

HISTORICAL INSTITUTIONALISM IN CONTEMPORARY POLITICAL SCIENCE

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Like the character in Moliere's play who spoke prose all his life without knowing it, contemporary political scientists are familiar with leading examples of historical institutionalist research without necessarily realizing that they exemplify a coherent genre—much as do works in the other two major research approaches practiced in empirical political science, survey-based behavioralism and rational choice modeling. Historical institutionalists analyze organizational configurations where others look at particular settings in isolation; and they pay attention to critical junctures and long-term processes where others look only at slices of time or short-term maneuvers. Researching important issues in this way, historical institutionalists make visible and understandable the overarching contexts and interacting processes that shape and reshape states, politics, and public policymaking.

Stephen Skowronek's The Politics Presidents Make (1997), for example, reveals recurrent cycles in the nature and success of presidential leadership throughout U.S. history. Another long-term study in American politics, John Mark Hansen's Gaining Access: Congress and the Farm Lobby, 1919-1981 (1991), develops a model of interest group interaction with government and uses it to explain the emergence, persistence, and ultimate eclipse of the national influence of national farmers' associations. Ranging across nations as well as time, Peter A. Hall's 1986 book Governing the Economy: The Politics of State Intervention in Britain and France explains how institutions and organizations intersect to shape not just the policies of governments but also the strategies and alliances of interest groups and public intellectuals. Painting on even grander canvases, Shaping the Political Arena: Critical Junctures, the Labor Movement, and Regime Dynamics in Latin America by Ruth Berins Collier's and David Collier (1991) and Birth of the Leviathan: Building States and Regimes in Early Modern Europe by Thomas Ertman (1997) explain regime dynamics and the varied formation of modern national states. And XXXX [need an IR example: Beth Simmons first book?]. The foregoing, moreover, are but a few of many possible citations, for recent historical institutionalist studies have cumulated to provide wideranging as well as causally precise understandings of such important matters as transitions to democracy;<sup>1</sup> the emergence and demise of authoritarian regimes;<sup>2</sup> the intersection of domestic and international politics;<sup>3</sup> the origins and development of welfare states;<sup>4</sup> social identities in politics;<sup>5</sup> the

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<sup>1</sup> See for example Baloyra (1987); Bratton and Van de Walle (1997); Diamond (1999); Downing (1992); Gould (1999); Haggard and Kaufman (1995); Rueschemeyer, Stephens, and Stephens (1992); and Yashar (1997). A comprehensive review of hypothesis-testing and cumulative theoretical development in this field appears in Mahoney (2000c).

<sup>2</sup> Examples include Doyle (1986); Ekiert (1996); Im (1987); Mahoney (2001); and Snyder (1998). For further review of the literature, see Mahoney (2000c).

<sup>3</sup> Examples include Friedberg (2000); Gourevitch (1986); Ikenberry (2001); Katzenstein (1978); Krasner (1978); and Simmons (1993).

roots and development of economic regimes;<sup>6</sup> and the causes and consequences of social movements and revolutions.<sup>7</sup>

Obviously, studies using historical-institutionalists strategies of analysis vary in many important ways. Some are explicitly comparative, while others analyze trends within just one macro context. Some offer suggestive interpretations (e.g., Hart 1994) while others offer explicit models framed in general terms (e.g., Hansen 1995). Some historical institutionalist studies draw extensively from primary sources (e.g., Gamm 1999), while others synthesize findings from secondary publications (e.g., Skocpol 1979, Downing 1992). And some deploy arguments about strategic choice and the impact of “rules of the game” (e.g., Immergut 1992, Pierson 1994), while others adopt culturalist modes of explanation (e.g., Hattam 1993). Any vibrant tradition of research encompasses variety and flourishes through internal debates--and historical institutionalism is certainly no exception. In another context, we could devote an entire article (or articles) to exploring debates and differences among works with historical-institutionalist features – defending our own choices within these debates. But we have a different goal for this chapter. Despite variety on many key dimensions, historical institutionalists share distinctive and complementary strategies for framing research and developing explanations. What historical institutionalists broadly share becomes apparent when we juxtapose their ways of asking questions and seeking answers – their research strategy – to the strategies normally used by behavioralists and rational-choice modellers. Without denying variety within major approaches, this essay aims to make distinctive core strategies visible, so that we can see the advantages and limits of historical institutionalism compared to the other research approaches extensively used in empirical political science.

We characterize historical institutionalism and other leading approaches in empirical political science on the basis of “elective affinities” shared in practice by many scholars who do each style of work. We are not saying that everyone in each camp marches to the same tune. And we recognize that many scholars blend styles of research in highly creative ways (as we will suggest in the conclusion to this chapter, “boundary

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<sup>4</sup> See Esping-Andersen (1990); Flora and Heidenheimer (1981); Hacker (1998); Huber and Stephens (2001); Immergut (1992); Maioni (1999); Pierson (1994); Skocpol (1992), and Steinmo (1996). The striking cumulation of knowledge in this field is reviewed in Amenta (2000) and Pierson (2000b).

<sup>5</sup> Examples include Hattam (1993); Katznelson and Zolberg (1986); Kryder (2000); Lustick (1993); and Marx (1998).

<sup>6</sup> For key examples from a wide-ranging literature, see Karl (1997); Richards and Waterbury (1990); Streeck (1992); Thelen (1993, 1994); and Zysman (1994).

<sup>7</sup> Important examples include Goldstone (1991); Goodwin (2001); McAdam (1982); McAdam, Tarrow, and Tilly (2001); Skocpol (1979); Tarrow (1998); Wickham-Crowley (1992).

crossers” are often among the most creative scholars in our discipline). Practitioners of major approaches nevertheless share ways of posing questions and developing explanations -- giving historical institutionalism, behavioralism, and rational choice modeling characteristic features, strengths, and weaknesses. Operating as if we were anthropologists documenting the folkways of neighboring and co-mingled clans, we explore how historical institutionalists, compared to their cousins doing behavioralism or rational choice, regularly go about defining research agendas and developing explanations.

Three important features characterize historical institutional scholarship in contemporary political science.<sup>8</sup> Historical institutionalists *address big, substantive questions that are inherently of interest to broad publics as well as to fellow scholars*. To develop explanatory arguments about important outcomes or puzzles, historical institutionalists *take time seriously*, specifying sequences and tracing transformations and processes of varying scale and temporality. Historical institutionalists likewise *analyze macro contexts and hypothesize about the combined effects of institutions and processes* rather than examining just one institution or process at a time. Taken together, these three features – substantive agendas; temporal arguments; and attention to contexts and configurations -- add up to a recognizable historical institutional approach that makes powerful contributions to our discipline’s understandings of government, politics, and public policies.

