanism or theoretical account showing why this linkage exists (Rueschemeyer/Stephens 1997). Clearly, it is important to establish that the hypothesized cause actually precedes the effect. This is why, as Rueschemeyer, Stephens and Stephens once put it, »causal analysis is inherently sequence analysis« (Rueschemeyer/Stephens/Stephens 1992: 4).

However, beyond that, the emphasis on timing and sequencing in historical institutional research is also motivated by the insight, borne out in a number of studies, that *when* things happen, or *the order* in which different processes unfold, can itself be an extremely important part of the causal story (Pierson 2000c). Here there is an important difference worth flagging, between cross-sectional statistical analysis that see cases as receptacles of values on particular variables versus an approach more attuned to process and temporality. The analogy that I find most memorable, from Paul Pierson, is a cooking metaphor. Cross-sectional statistical analysis is frequently based on the assumption that it does not matter in what order the different ingredients were introduced into the pot; the analyst is simply measuring for their relative presence or absence at some point in time. Historical-institutional research, by contrast, has shown that in many cases *the order* in which you put the ingredients together can produce distinctive dishes.

An example is Shefter's classic account of the origins of patronage politics (Shefter 1977). Shefter's argument depends on a logic of sequencing; the question is when – in the context of political liberalization – does a professional bureaucracy emerge? The way in which the dual processes of democratization and bureaucratization interacted in nineteenth century Europe determined whether party competition came to be organized around patronage politics or programmatic appeals – an outcome that mattered massively to the way in which citizens were incorporated into the polity, and that shaped subsequent political dynamics in these countries in very fundamental and enduring ways.

A more contemporary example comes out of Tulia Falleti's study of the current wave of decentralization in Latin America (Falleti 2002). Many scholars have asserted or simply assumed that decentralization promotes local autonomy and accountability and, as such, enhances economic efficiency and democracy. However, Falleti separates out different aspects of decentralization – distinguishing administrative, fiscal, and political decentralization – and she finds that the sequence in which these various processes take place has an enormous impact on the *end point* that countries finally achieve. Depending on the sequence of its component parts, decentralization can in fact lead to *less* local autonomy and *less* democratic accountability.

In these cases and many others in the historical-institutional tradition, the point is not just that we have to pay attention to sequencing in order to establish the validity of particular causal claims; it is that sequencing itself is actually doing some of the explanatory work. In this context it is perhaps worth noting that issues of sequencing also play a prominent role in some rational choice analyses as well. Scholarship in the game-theoretic tradition has shown that the sequence of moves that actors are allowed to make in a particular game can lead to radically different outcomes. That can be very important, of course, but the emphasis in these analyses is different from that in the historical-institutional scholarship cited above, where the analysis focuses not so much on the order of moves in a particular, more or less well defined game, but often, instead, the order in which various games get played.

### 3 Institutional Genesis, Reproduction and Change<sup>5</sup>

This last issue of temporality leads directly into the second major theme of this paper, namely historical-institutional approaches to institutional creation, reproduction, and change. One of the features that distinguishes historical institutionalism from rational choice institutionalism has to do with the relative centrality of »equilibrium order« versus »historical process« in the analysis of institutional stability and change (Thelen 1999). Rational-choice institutionalists often view institutions in terms of their role as coor-

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5 This section draws on much more extended treatments of these subjects in Thelen (1999, 2002).



dinating mechanisms that sustain a particular equilibrium among individual actors (and thus »hold together« a particular pattern of politics). By contrast, historical institutionalists see institutions as the legacy of concrete historical processes, and as a result, this body of work has produced a distinctive set of answers to questions of institutional origins, stability, and change.

On the issue of *institutional origins*, historical-institutional research has provided an important corrective to more functionalist perspectives. By tracing the genesis of particular institutions historically, studies have made clear how spurious it often is to simply »read« the origins of institutional arrangements off the functions they currently perform (Pierson 2000b; Thelen 1999, 2001). The focus in a significant number of historical-institutional analyses on »critical junctures« – turning points that are shown to have established important institutional parameters that subsequently shape what is politically possible, even conceivable – have illuminated aspects of political life that do not emerge through other sorts of analytic strategies or points of departure.

Another area in which historical-institutional research has made an important contribution is in our understanding of *institutional stability or continuity* (for an extended discussion, see also Thelen 1999: 392–396). A number of authors have explored the processes of »positive feedback« that account for the stable reproduction of some institutional arrangements over time (see, especially, Pierson 1993, 2000a; Skocpol 1992). As a result of this work, we have come some considerable distance since early formulations of the problem of institutional continuity that saw particular configurations as the »frozen residue« of critical junctures, or as »sticky« legacies of previous political battles. These previous conceptions were always problematic because they suggested that things stand still, when in fact we know intuitively that institutions inherited from the past must adapt to changes in the political and social environment in order to survive over long stretches of time. One of the most important contributions of the literature on feedback effects and increasing returns has been to capture the dynamic processes through which institutions are reproduced over time.

