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NÚMERO 91

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HISTORICAL INSTITUTIONALISM
IN COMPARATIVE POLITICS
Introduction

A decade ago, scholars working out of diverse traditions were united in bemoaning the lack of attention to the role of institutions in political analysis (Shepsle 1989; Hall 1986). Now, of course, institutions are ubiquitous in the study of politics. But, if the refrain "institutions matter" is widely embraced, this has only given rise to new debates, over how to define institutions, over the way in which they are seen to affect political outcomes, and over the way in which institutional change is characterized.

It is now conventional to distinguish three different "varieties" of institutionalism: rational choice institutionalism, historical institutionalism, and sociological institutionalism.¹

Each of these three schools in fact represents a rather sprawling literature characterized by tremendous internal diversity, and it is often also difficult to draw hard and fast lines between them. The differences that have been identified in fact amount to tendencies which, as Hall and Taylor note, apply unevenly across particular authors within each school of thought. Moreover, the walls around the three perspectives have also been eroded by "border crossers" who have resisted the tendencies toward cordonning these schools off from each other and who borrow liberally (and frequently very fruitfully) where they can in order to answer specific empirical questions. A few examples will suffice to illustrate this point.

First, a group of prominent theorists working out of a rational choice perspective have become proponents of a more eclectic approach that combines elements of deductive theory —the hallmark of rational choice— but combined with an explicit attempt to contextualize the analysis in ways that historical institutionalists have long advocated (Bates et al., forthcoming). This strategy, which they call "analytic narratives", represents an attempt to construct explanations of empirical events through analyses that “respect the specifics of time and place but within a framework that both disciplines the detail and appropriates it for purposes that transcend the particular story” (Levi, n.d., p. 4). In one application, Margaret Levi begins with a deductive model (of "contingent consent"), and confronts this with a set of cases that are "linked by theoretical concerns rather than by time and place" (n.d., p. 4). Barry Weingast’s application differs in that he focuses on a single case and begins with a set of hypotheses that —coming out of the work of historians— are inductively rather than deductively derived. Though approaching

their respective problems from two rather different angles, both Levi and Weingast incorporate elements of deduction and induction in highly creative fashion and in ways that overcome traditional distinctions between historical institutionalism's characteristic focus on specific contextual conditions and rational choice's characteristic search for generalizable features of political behavior rooted in the incentive structures that individuals face.

Second, it is safe to say that rational choice has had an equally important impact on the work of historical institutionalists. One development has been an enhanced appreciation of the need for explanations that rest on firm micro foundations. Although much macro-historical work was already implicitly sensitive to these issues, articulating the micro foundational logic of the arguments offered was not always a top priority. A heightened appreciation for such issues has made for sharper arguments overall; among other things, it has led to a closer examination—and frequently, disaggregation—of commonly used categories (in the political economy literature, for example, "labor") which has resulted in more fine-grained analyses (e.g., of conflicts among different categories of workers and of cross-class coalitions).

Related to this, many historical institutionalists have taken on board the notion that institutions that solve collective action problems are particularly important in understanding political outcomes. This has long been a central concern of rational choice theory, and we now see an increasing number of historical studies that focus precisely on explaining the emergence and persistence of institutions that do (or do not) facilitate coordination among employers and other groups (see, e.g., Hall 1993; Thelen and Kume 1997). As Rothstein points out, some of the most important institutions in political life are those that solve collective action problems (1996, p. 159), and this recognition has set an important agenda for historical institutionalists as well.

Third, there has also been some important borrowing and cross-fertilization between historical institutionalism and sociological institutionalism. The works that lie at this intersection often embrace a more expansive view of institutions, not just as strategic context, but as a set of shared understandings that affect the way that problems are perceived and solutions are sought. Peter Katzenstein's (1996) analysis of the evolution of Japanese security policy is one example; he puts forth a cultural explanation, but one which is rooted in an analysis of how some norms (and not others) came to be institutionalized. For Katzenstein, norms become part of the objective reality in which policy makers maneuver (1996, p. 21); among other things they provide "focal points of concern" that reveal social agreement on what is worth

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2 Though this is not the place to pursue the issue, some of this literature relates as well to ongoing work by rational choice theorists that emphasizes the role of culture in defining "focal points" that influence which of a number of possible equilibria is actually achieved (Ferejohn 1991; Greif 1994, Bates, de Figueiredo, Weingast 1998).
arguing about" (p. 22). By defining appropriate conduct, shaping actor identities, and influencing actor interests (p. 23), collectively held norms “inform how political actors define what they want to accomplish” (ix). Katzenstein’s work draws insights from institutional sociology but it places more emphasis than most early versions of the “new” institutionalism in sociology on the political struggles that determine which norms prevail. This perspective shares many similarities with analyses by Hall (1993, 1989) and Weir (1992), both of whom, like Katzenstein, focus specifically on explaining how some ideas prevail over others and with what consequences for politics. This view resonates especially with those versions of institutional sociology that specifically incorporate considerations of power and/or legitimacy in explaining how institutions emerge and are reproduced (e.g., Fligstein 1991; DiMaggio 1988; Stinchcombe 1997).