To explain how this approach works and make the case for its fruitfulness, we discuss in turn each of the three aspects of historical institutionalist scholarship, pausing at appropriate points to explore advantages or limitations compared to strategies used by other families of political scientists. We focus on what creative communities of scholars actually do, paying less attention to what they or others say they do (or claim they should do). At the end of the chapter, we step back to consider some of the broadest issues of empirical research method and strategies of knowledge cumulation: Can historical institutionalists really develop valid arguments from case studies and small-n comparisons? How scientifically fruitful are research agendas driven by substantive questions rather than elaboration of a theory or techniques? And what are the prospects for combining the strengths of historical institutional analysis with advances in strategic modelling or statistically sophisticated survey research? Historical institutionalism is experiencing a bold new phase of methodological development, yet its focus on substance and its theoretical eclecticism simultaneously open the way for fruitful cross-fertilization with the best of sister research traditions.

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<sup>8</sup> Although we discuss historical institutionalism as one of three major research tendencies in contemporary political science, we readily acknowledge that the relevant literatures also include contributions from “comparative historical” political sociologists. This is hardly surprising. Leading approaches in the social sciences invariably bring scholars together across disciplinary boundaries; and political scientists of every persuasion have long shared theories and methods with their cousins in sociology and economics.

## BIG QUESTIONS AND REAL-WORLD PUZZLES

As the examples cited above illustrate, historical institutionalists investigate many different phenomena. All the same, scholars in this community formulate their research programs, individually and collectively, in recognizable and distinctive ways. A historical institutionalist scholar usually starts by asking about varied, historically situated outcomes of broad interest -- perhaps posing a puzzle about why something important happened, or did not happen, or asking why certain structures or patterns take shape in some times and places but not others. Why have revolutions occurred in some times and places, but not others? How did the U.S. state develop its specific pattern of institutional features? Why have welfare states emerged and developed along various paths? Why have some countries become stable democracies, while others have not? Under what circumstances do ethnic identities become prominent in national or international politics? The focus is on explaining variations in important or surprising patterns, events, or arrangements--rather than on accounting for human behavior without regard to context or modeling very general processes presumed to apply at all times and places.

In practice, moreover, historical institutionalists proceed through a constant movement back and forth among cases, questions, and hypotheses. Often, scholars immerse themselves in cases to come up with fruitful new questions, not only to test previously formulated hypotheses. Indeed, the problems that interest historical institutionalists very often come from noticing heretofore unexplained real-world variations, or from realizing that empirical patterns run counter to received academic or popular wisdom. A COUPLE OF EXAMPLES HERE?

From the points of view of academic marketing and disciplinary recognition, there may be drawbacks to the substantively focused and puzzle-driven nature of historical institutionalist research. Contributions tend to be clustered in somewhat separate topical literatures -- and scattered across subfields dealing with different eras or regions of the world. Even though they share theoretical and methodological proclivities, historical institutionalist scholars in different subfields may not regularly interact -- a situation which may limit theoretical clarity and frustrate steady methodological development. What is more, if historical institutionalists themselves are insufficiently clear about shared strategies, then scholars using other approaches may also fail to notice the theoretically relevant commonalities among historical institutionalist contributions addressing so many distinct topics and questions. Important modes of analysis -- such as ways of conceptualizing and measuring causal processes that play out over a long stretch of time -- may thereby recede from view within the discipline.

But research focused on big, important substantive problems also has obvious advantages. The "supreme test of social science," Lewis Coser (1975, p.698) once observed, can be summed up in a challenging question: Does it offer "substantive enlightenment... about the social structures in which we are enmeshed and which largely

condition the course of our lives”)? Historical institutionalists are mindful of this test. Grappling as they are with such matters as social movements, the development of the modern state, the rise and fall of civic engagement in democracies, the origins and dynamics of political economies, regime transformations, and patterns of public policies, historical institutionalists avoid academic navel gazing. They address real-world questions of interest to educated publics and university students—not to mention topics that appeal to book publishers.

The work of historical institutionalists also bridges divides within political science, including the gulf that sometimes separates normative theorists from empirical researchers. Normative dilemmas are frequently apparent in the phenomena explored by historical institutionalists, whose studies thus give substance to debates raging among political theorists. Historical institutionalist research agendas likewise engage formal theorists and behavioralists. The literature on the development of modern welfare states, for example, has grappled with key normative concerns about equality, democracy, and liberty, and empirical investigations have brought historical institutionalists into regular dialogue with choice theorists and survey researchers. Precisely because this literature has been problem-driven, theoretical and methodological dialogues and combinations have been highly fruitful (on this, see Amenta 2000, Pierson 2000b). Scholars of various theoretical and methodological persuasions have been able to avoid sterile paradigm disputes. Under the overall leadership of historical institutionalists, the entire research community has steadily developed sharper and broader explanations – and normative understandings -- of the origins, variety, and dynamics of national systems of economic regulation and social provision.

## TRACING HISTORICAL PROCESSES

Historical institutionalists may ask big questions and bridge divides within and beyond academia, but how do they go about developing explanations? We have already alluded to what is perhaps the most distinctive feature of this approach: Whether particular works use comparisons or analyze various aspects of one theoretically justified case, historical institutionalists take history seriously -- as something much more than instances located in the past. To understand an interesting outcome or set of arrangements usually means to analyze processes over a substantial stretch of years, maybe even many decades or centuries. Scholars working in this tradition have developed compelling methodological and theoretical justifications for historically-grounded investigations—by which they mean not just looking at the past, but looking at *processes over time*.

Some reasons for taking history seriously are straightforward, and already recognized by political scientists of many methodological persuasions. Extending the time frame of social inquiry obviously widens the range of experience available for examination. This simultaneously makes more data available and generates greater variation in outcomes. Such widening of the empirical terrain is especially important for

political scientists because many phenomena of great interest -- especially macro ones such as revolutions, state-building, democratization, the construction of welfare states), occur relatively infrequently, or only partially -- in any particular slice of time. Historically informed investigation also sensitizes investigators to temporal boundary conditions, or period effects, with respect to claims about causal relationships. By examining a wider range of historical settings, an analyst may consider the possibility that supposedly universal effects in fact only hold under particular circumstances.

Yet historical institutionalists push beyond these general considerations to *theorize about historical dimensions of causation* (Rueschemeyer and Stephens 1997; Mahoney 2000b). Optimally, assertions of causality should be borne out not just by a correlation between two variables, but by a theoretical account showing why this linkage should exist and by evidence suggesting support for that theorized linkage. Thus efforts to systematically trace social processes can make an essential contribution to supporting or challenging claims about social causation (Bennett and George 1997; Hall 2001). The relatively small number of cases in many historical institutional studies allows the kind of detailed examination of processes that can facilitate the evaluation of claims about causal mechanisms. In addition, theoretically-grounded assertions of causal relationships generally imply particular temporal relationships among variables -- that one precedes the other, or that the two occur at essentially the same time. Sophisticated process-tracing thus often involves a substantial historical component. Without the kind of attentiveness to temporally specified process that is a distinctive hallmark of historical institutionalist scholarship, important outcomes may go unobserved, causal relationships may be misunderstood, and valuable hypotheses may never receive consideration.