These insights are important not just for understanding institutional stability, but also for providing insights into *institutional change*. A good deal of thinking on institutional change falls back, explicitly or implicitly, on a »punctuated equilibrium« model that suggests that institutions either persist or they break down as a result of some exogenous shock or environmental shift (Krasner 1988). We can certainly find instances in which this kind of reasoning applies. For example, Barry Weingast suggests that the break-

down of the »balance rule« that held together a particular pattern of politics in antebellum America was brought about by demographic changes that favored the North (Weingast 1998).

Of course, the context in which institutions are embedded – demographic, political, economic, sociological – is always shifting in some way. But we also know that not every environmental shift is destabilizing, and not every exogenous »shock« brings about institutional breakdown. Therefore, in order to know which exogenous events or processes are likely to be *politically consequential* for particular institutions, we need to know something about what is sustaining these institutions in the first place. Institutions rest on a set of ideational and material foundations that, if shaken, open possibilities for change. But different institutions rest on different foundations, and so the processes that are likely to disrupt them will also be different, though predictable.

An example from the literature on comparative welfare states can illustrate. Esping-Andersen's (1990) three models – social democratic, conservative corporatist, and liberal welfare systems – not only rest on different levels of support (from broad and diffuse to narrow and weak) but also rely on different mechanisms of reproduction, and they therefore are differently affected by specific other »external« changes and trends. For instance, changes in gender relations and family structures are likely to reinforce elements of the universalistic and liberal welfare states (which both, though in very different ways, support a high level of labor-force participation by women), but these changes create new frictions and contradictions for conservative welfare states, which are premised on the single-breadwinner model of the family. In other words, we might well expect a (politically consequential) collision between changing gender roles and welfare state development, but only in the conservative welfare states.

The general point is that the analysis of institutional stability and change are necessarily linked in important ways, because the kinds of vulnerabilities and openings that particular institutional configurations offer (in the face of specific exogenous events or forces) depend critically on the particular mechanisms of reproduction that sustain them.

### 3.1 Beyond Punctuated Equilibrium

Finally, historical-institutional research has also begun to bring fresh insights into the different ways that institutions evolve in more incremental ways over time. Punctuated equilibrium models suggest that institutional arrangements either persist or they break down. But this is often not what we find, empirically. What we instead often see is, on the one hand, a remarkable resilience of some institutional arrangements even in the face of huge historic breaks, and, on the other hand, ongoing subtle shifts beneath the surface of apparently stable formal institutions that, over time, can completely redefine the functions and political purposes they serve.

For example, some of the core organizational and institutional features of the German political economy (for example, certain aspects of the system of interest intermediation) can be traced back very far into German history (e.g., Manow 2001; Thelen 2001; Zeitlin 2000). This means that these institutions have survived some rather huge disjunctures in the twentieth century, including defeat in two world wars, fascism, and labor incorporation. In such cases, it is not at all obvious – especially against the backdrop of a strong punctuated equilibrium model – how institutions created at some critical juncture in the sometimes quite distant past actually *make it to the present*, given the magnitude of some of the intervening events and developments.

Conversely, however, what we also find if we look at particular empirical cases is that even »sticky« institutions that persist over long stretches of time undergo subtle but very significant changes in terms of their form and functions. This is a quite different problem, but one that is equally knotty for punctuated equilibrium models that are premised on a more or less dichotomous view of institutional stability versus institutional breakdown. One example of this second point is the United States Supreme Court, which has of course at some formal level not changed very much at all over the course of the last century, but which has undergone a profound transformation in terms of its role within American politics; from a much more minimal one in the nineteenth century (presiding over issues of federalism and presidential power, also some commerce issues) to an enormously important role in the twentieth century on a whole range of issues concerning civil rights and individual liberties. Anyone interested in the way in which this institution shapes political outcomes in the United States can scarcely afford to ignore such changes.

In other words, from the perspective of the dominant punctuated equilibrium model, there often seems to be *too much continuity* through putative break points in history and *too much change* beneath the surface of apparently stable formal institutional arrangements. A growing number of works have begun to address these issues head-on, by moving beyond sterile definitional debates (how to define path dependence, for example) to distinguishing between the mechanisms of reproduction and the logic of change at work behind particular cases of institutional evolution. If successful, this will shift the debate (fruitfully, in my view) beyond the issue of whether particular cases conform to a strict or loose version of path dependence toward the more fruitful questions of distinguishing what is staying the same and what is changing, and of producing insights into different modes of institutional change.