There have thus been some rather fruitful developments at the intersection of these various schools of institutionalism, and in my view historical institutionalism has been enriched by encounters with alternative perspectives. This article provides an overview of recent developments within the historical institutional tradition. Given the vast scholarship in this area, this tour is necessarily selective. I attempt to capture the current state of the literature in two passes. First, I review the common distinctions that are drawn between historical institutionalism and alternative perspectives; the purpose is to sort out the real differences from some of the more commonly drawn distinctions. Second, I revisit one of the key “frontiers” in historical institutionalism that Sven Steinmo and I identified in a previous article (Thelen and Steinmo 1992), and assess the progress that has been made in our understanding of institutional formation and change. A concluding section evaluates where we stand on this issue, and identifies directions in which historical institutionalists might fruitfully push on.

Red Herrings and Real Issues

Theoretical versus empirical work. One of the lines that is frequently drawn between historical institutionalism and rational choice institutionalism is between “theoretical” versus “empirical” work. In a well-known critique, Green and Shapiro (1994) charge that rational choice has produced elegant theories but has generated little in the way of explanations of real observed events. From the other side, rational choice theorists have often argued that historical institutionalists are engaged in something less than theory building, they are merely stringing details together, in other words “telling stories”. Even where the distinction is not drawn so starkly, the assertion is that the difference is fundamental. Thus for example, Margaret Levi argues (in opposition presumably to historical institutionalism) that rational choice is almost always willing to sacrifice “nuance for generalizability [and] detail for logic” (1997, p. 21). In my view this is a false dichotomy, for the best work in both
perspectives is precisely concerned with generating hypotheses that are then brought to bear on empirical phenomena. A few examples will illustrate. Gregory Luebbert’s analysis of the origins of fascism, social democracy, and liberal democracy is based on a comparative analysis of (inter- and cross-) class relations in the interwar period in Europe. Combining comparative method with close historical process tracing in individual cases, Luebbert singles out as the crucial explanatory variable the issue of how the landed peasantry was mobilized politically. Where working class based parties allied themselves with the landed peasantry, this produced the mass base necessary for the establishment of social democratic regimes. By contrast, where social democrats failed to forge this alliance, the landed peasantry turned against the working class and provided the mass base on which fascism grew. One can disagree with his findings, but Luebbert’s work is an exemplary model of the testing, through the comparative method, of a strong and clear hypothesis, one that is also obviously capable of being falsified.

Another example is Ruth Berins Collier’s and David Collier’s impressive study of regime transformation in Latin America. Based on structured comparisons and historical process tracing for individual cases, they found that differences in patterns of labor incorporation were key to the type of regime that emerged. As in Luebbert’s work, this study was not meant to capture every detail; on the contrary, Berins Collier’s and Collier’s account is precisely designed to probe the plausibility of alternative hypotheses. Much is thus “lost” in terms of a comprehensive history of each country, but the payoff is a set of general propositions about the way in which labor incorporation affected subsequent regime outcomes across a number of countries.

A number of other works could be invoked, which —like those just cited— use the comparative historical method to sort out the causal mechanisms at work behind observed empirical patterns (see the discussion, below, of the “critical junctures” literature). All of them “go beyond conventional history’s preoccupation with historical particularity and aim for theoretical generalization” (Rueschemeyer, Huber Stephens, Stephens 1992, p. 4). In this, they share much with rational choice analyses as characterized by Levi above, sacrificing much detail in order to identify general causal patterns that hold across a number of countries.