### Path Dependence, Sequences, and Conjunctures

A central example of why history may be causally critical involves claims about *path dependence* that are common in historical institutionalist scholarship (see e.g. Collier and Collier 1991; Ertman 1996; Hacker 1998; Shefter 1977; Huber and Stephens 2001). "Path dependence" can be a faddish term, lacking clear meaning, but in the best historical institutionalist scholarship it refers to the dynamics of self-reinforcing or positive feedback processes in a political system -- what economists call "increasing returns" processes (Pierson 2000a; cf. Mahoney 2000b). A clear logic is involved in strictly defined path dependent processes: Outcomes at a "critical juncture" trigger feedback mechanisms that reinforce the recurrence of a particular pattern into the future. Path dependent processes have very interesting characteristics. They can be highly influenced by relatively modest perturbations at early stages. Once actors have ventured far down a particular path, however, they are likely to find it very difficult to reverse course. Political alternatives that were once quite plausible may become irretrievably lost. Thus, events or processes occurring during and immediately following critical junctures emerge as crucial.

There are strong theoretical grounds for believing that self-reinforcing processes are prevalent in political life. Once established, patterns of political mobilization, the

institutional “rules of the game”, and even citizens’ basic ways of thinking about the political world will often generate self-reinforcing dynamics. In addition to drawing our attention towards “critical junctures” or formative moments, arguments about path dependence can thus help us to understand the powerful inertial “stickiness” that characterizes many aspects of political development. These arguments can also reinvigorate the analysis of power in social relations, by showing how inequalities of power, perhaps modest initially, are reinforced and can become deeply embedded in organizations, institutions, and dominant modes of political understanding. Path dependence arguments also provide a useful and powerful corrective against tendencies to assume functionalist explanations for important social and political outcomes. Perhaps most important, an appreciation of the prevalence of path dependence forces attentiveness to the temporal dimensions of political processes. It highlights the role of what Arthur Stinchcombe has termed “historical causation” in which dynamics triggered by an event or process at one point in time reproduce themselves, even in the absence of the recurrence of the original event or process (Stinchcombe 1968).

An appreciation of increasing returns dynamics is one important justification for the focus on issues of *timing and sequencing* that constitutes a second important theoretical rationale for focusing on historical processes. In path dependent processes, the order of events may make a fundamental difference. Historical institutionalists tracing broad patterns of political development across a number of countries often argue that the timing and sequence of particular events or processes can matter a great deal (Gerschenkron 1962; Kurth 1979; Shefter 1977; Ertman 1997). In Ertman’s analysis of European regime formation, for example, it is the relative timing of expanding literacy and the onset of military competition that are crucial to paths of state-building. Where countries faced intense military challenges prior to the era when literacy became widespread, the character of the state’s fiscal apparatus was likely to be very different than when the order of these two processes is reversed. SAY MORE?

Jacob Hacker’s forthcoming study of the development of American social policy presents a second compelling example of this kind of argument (Hacker forthcoming). Hacker explores the distinctive paths of development of public versus private, employer-based systems of health care and pension provision in the United States over the past century. Although both policy sectors have roughly equal mixes of public and private spending, they developed along fundamentally different lines. In U.S. health care, public interventions have served as supplements or compensations added to a prior set of institutions featuring private provision. By contrast, in U.S. pensions, a public core system of retirement provision developed first, with private arrangements playing a supplementary or complementary role. Through processes of policy feedback, these different sequences of public and private intervention generated quite different interest group environments – shifting both the preferences and political resources of crucial actors like employers. Given the interplay of preferences and interest groups in politics, moreover, the different patterns of development produced very different distributional outcomes, profoundly shaping the contours of contemporary political struggles over social provision. Any scholar who merely discusses these contemporary struggles



without awareness of the history that shaped the terrain of preferences and actors will miss much of central causal relevance to explaining politics and policymaking today.

Like these arguments developed by Ertman and Hacker, most propositions historical institutionalists offer about the causal impact of sequences are grounded in claims about self-reinforcing or increasing returns processes (Pierson 2000c). Relative timing, or sequence, matters because subsequent self-reinforcing processes, playing out over time in political and social life, transform the consequences of later developments. Path dependent arguments about self-reinforcement explain why and when sequencing can matter. Increasing returns processes occurring during particular periods generate irreversibilities, essentially removing certain options from the subsequent menu of political possibilities. **THE FOLLOWING MAY NEED TO BE A SEPARATE PARAGRAPH WITH AN EXAMPLE SPELLED OUT.** At the same time, the specific focus on sequencing illuminates how arguments about path dependent processes can be incorporated in claims about political change as well as political inertia. For instance, path dependent processes may operate to institutionalize specific political arrangements that ultimately prove vulnerable to some displacing event or process newly emergent at a later stage in political development (Collier and Collier 1991; Luebbert 1991).

It is highly instructive to contrast these arguments about path dependent sequences with arguments rational choice theorists have made about sequences within highly institutionalized settings (Shepsle 1986). Working from Arrow's impossibility theorem, which suggests the likelihood of endless cycling in many collective choice situations, rational choice theorists have argued persuasively that institutional arrangements governing agenda control and decision-making procedures can produce stable outcomes. These arguments, which also demonstrate that sequencing (among alternative choices) will be crucial, rest on the equivalent of a path dependent mechanism: steps in a sequence are irreversible, in the sense that losing alternatives are dropped from the range of possible options. By showing how such irreversibilities can be generated in a wide variety of social contexts, however, it is possible to expand this crucial finding to a far broader range of social phenomena than those covered by the rational choice literature stemming from Arrow's work. Sequencing matters not only for collective choices within legislatures, but potentially for *any* social process where self-reinforcement means that forsaken alternatives become increasingly unreachable with the passage of time. In comparative historical analyses, these arguments are often applied to large-scale social changes such as democratization (Collier and Collier 1991; Collier 1999), industrialization (Gerschenkron 1962, Kurth 1979), state-building (Ertman 1997, Shefter 1977), or welfare state development (Huber and Stephens 2001).

Historical institutionalists also employ timing and sequence arguments to focus on *conjunctures* -- interaction effects between distinct causal sequences that become joined at particular points in time (Aminzade 1994; Orren and Skowronek 1994). The ability to identify and explore such conjunctures is a major advantage of the more macroscopic inclinations of historical institutionalism. Skowronek's (1997) account of presidential leadership emphasizes the interaction of slowly developing social capacities with the particular position of an individual president within the sequential rise and decline of a

dominant political coalition. Many of the path dependent accounts discussed above focus on the distinct interaction effects produced when multiple social processes conjoin in different ways. Thus Ertman (1997) stresses links between military competition and developing social capacities for bureaucratic governance, while Shefter (1977) analyzes the interaction of state-building and party formation. These analysts focus on distinct social processes that become linked in different and causally-crucial ways depending on relative timing. These conjunctures would never be noted by micro-level analyses focusing on single processes in isolation. THIS PARAGRAPH IS NOT ALL THAT CLEAR.