### 3.2 Modes of Institutional Change

For understanding the logic of institutional reproduction, the literature on increasing returns and positive feedback continues to be a source of important insights. However, understanding the logic of change requires new concepts and analytic tools. In this essay, I can only allude to what in my view represent some of the most promising new leads in this area, lifting out of some of this work general concepts and insights that may constitute fruitful ways forward in understanding such processes (but see Thelen 2000, 2001, 2002).

A first set of ideas I take from Eric Schickler's work on the United States Congress, where he writes about institutional change through a process that he calls *institutional layering* (Schickler 2001). The concept of layering provides a way of thinking about institutional evolution that combines some elements of increasing returns and even »lock in« with elements of institutional innovation. Examining how congressional institutions have evolved over time, Schickler argues that institutional innovators often have to work around some institutional features that are locked in, but they can add on other elements in ways that do not just reproduce or extend the old institutions, but actually *alter the overall trajectory*. That is to say, institutions do not change abruptly, or »jump tracks« but nor is it the case that every move within a given stable institutional configuration operates to reinforce the previous trajectory, or even push at all in the same direction, as in increasing returns arguments.

The second concept that might provide some leverage on typical ways in which institutions change is *institutional conversion*.<sup>6</sup> The idea of conversion draws attention to the idea that institutions that were forged at one historical juncture, and thus as the product of one particular set of conflicts and interests, can in fact undergo a kind of transformation as they get redirected to new ends. One way that this can occur is through the inclusion of new groups whose participation was not anticipated at the time the institutions were created. In many such cases, newly incorporated actors do not simply adapt to the prevailing system and contribute to its reproduction, as in increasing returns. Rather, their very participation alters the trajectory of the system in important ways. One example, again from the development of the German political economy, is works councils, which were transformed through the incorporation of labor from instruments of employer paternalism to an institution that reflected but also substantially shored up labor strength (Thelen 1991). Other examples can be drawn from the broader literature on political economy, for example, institutional arrangements developed under wartime conditions in some countries that survive through their conversion to peacetime purposes (e.g., Shonfeld 1969: 179).

Focusing on these kinds of processes can be an important complement to the powerful but also (when used precisely) limited concept of increasing returns, and together they can yield a more nuanced sense of how developments in the past shape the strategies and possibilities of actors at later points. As such, these concepts (and these two by no means exhaust all the possibilities) may provide some fruitful ways forward as we attempt to gain a better understanding of institutional evolution and change.

#### 4 Final Thoughts on the Contributions of Historical Institutionalism in Political Analysis

The debates on the relative merits of different approaches to the study of politics may be subsiding and giving way to a more constructive mutual engagement process that taps into the relative strengths of different modes of analysis based on the kinds of empirical puzzles scholars are trying to solve. Thus, for example, a number of authors have suggested that rational choice

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6 The term was suggested to me by Wolfgang Streeck.

institutionalism applies best to understanding the strategic interaction of individuals in the context of specific, well established and well known rules and parameters (e.g., Bates 1997; Geddes 1995). By contrast to this, the strength of historical-institutional approaches is precisely in the leverage it provides on understanding *configurations of institutions* (Katzenelson 1997) and over much *longer stretches of time* (Pierson 2001). Historical institutionalism is concerned not just with how a particular set of rules affects the strategic orientation of individual actors and their interactions, but also with the broader issue of the ways in which institutional configurations define what Theda Skocpol has termed »fields of action« that have a very broad influence not just on the strategies of individual players but on the identities of actors and the networks that define their relations to each other.

Pierson and Skocpol have recently criticized rational choice perspectives in the context of American politics for focusing on ever-narrower time frames and institutions. As they put it,

in certain areas, arguably, instead of the intellectual problems faced by rational choice getting bigger, the universe of politics deemed as suitable for scrutiny gets redefined in ever more diminutive terms: the study of American politics becomes the study of Congress ..., the study of comparative politics becomes the study of parliaments and government coalitions.

(Pierson/Skocpol 2002: 7)

This narrowing of focus may not necessarily reflect anything inherent in the rational choice framework itself. However, against this backdrop perhaps the most important contribution that historical institutionalism has made and can continue to make is to keep our attention focused on the broader picture – large, substantively compelling empirical puzzles and the longer term processes that shape the political context in which contemporary politics are made (Skocpol 2002).

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