Conversely, rational choice work in comparative politics arguably has become more “empirical” over time. For example, the authors of the “analytic narratives” project mentioned above specifically emphasize that the papers they present are “problem driven, not theory driven; they are motivated by a desire to account for events or outcomes. They are devoted to the exploration of cases, not to the elaboration of theory...” (1998, p. 14) Some of the analyses are comparative, others focus on single cases; however, as with good single case studies by historical institutionalists, the latter use close analysis of critical cases to illuminate important general issues.
If the difference between "historical" and "rational choice" variants of institutionalism cannot be summed up accurately in a strict dichotomy between "theoretically" versus "empirically" oriented work, there do appear to be some differences in terms of how authors in the two traditions approach the issue of theory building. First, most historical institutionalists are working at the level of mid-range theory of the sort that Bendix and others have advocated. Often, though not always (e.g., Rueschemeyer, Huber Stephens, Stephens 1992; Karl 1997), this involves focusing on a limited range of cases that are unified in space and/or time, e.g., explaining transitions to democracy in Eastern Europe (Stark 1998), or regime formation in Latin America (e.g., Berins Collier and Collier 1991), or the effects of new international pressures on the political economies of the advanced industrial democracies (Hall 1993). Rational choice theorists, by contrast, sometimes aspire to produce more general (even universal) theoretical claims, or use historical examples not so much for their intrinsic importance (e.g., as "critical cases") but to demonstrate how widely applicable are the theoretical claims (e.g., Knight 1992; Levi 1988; Tsebelis 1990). Still, the object of both is to test theoretical propositions against observed phenomena as a way not only of explaining the cases at hand, but of refining the theory as well.

A second difference lies in the different ways that historical institutionalists and rational choice institutionalists approach the issue of hypothesis formation. Very frequently, historical institutionalists begin with empirical puzzles that emerge from observed events or comparisons: why did the policies of the advanced industrial countries differ so much in response to the oil shock of 1973 (Katzenstein 1978)? Why have some industrial relations systems proved more stable than others in the face of globalization pressures (Thelen 1996)? Why do some countries tax and spend more than others (Steinmo 1993)? The analyst then uses the comparisons to test hypotheses that can account for the observed differences.

Rational choice theorists often proceed somewhat differently, deriving their puzzles from situations in which actual observed behavior appears to deviate from what the general theory would lead them to expect: why, given free rider problems, do workers join unions (Wallerstein 1989)? Why would unions lead workers into hopeless battles (Golden 1996)? Why would citizens ever volunteer for war (Levi 1998)? This is what accounts for the extensive use of counterfactuals in rational choice research (e.g., Bates, de Figueiredo, Weingast 1998), which function in much the same ways as cross-national comparisons do in much historical-institutional research. The question is: what is "off the equilibrium path"? (Levi, p. 31); the theory is what gives us prior expectations that we can then hold against the observed phenomena.

Although again, not always, and this tendency may in fact be weaker among comparativists. For example, those who study the political economy of the advanced industrial countries (e.g., Fritz Scharpf and David Soskice) tend to formulate their hypotheses (and conclusions) at the same, mid-range, level as most historical institutionalists.
Thelen/Historical institutionalism in comparative politics

outcomes. Fritz Scharpf characterizes the contribution of rational choice in precisely these terms, that is, in how it disciplines the analysis of empirical events by providing the basis for generating hypotheses: "our search for explanations must be disciplined by strong prior expectations and we must take the disconfirmation of such explanations as a welcome pointer to the development of more valid explanations" (1997, p. 29).

The point is that it is not the case that in one perspective the analysis is guided by clear hypotheses and not in the other. Rather, the issue is where these hypotheses come from. Moreover, this is not a hard and fast rule. As Scharpf points out, rational choice theories seek "to explicate what the authors of 'good' case studies always have in the back of their minds: a 'framework' that organizes our prior (scientific and prescientific) knowledge about what to expect in the province of the world that is of interest to us, that emphasizes the question that are worthwhile asking, the factors that are likely to have high explanatory potential, and the type of data that would generally be useful in supporting or invalidating specific explanations" (1997, pp. 29-30). Moreover, there is much overlap between the two when it comes to testing the hypotheses, once generated, against the empirical cases, for this invariably involves contextualizing the theory (assumptions and propositions) and also demonstrating that the hypothesized processes are actually at work (Pierson 1996, p. 158). As Scharpf puts it: "We need to have hypotheses that specify a causal model showing why and how a given constellation of factors could bring about the effect in question", but equally, "we need to have empirical evidence that the effect predicted by the hypothesis is in fact being produced" (1997, p. 28).

There is, in other words, no dichotomy between theoretical and empirical work because good analyses have to do both. The generation of the hypotheses is not the analysis, though it is the vital starting point for engaging the empirical material. The utility of a theory, after all, cannot be assessed apart from the empirical material it is meant to explain.