### Slow-Moving Causal Processes

Another theoretical justification for focusing on historical process is to draw attention to lengthy, large-scale, but often very slow-moving social processes (Pierson 2001). Historical institutionalists seek to be attentive to the unfolding of both causal processes and important political outcomes over extended periods of time. Most political scientists are strongly predisposed to focus on aspects of causal processes and outcomes that unfold very rapidly. Yet many things in the social world take a long time to happen. Some causal processes and outcomes occur slowly because they are incremental – it simply takes a long time for them to add up to anything. Changes in pension systems, for example, are not fully translated into levels of public spending for a half-century or more. A second possibility is the presence of threshold effects – many social processes may have little significance until they attain a critical mass, which may then trigger major change (McAdam 1982; Goldstone 1991; Baumgartner and Jones 1993). Alternatively, slow-moving processes may involve transformations that are probabilistic during any particular period and therefore several periods may be necessary before the transformation occurs. Under such circumstances, the social outcome of interest may not actually take place until well after the appearance of key causal factors. Particularly when it focuses on macroscopic processes, historical institutionalist research is often primarily interested in such structural preconditions for particular outcomes, rather than the specific timing of those outcomes (Collier 1999; Moore 1966; Rueschemeyer, Stephens, and Stephens 1992). When either structural causes or threshold effects are at work, analysts adopting a short time frame are likely to focus erroneously on the more idiosyncratic or precipitating factors that trigger outcomes. Because some crucial social conditions may change only slowly, analysts studying a narrow time frame will be strongly inclined to take them as fixed and therefore irrelevant to their causal accounts (Rueschemeyer, Stephens and Stephens (1992).

Another possibility is that causal processes involve chains with several links, which require some time to work themselves out. To the extent that causal chains of this sort are at work, analyses must frame their studies on a broad time-scale. Collier and Collier's influential work on labor incorporation in Latin America presents arguments of this kind, in which the ultimate outcomes of interest reflect a sequence of key developments over extended periods of time (Collier and Collier 1991). Indeed, this type

of claim about long-term, multi-stage causal processes is often invoked in work on state-building (Flora 1999a, 1999b) or democratization (Luebbert 1991; Collier 1999).

Swank (2001) offers an instructive recent example. In assessing the impact of political institutions on welfare state retrenchment, he criticizes the view that fragmented institutions will limit cutbacks by increasing the number of veto points available to defenders of the status quo. Swank argues that this is true as far as it goes, but notes that the long-term, indirect effects of institutional fragmentation run largely in the other direction. Not only does institutional fragmentation limit the initial expansion of the welfare state, but it also reinforces social heterogeneity, inhibits the growth of encompassing interest groups, and weakens cultural commitments to universalist social policies. All of these long-term effects strengthen the welfare state's opponents and weaken its advocates. Thus many of the most important effects of institutional fragmentation work themselves out only indirectly and over extended periods of time. Ahistorical analyses typically seek to consider the effects of institutions while "holding constant" other variables – but these variables are in part the long-term consequences of institutional structures. These investigations are therefore likely to systematically misread the impact of institutional structures on the politics of the welfare state.

Analysts who fail to be attentive to these slow-moving dimensions of social life may ignore potentially powerful hypotheses. They are particularly likely to miss the role of many "sociological" variables, like demography (Goldstone 1991), literacy (Ertman 1997), or technology (Kurth 1979), as well as the impact of other slowly-building pressures such as international military competition and fiscal overload (Skocpol 1979). Their explanations may focus on triggering or precipitating factors rather than deeper causes (Kitschelt 1999). Perhaps most fundamental of all, they may fail to even identify some important questions about politics because the relevant outcomes happen too slowly and are therefore simply off their radar screens.

### History as Process, not Just Illustration

In the ways we have just surveyed, theoretical attentiveness to historical processes represents a formidable comparative advantage of historical institutionalism, especially since this attentiveness is linked to macroscopic analysis focusing on institutions and organizations in addition to aggregates of people. Most research in the behavioral tradition uses surveys that offer a snapshot in time. And even when surveys are repeated to offer a longitudinal series, it is rare indeed for behavioral analysts to consider changing institutional contexts, critical conjunctures, or path dependent large-scale processes as causally relevant to the changing modes of individual behavior they probe. There are exceptions, certainly, such as Carmines and Stimson's dissection of the processes by which race became a transforming issues in U.S. partisan politics, or occasional consideration of the impact of World War II on "life course" developments for late-twentieth century American adults (XXX, Putnam). But for the most part, survey analysis relies on one-time data about attitudes and self-reported behaviors to explore individual-level hypotheses about mass individual patterns. Change over time often enters the discussion only speculatively, as when a researcher reports differences between working

women and housewives at a moment in time, and then go on to speculate that the changing proportions of such persons in the population may signify a continuous social change.

To be sure, some rational choice scholars have turned to historical case studies in recent years. But most of the analytic advantages we have outlined are absent in this work, because the past enters only a highly restricted sense -- as what might be termed “illustrative history,” the mining of the historical record for outcomes which can be “explained” by particular rational choice models. Previously established models may be applied in interesting ways to examples in the past (cf. Bates et.al. 19XX), but the tools of game theory turn out to be poorly suited to analyzing conjunctures or exploring slow-moving macro processes (Elster 2000; Munck 2001). Rational choice theory faces considerable difficulties in moving from the micro to the meso- or macro-levels of analysis that are typically featured in works that analyze processes over long stretches of time. Rational choice’s reliance on theory-driven agendas and on the identification of empirical terrain favorable for its methods leads relentlessly back to an emphasis on the micro. And the results of game theory quickly become indeterminate or unmanageably complex as one increases the number of actors involved (indeed, in game theory the problem of indeterminacy is often rife even at the micro level). The fact that many macro-processes take considerable time to play out presents a further difficulty, since game theory generally requires that all the relevant actors, preferences, and pay-offs be established and fixed simultaneously at the beginning of a game. In short, there are real obstacles in rational choice theory to serious consideration of many key aspects of historical processes.<sup>9</sup>

## ANALYZING INSTITUTIONS IN CONTEXT

Historical institutionalism is characterized by the second part of its label as well as the first, but what does “institutionalism” mean for this family of scholars? We can say of much political science today what Richard Nixon once said about Keynesianism: we are all institutionalists now. Although this may exaggerate a bit, in political science today many scholars analyze how institutions influence political behavior and shape processes ranging from legislative decisionmaking to social movements (Hall and Taylor 1996). As Thelen (1999) has ably elaborated, both rational choice institutionalists and historical institutionalists presume that organizationally embodied routines play a crucial role in allocating resources and structuring the incentives, options, and constraints faced

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<sup>9</sup> Even where rational choice theory seems better equipped, it has made very limited moves in this direction. For instance, despite North’s important work on path dependence, his strong focus on historical process has been largely ignored by rational choice theorists (although cf. Harvey 1998). Nor, despite occasional hints (Aldrich 1994) has the potential to generate richer and broader insights about sequencing from Arrow’s theorem been exploited.

by political participants. In this sense, institutionalism is indeed a broadly shared approach in contemporary political science.

But even though institutionalists of different stripes have converged on complementary questions and findings (Thelen 1999, 372-81), important differences remain. Rational choice scholars tend to focus on “rules of the game” that provide equilibrium “solutions” to collective action dilemmas. Historical institutionalists, meanwhile, probe uneasy balances of power and resources, and see institutions as the developing products of struggle among unequal actors. Rational choice scholars often focus on one set of “rules” at a time. Historical institutionalists, by contrast, typically do meso- or macro-level analyses that examine multiple institutions in interaction, and operating in, and influenced by, broader contexts. Historical institutionalists do not presume that various, intersecting institutional “pieces” fit together into any coherent whole, and pay close attention to ways in which multiple institutional realms and processes intersect with one another, often creating unintended openings for actors who trigger changes. Historical institutionalists probe the origins, impact, and stability or instability of entire institutional configurations – sometimes to explain the institutional arrangements themselves, at other times to use variables referring to institutional configurations to explain other outcomes of interest.

### Institutional Effects

Much research in historical institutionalism adopts a meso-level focus, concentrating, for example, on policy developments in a particular issue area (e.g. Hacker 1998; Immergut 1992; Weir 1992) or changes in organizational fields (e.g., Skocpol, Ganz, and Munson 2000). Historical institutionalists may also tackle the most macro-level developments, such as modernizing intellectual transformations (Wuthnow 1989) or state formation (e.g. Anderson 1986, Doyle 1986, Tilly 1978, Downing 1992; Ertman 1997; Skowronek 1982). In either case, analyses tend to highlight and explore causes operating at the inter-organizational or interinstitutional level. Certainly, historical institutionalists accept the principle that causes should ultimately be consistent with plausible accounts of individual motivation and behavior (Little 1991). But they also believe that the patterns of resources and relationships in which individuals find themselves have powerful channeling and delimiting effects. So historical institutionalists aim to make those patterns visible and trace their causal impacts.

Historical institutionalists rarely focus on a single institutional or organizational site of contestation, as rational choice scholars often do. Instead, they typically analyze how sets of organizations and institutions relate to each other and, in turn, shape the processes or outcomes of interest. There is a strong tendency to doubt the power of many claims about institutional effects that rest solely on an analysis of that institution in isolation. Instead, historical institutionalists generally expect to see powerful effects stemming from interactions among institutions, or between institutions and organizations. Research on American policy development, for instance, typically focuses on the interplay among multiple organizational actors in multiple institutional settings (Skocpol

1992; Weir 1992). Melnick's (1994) analysis of the "rights revolution" that has fueled regulatory expansion in the United States provides a good example. Tracing this revolution through multiple venues and over an extended stretch of time, he demonstrates that it must be understood as the result of an interaction between the courts and Congress, with newly emergent citizens' organizations playing a crucial role in coupling these distinct institutional sites. Advocates of policy activism in the federal courts and Congressional committees have, through interplay between the two branches, been able to advance their agendas beyond what a majority in Congress would have been likely to produce on its own.

Similarly, recent work in both comparative political economy (Kitschelt, Lange, Marks and Stephens 1999) and social policy (Esping-Andersen 1990; Huber and Stephens 2001) has focused on how *configurations* of policies, formal institutions, and organizational structures generate distinctive "welfare state regimes" or "varieties of capitalism" that operate in fundamentally different manners. Outcomes are generated not by some universal operating principles characteristic of a given type of actor or realm of activity, but by intersections of organized practices (such as labor markets, firm structures, or crucial policy arrangements). These practices will often have originated at different times and then formed configurations which advantage key actors (such as employer associations and labor unions). These actors, in turn, work to maintain the configuration as economic, cultural, and geopolitical circumstances shift.

### Institutional Development

Issues of long-term institutional development are also central to historical institutional research agendas (Thelen 1999). Historical institutionalists are typically suspicious of functional explanations, in which institutional outcomes are explained by their consequences. In such functional accounts, institutions develop because of their capacity to solve certain collective problems. The implicit or explicit claim is that rational actors produced these outcomes in order to solve these problems.

As suggested above, concern with issues of institutional development within historical institutionalism is strongly linked to theorizing about the causal relevance or origins, sequences, and temporal processes. Functional accounts of institutions seem most plausible when investigations take a one-time snapshot, because long-term effects and inconsistent "layers" of institutional development (see Schickler 2001) are not so readily noticed in such studies. In most cases, synchronic analysts simply probe an extant institution to uncover benefits for particular actors. Analysts then infer that these benefits explain the institution, implying that the actors who are presently advantaged (or their forbears) created the institution to produce the benefits. This is a plausible hypothesis, but it is only a hypothesis – and over-time investigation of institutional origins and dynamics often does not bear out such lines of reasoning.

By examining issues of institutional origins and change over an extended time frame, historical institutionalists have been able to highlight a number of potential

problems for functional accounts (see Thelen 1993, 1994, 1999; Pierson 2000d for relevant findings and arguments). Functional interpretations of politics are often suspect because of the sizable temporal gap between actors' actions and the long-term consequences of those actions. Political actors, facing the pressures of the immediate or skeptical about their capacity to engineer long-term effects, may pay limited attention to the long-term. Thus the long-term effects of institutional choices, which are frequently the most profound and interesting ones, should often be seen as the *by-products* of social processes rather than the realization ("congealed preferences" as Riker put it) of actors' goals.

A second issue concerns unintended consequences. Even where actors may be greatly concerned about the future in their efforts to design institutions, they operate in settings of great complexity and high uncertainty. As a consequence, they will often make mistakes. Thus institutions may not be functional even in a context of far-sighted actors, because they do not operate as intended. Although widely acknowledged to be significant in actual politics, political scientists often treat unintended consequences as an error term or simply ignore them by failing to investigate institutions over time. In cross-sectional studies of institutions, the issue of unintended consequences vanishes from view, since either the long-term consequences of institutional choices or the original factors generating the institutional choice will be outside the scope of the analysis. By contrast, historical institutionalists examining institutional development often stress the surprising long-term consequences of earlier political choices and conflicts (Anderson 1986; Luebbert 1991; Skocpol 1992; Thelen 1999, 2000).

Historical institutionalists, finally, demonstrate the ways in which institutions are remade over time (Thelen 1999, 2000). Because of strong path dependent effects, institutions are not easily scrapped when conditions change. Instead, institutions will often have a highly "layered" quality (Schickler 2001; Stark and Bruszt 1998). New initiatives are introduced to address contemporary demands, but they add to, rather than replace pre-existing institutional forms. Alternatively, old institutions may persist but be turned to different uses by newly ascendant groups. In either case, the original choices are likely to figure heavily in the current functioning of the institution. Thus institutions will rarely look like optimal solutions to present collective action problems.

In final analysis, clearly, attentiveness to history and analysis of meso- or macro-level institutional configurations are highly complementary strategies of analysis. Tracing politics through time is very helpful for identifying the boundary conditions for particular theoretical claims. Even more significant, the emphasis historical institutionalists place on conjunctures and sequencing draws attention to the temporal connections among social processes, and highlights the importance of meso- or macro-level analysis of institutional configurations. Furthermore, while path dependent or increasing returns processes can play out at a micro level (e.g., in the way individuals develop and reinforce particular mental maps of the social world) they are often most significant at the meso- or macro-level. Particular sets of institutions and organizations are often mutually reinforcing or complementary – the presence of each enhances tendencies for the development of the other – in a manner that can be seen as a kind of coevolutionary or

selection process playing out over considerable stretches of time. A good example is the recent research in comparative political economy focusing on “varieties of capitalism” (Hall and Soskice, 2001.; Estevez-Abe, Iversen and Soskice 1999; Soskice 1999), which stresses how different political economies have developed along quite different lines because of the coevolution of mutually-reinforcing institutional and organizational structures. **MAYBE DROP THE WELFARE STATE EXAMPLE HERE, AND SPEND SOME SENTENCES SPELLING OUT THE VARIETIES ARGUMENTS A BIT MORE, SO READERS CAN UNDERSTAND THE MAIN ARGUMENTS.** Similar arguments have become common in research on the comparative development of welfare states (Shalev 1998; Huber and Stephens 2001).

### Causal Configurations and Context Effects

Historical institutionalism’s strong emphasis on interaction effects reflects some core working assumptions about how most sociopolitical processes operate. Analysts are strongly affected by their basic presumptions about how social processes work; and they tend to frame their problems, generate hypotheses, and employ methods of analysis and research designs that fit such presumptions (Abbott 1988; Hall 2000). Behavioralists, for example, are happy to use statistical techniques to analyze data from as many “cases” as possible – often data from surveys of thousands of individuals – because they are prepared to assume that very general variables operating independently of one another come together to account for the patterns of behavior they are trying to explain. Historical institutionalists, by contrast assume that operative variables may not be independent of each other at all. When it comes to analyzing the origins and impact of institutions, causally important variables are often bundled together in the real world; and there may be alternative causal paths to similar outcomes (Ragin 1987; Shalev 1998). Historical institutionalists tend to suspect from the get-go that causal variables of interest will be strongly influenced by overarching cultural, institutional, and/or epochal contexts (Abbott 1994). Given these expectations, research in this tradition tends to move up from single institutions to broader contexts – *historical institutionalists look at forests as well as trees*. And they almost always seek to discover and explicate the impact of configurations of organizations and institutions on outcomes of interest (Katznelson 1997).

In addition to presuming—and concentrating attention on—causal configurations conceptualized at the organizational and institutional level, there is another way in which historical institutionalists highlight interaction effects. They often do this by pointing to overarching contexts—types of regimes, eras, regions, cultures—that place bounds around the theorizing being done in any given study. Historical institutionalists rarely aim to write about all of humankind through all of global history. Scan the titles of major historical institutionalist works, for example, and you will often find the proper names of regions of the world and/or beginning and ending dates, highlighting the specific period what the argument is to cover. This is not done because historical institutionalists in political science are trying to be historians; they do not aspire to “cover” everything, or just tell stories, about a time and place. Nor are dates given only



to describe critical junctures when a change occurs that the analyst seeks to explain (e.g., Clemens 1997; Skowronek 1997). Beyond such an obvious use of dates and place, historical institutionalists often set limits to the applicability of their causal arguments, arguing in theoretically explicit ways why variables appear and combine in characteristic ways in one era, but might not exist or combine in the same way in other eras.

For example, in his book about the growth (or not) and success or failure of guerrilla-led revolutionary movements, Timothy Wickham-Crowley (1992) develops a rigorous causal analysis and suggests (in his conclusion) that his model links up with theoretical explanations of revolutions in many regions and eras. But Wickham-Crowley carefully bounds his own argument, restricting it to Latin America since 1956. In analytical terms, he explains that certain unique developments and events created background conditions to generate many guerrilla movements with similar aims and methods, thus setting the stage for the variables he explores to account for movement growth and success or failure. In the end, Wickham-Crowley's careful delineation—and explanation—of the overarching context within which his variable-based analysis applies reinforces its theoretical power. As we move to other continents and periods, we can ask about changes that might influence the variables in play—and the likely relationships among them.

Another way in which historical institutionalist works highlight overarching contexts is by deliberately juxtaposing two or more contexts, to show how variable configurations already analyzed (across multiple cases) may play out in new ways when the overarching context changes. In the revolutions literature, Goodwin (2001) does this by, first, developing explanations for revolutionary successes and failures within Central America and East Asia, and then highlighting the somewhat different actors and conditions that come into play within each region. Addressing a very different problem – Pierson (1994) identifies variables about institutions and established policy characteristics that can explain why British and U.S. conservative politicians succeeded or failed with attempted cutbacks in a number of social policy areas. Yet he also steps back and stresses that the key causal relationships governing policy determination are quite different in the recent period of austerity than they were during earlier periods during which welfare states were greatly expanded.

Scholars in other major political science traditions are often much less attentive to overarching contexts than historical institutionalists, in part because they prefer to focus on individual-level behavior or micro-processes, but also because they are reluctant to write “like historians.” But, ironically, the result can be less theoretically powerful work. Behavioralists happily rely on types of data only available at one point in time, or just for short time spans, but do not necessarily think through what that means. As a result, significant overarching contexts can go unnoticed, unconceptualized. For example, behavioralists sometimes fail to notice that very similar individual-level patterns—joining more or fewer voluntary organizations, voting at different rates—can end up having very different meanings, depending on the kinds of organizations or institutions that predominate in a given nation or era. Theories invoked in behavioralist studies can end up being profoundly underspecified.

Rational choice scholars, meanwhile, often write as if the models they present were infinitely generalizeable-- even as they smuggle all kinds of institutional, cultural, and epochal specifics into their empirical operationalizations. This may seem an optimal way of arguing -- isn't social science supposed to generalize? But, in practice, this kind of approach means that we leave important variables implicit, and we fail to see how changing background conditions might cause the same variables to play out in very different ways. Again, the preference of rational choice theorists for examining micro-settings strongly reinforces this tendency.

## RESEARCH STRATEGIES AND THE ACCUMULATION OF KNOWLEDGE

Tackling big, real-world questions; tracing processes through time; and analyzing institutional configurations and contexts – these are the features that define historical institutionalism as a major strategy of research in contemporary political science. We have stressed the comparative advantages these approaches afford to all who would better understand government and politics. But we must also acknowledge the claims of critics who dismiss historical institutionalism as a valid approach to doing scientifically cumulative social science. Critics of historical institutionalism sometimes pen manifestos announcing that case studies and small-n comparisons cannot generate valid knowledge, because cases are not randomly selected and there may not be enough statistical “degrees of freedom” to test all conceivable hypotheses rigorously (cf. Geddes 1990, Goldthorpe 1991, Lieberson 1991). Statistical methodologists (such as King, Keohane, and Verba 1994) also worry that the historical institutionalist proclivity for tackling significant issues predisposes them toward “selection on the dependent variable,” that is, choosing cases where a phenomenon of interest has occurred, while ignoring the instances where it has not occurred. Historical institutionalist books and articles are sometimes denounced in ways such as these one at a time, in isolation from one another—an approach that surely would be laughed out of court if applied to isolated works in any other tradition. In all research traditions, individual works build upon one another, often extending lines of analysis, re-testing arguments, and correcting for earlier limitations. The real issue is whether historical institutionalists in general have headed up blind alleys, because their studies, considered collectively as well as individually, are improperly designed. This is not the place for an exhaustive review of the profusion of recent methodological reflections in this genre, but the major signposts on the road can be mentioned.

Methodological challenges have, in our view, been good for historical institutionalism. Not only have critiques (such as XXX) been testimony to the visibility and intellectual impact of the studies they have dissected. Challenges have had a bracing impact, prompting historical institutionalists to spell out their metatheoretical presumptions and sharpen rationales and tools for doing valid macroscopic and historical studies. A number of scholars, for example, have asked how studies can best be designed to take intuitions about configurational causation and temporal process seriously. In other words, if historical institutionalists doubt that single, highly general variables have uniform effects across contexts, and regardless of interactions with other factors, how can

research designs allow for adequate empirical exploration of hypotheses about contexts and configurations (see Abbott XXXX, Hall XXX; Skowronek and XXX; Ragin XXXX). If historical institutionalists stress mechanisms rather than simple association in causal arguments, how can “process tracing” be done in a rigorous way? And what criteria must be met to demonstrate valid arguments about “path dependence,” critical conjunctures, or sequence effects? Although Pierson (XXXX), Thelen (XXX), and others have primarily focused on the theoretical characteristics of arguments about temporal causality, their reflections also set empirical standards to be met in case analyses and comparative studies that aim to establish the presence of causally relevant processes or events.

Reflecting on the strengths and limits of (by-now) hundreds of studies in various important literatures, methodologically oriented scholars have also made the case for circumstances under which in-depth case studies and small to medium-n comparisons are an optimal research strategy (Mahoney 2000a; Munck 1998; Ragin 1987; Rueschemeyer and Stephens 1997). Random selection of cases – say of particular regimes or nations from hundreds across the globe – is often not appropriate; and it is far from the only way to test hypotheses rigorously. Whatever the risks or drawbacks, hypotheses can be rigorously tested even when scholars cannot sample from large numbers of truly independent cases (for an excellent overview of strategies of inference in small-n research, see Mahoney 2000a). Alternative strategies of causal inference have been developed and applied, because there are important intellectual and practical advantages to focusing agendas of research on a small to intermediate number of cases, including instances of substantively compelling outcomes and arrangements we want to understand (Collier and Mahoney 1996; Dion 1998; McKeown 1999; Munck 1998; Ragin 1997; Rueschemeyer and Stephens 1997).

For example, a scholar who wants to understand revolutions is unlikely to be willing to start with a purely random set of times and places; she will want to make sure that clear-cut revolutions are included in her research. But in the best past studies of revolutions – as in many other historical institutionalist literatures -- it has long been standard to test hypotheses with comparisons between “positive cases” where phenomena of interest occur and closely matched “negative cases” where the phenomena do not occur (Skocpol 1979, Goodwin forthcoming, XXXXX). Speaking about comparative studies in many topic literatures, analyses have juxtaposed time periods, regions, and policy sectors – turning what appear to be one or a few national instances into settings for many carefully compared cases. And even within what appear to be single “case studies,” empirical observations have often been multiplied by formulating and testing hypotheses about the *mechanisms* that connect causes to effects (Bennett and George 1997). All of these strategies of analysis have been used to good effect by scientifically sound historical institutional studies -- and recent methodological literatures will make it easier for future scholars to recognize and apply the standards and rationales for “best practices” in case-based and small-n research.

No matter what theories or research methods are deployed, individual studies in isolation never do more than move the scholarly enterprise a step or two forward. Scholarship is an inherently communal enterprise, and so it is appropriate to reflect on

how well clusters of scholars do at *cumulating* valid and important findings. Here is where historical institutionalists do quite well, in our view, because substantively compelling, problem-driven research facilitates exactly the sort of intellectual cumulation that allows a community of researchers to make clear progress over time.

Because historical institutionalists tend to tackle big, humanly important questions, scholars are likely to come back to the issues again and again over several academic generations. Within each generation as well, smart people jump in, willing to argue about how best to frame the questions and describe patterns worth explaining, as well as about how to formulate and test hypotheses. People reexamine cases and hypotheses, and extend established hypotheses to new sets of cases. A good example is spelled out in Jim Mahoney's review of several decades of historical-institutionalist research on the origins and dynamics of democratic and authoritarian regimes. In this literature, research took off from pioneering agenda-setting works (i.e., Moore 1966, O'Donnell 1973, Linz 1978), with waves of succeeding scholars executing dozens of historical and comparative-historical studies that served to refute some arguments, refine others, discover new lines of causal argument, and extend findings across eras and continents. Cumulative research now offers a convincing picture of when, why, and how different types of regimes have emerged in various continents over much of modern world history. "Most of our knowledge about the relationship between democracy and various other factors (social classes, international conditions, elite behavior) has been derived through such step-by-step accumulation..." Mahoney concludes. "Plainly, if one were to strike all comparative-historical research from the record, most of what we currently know about the causes of democracy and authoritarianism would be lost."

What Mahoney demonstrates about the literature on democracy and authoritarianism holds as well for literatures on revolutions, welfare states, and varieties of public policymaking, to name just some areas of clear-cut progress in recent decades. In each of these literatures, historical institutionalists have persistently addressed big questions, examining and reexamining cases and long-term transformations. Over several scholarly generations, they have moved the ball of scientific progress further toward the goal-lines toward which scholars of many persuasions are struggling.

Of course, scholars in other major traditions also think of themselves as savvy practitioners of cumulative social science, and we do not suggest that steady progress can occur only in areas where historical institutionalists predominate. But it is worth noting that sets of scholars who define cumulation in theoretically orthodox terms, or who let themselves be captured by their methodological tools, can lead us up blind alleys.

Rational choice scholars often focus research agendas on puzzles internally generated by their overarching theory. Prominent examples of theory-derived problems include CAN THIS BE SAID BETTER, PAUL? DO YOU MEAN ASKING WHY IT IS RATIONAL FOR PEOPLE TO VOTE AT ALL? the prevalence of voting (see Green and Shapiro 1996 for a critique) and the relative absence of Arrow-style "cycling" dynamics in Congress. AGAIN, SAY THE PROBLEM CLEARLY, SO READERS CAN SEE WHY THEY MUST SUSPECT IT IS TRIVIAL. Each of these "problems"

has generated a massive amount of published work, and there is no doubt that the answers provided have become more sophisticated over time. What is less clear is whether these agendas have generated knowledge that speaks to major social problems, or would be of significant interest to anyone other than enthusiasts of rational choice theory. How might these bodies of work fare if we ask James Rule's (1997) question for evaluating research programs: Would someone outside your camp be likely to acknowledge the usefulness of the knowledge you are providing? [IS THIS A QUOTE?] I THINK THIS ENTIRE PARAGRAPH NEEDS MORE PUNCH.

Methods of investigation rigidly tied to theoretical presumptions can also prompt rational choice research agendas into overly constricted channels. With game theory generally providing the central analytical tool, studies too easily confine themselves to modelling strategic action at the micro-level. Rational choice practitioners typically focus on political contexts with coherent strategic actors – preferably individuals, such as politicians or candidates -- operating in particular, well-bounded contexts – such as legislatures where choices are clearly identifiable and pay-offs relatively transparent. Efforts to deal with broader social aggregates, whether interrelated organizations or looser social groupings, are often avoided. Or such challenges are handled by simply treating these groups as themselves being coherent strategic actors – a highly questionable move. What is more, rational choice studies typically make the assumption that all major actors and their preferences are present at the outset of the processes analyzed, when in the real world, new actors and changed preferences often emerge in later stages. The story of politics ends up sliced and frozen into artificial moments on the slide of a powerful, but tightly focused microscope.

The use of one grand theoretical framework to frame research programs is often justified, at least implicitly, by what could be called the “lego” rationale. This rationale holds that social scientists should focus on developing solid blocks of findings within a presumed whole. No matter how small or distant from pressing social concerns, these blocks can then be pieced together to produce robust answers to important questions. Triviality is acceptable if it provides something to build on. But there are strong reasons to doubt the validity of the lego approach. As rational choice scholars, above all, should know, partial actions do not invariably add up to optimal outcomes. More to the point, where is the evidence that a quarter-century of lego-style research has generated ever broader findings? In certain areas of political science, 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. Thus the study of American politics becomes the study of Congress (or, at its most expansive, the study of Congress and administrative agencies). And the study of comparative politics becomes the study of parliaments and government coalitions. Big questions, broader contexts, and long-term transformations recede ever farther from view --and political science risks cutting itself off from concerns important to broad audiences.

Obsession with a single theory for its own sake is not the only way that cumulative research agendas can go awry. Another danger is capture by technique, or over-reliance on a single kind of data. Like historical institutionalists, behaviorialists are

usually problem-focused. But they risk becoming exclusively enamored with social surveys and the statistical manipulations that can be performed on the answers of random samples of individuals. This can lead behavioralists to neglect other kinds of data that might be relevant to the substantive questions at hand. Tendencies in this direction plague some current work on civic engagement, for example. Why should we presume that everything we need to know about civic participation can be discovered from social surveys – usually at confined to one moment in time – that ask vast aggregates of people about their attitudes and reported behaviors? Heavy reliance on social surveys has channeled much of the current debate about “civic engagement in American democracy” toward exploring attitudes of social trust and indications of whether individuals choose to participate in various ways. But data of this sort is available only for the 1970s and after, and key changes in behavior and institutions happened long before that. What is more, we cannot understand the impact of rising or declining individual memberships in voluntary groups unless we know about what kinds of groups have increased or decreased, and unless we know how groups interact with institutional centers of decision-making. Questions about an important topic like civic engagement and democracy can rarely be adequately addressed with just one type of data, or one technique of empirical analysis.

When it comes to avoiding the dangers of capture by theory, technique, or single data sources historical institutionalists may have an easier time than scholars in the other major political science traditions. Precisely because historical institutionalists are so riveted by big, compelling real-world problems with clear substantive import to audiences beyond fellow academics, they almost never forget that keeping the eye on the substantive prize—remaining determined to understand important phenomena in the real world—is the formula for enduring scholarly achievement. Because the substantive issues at stake are important in their own right, historical institutionalists are often willing to combine theoretical insights, mine various data sources, and stretch the limits of methodological creativity to gain leverage on those issues (for some nice examples, see McAdam 1982, Rueschemeyer, Stephens, and Stephens 1992, and Schickler 2001). Each scholar knows that his or her interim answers may soon be subject to reconsideration by other scholars (or members of educated audiences) who care just as much about explaining the outcome, or solving the puzzle. And real-world developments may intervene to change the definition of important issues, or bring new dimensions of them into view. Literatures leavened by many historical institutionalist participants remain, in short, committed to theoretical and methodological pluralism, and committed to broad conversations. This introduces dynamism and reality-checks. If the legos are not fitting together to make at least the foundations of a beautiful structure, historical institutionalists won't keep playing with them forever.

## THE ADVANTAGES OF PLURALISM AND SYNERGY

ENTIRELY NEW CONCLUSION NEEDS TO BE WRITTEN, USUALLY ONLY A LITTLE OF WHAT FOLLOWS. MOST SHOULD BE CUT.

Each of the leading approaches in contemporary empirical political science--behaviorialism, rational choice, and historical institutionalism--has proven its value. Not only does political science--indeed social science as a whole-- benefit from the coexistence and competition of varied approaches to theory and research; breakthrough studies often combine lines of analysis.

OMIT REST?

So it is has certainly not been our purpose to pit major approaches against one another in any kind of zero sum game. Historical institutionalists are a theoretically pluralistic lot, and have always been open to friendly dialogue with scholars favoring other approaches. We have simply sought to clarify the distinctive contributions -- and advantages -- of historical institutionalist scholarship. Indeed, one reason to emphasize and clarify distinctions and potential comparative advantages is to facilitate exchanges that combine the strengths of different approaches to maximum effect.

Historical institutionalists are not doing narration for the sake of story-telling. Nor are they anymore lone wolves making occasional idiosyncratic contributions to cumulative scientific enterprises run by others. On the contrary, historical institutionalism is a coherent and fruitful strategy of research. And steady progress toward explaining important phenomena has been made by vibrant communities of scholars using variants of this approach – including many individuals who combine historical institutional tactics with survey analysis or formal modeling.

Historical institutionalism has become, of late, more self conscious both theoretically and methodologically, in ways we have indicated and illustrated throughout this review essay. Our aim has been to make the case for historical institutionalism as one of the three research pillars in contemporary political science – and our task has been made easy by the outpouring of so much substantive, theoretical, and methodological work in recent years. Readers who wonder what political science would be like without self-conscious historical institutionalism need only ask what we would have lost if all the works in our bibliography, and many others like them, had not been published. We think the answer is obvious: without historical institutionalism, our discipline would be shorn of much of its ability to tackle major agendas of concern to all political scientists. And without historical institutionalists, political science would have much less to say about questions of great import to people beyond as well as within the ivory tower.

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