Preferences: Problematical or not. Writing in 1992, Sven Steinmo and I argued that "one, perhaps the core, difference between rational choice institutionalism and historical institutionalism lies in the question of preference formation, whether treated as exogenous (rational choice) or endogenous (historical institutionalism)" (1992, p. 9; emphasis in original). I now think this may be a less stark difference than before. Setting aside some important ambiguities on this issue in both the rational choice and the historical-institutionalist literature, it is certainly

4 Within the rational choice literature that deals with norms, for example, there is some ambiguity on whether norms affect belief formation or preference formation, the difference being that "beliefs" simply refer to how you think others will behave, while "norms" are collectively shared convictions (see Ferejohn 1991, Bates et al 1998, Levi 1997). (I am indebted to Guillermo Trejo for pointing this out to me.) Among historical institutionalists, Ellen Immergut argues that the claim that institutions shape preferences has often conflated several analytically separate issues, and she for one prefers "to distinguish more sharply between preferences, interests and choices" and to focus on
the case that one of the core claims of historical institutionalism from the beginning was that institutions do more than just channel policy and structure political conflict; rather, "the definition of interests and objectives is created in institutional contexts and is not separable from them" (Zysman 1994, p. 244). In almost any version this is quite different from strong versions of rational choice theory, which begin with a (universal, not context specific) rationality assumption.

Or is it? As Margaret Levi has suggested, in rational choice "the trick is in defining the preferences in general, ex ante to a particular application" (1997, p. 24). She notes that this is frequently quite difficult, for example, in the case of citizens, "given the range of interests [they] might have; there is nothing comparable to the economics dictum of getting the most for the least for one's money in the marketplace" (1997, p. 24). In a candid discussion of the problem of imputing preferences, she notes the danger that assuming utility maximization "can produce tautology: Whatever people do becomes a 'revealed preference'".6

It seems that even the assumptions that we have traditionally viewed as "safe" may be trickier than we thought, the most common perhaps being that politicians are seeking re-election. This cannot be valid for Mexico, where the rules of the game (read: the specific institutional configuration) rule out re-election of legislators, by law. If you start the analysis with this assumption you will be widely off the mark. How do rational choice analysts deal with this situation? By and large they cope with it by trying to figure out, within a given context, what would make sense for a politician to seek, which is to say that they contextualize the preferences based on the particular institutional incentives that these politicians face in different settings. This is not so different from how historical institutionalists often proceed.

interests as "publicly expressed, organized demands" rather than preferences in the sense of the views that individuals hold. She argues that "most institutionalists focus on how institutions may foster the emergence of particular definitions of mutual interest, or advantage particular political choices, without necessarily re-socializing citizens in a fundamental way" (1997, p. 24).

In economics, theories of the firm have been more successful than theories of consumption for this very reason. The assumption that firms seek to maximize profits is much more tangible, concrete, and therefore useful, than the assumption that consumers seek to maximize "utility" (whatever that might be). The quality of the theory appears to be a direct function of the degree of specificity of the core assumptions. I am grateful to Guillermo Trejo for this point. See also the discussion in Ferejohn and Satz 1994, esp. p. 72.

6 Levi, 1997, p. 24; see also Weingast, as well as the growing literature — e.g., Kuran — that shows that it is in fact impossible to distinguish what types of behavior reflect true preferences and what is the result of strategic action. Bo Rothstein (1996, p. 148) also discusses this at some length.

7 Ditto for presidents, and across a much larger number of countries.

8 For example, we can revise the re-election assumption and say that politicians are seeking to maximize their careers or trying to increase their power, but again what that means depends on the context, among other things, where power is located (institutionally) in different systems. For example, given the historical weakness of Congress in Latin America, "maximizing power" for a legislator there may mean angling for an appointive job in the bureaucracy. This is why rational choice theory works best in highly structured settings in which the players and the rules are stable.
In all these cases, the move from general propositions about what political actors are seeking to maximize inevitably brings the theorist face to face with the question of what it means to, say, maximize power within a given context. Until this step is complete, then the analysis really cannot begin. This seems to be what Bates, et al. (1998, p. 35) mean when they note that “the complementarity of the ["rational" and "interpretive"] approaches is revealed by the "cultural" knowledge required to complete a rational choice explanation. Game theorists often fail to acknowledge that their approach requires a complete political anthropology. ... In rule-governed political settings, it requires detailed and fine-grained knowledge of the precise features of the institutions within which individuals make choices and devise political strategies”.

In addition, the issues of norms and culture, which for a long time were more concerns for historical institutionalists and institutional sociologists, appears to be assuming an increasingly important role in rational choice analysis as well. Ferejohn’s recent work, for example, argues that “culturally shared understandings and meanings” are crucial to selecting among the many possible strategic equilibria (1991, p. 285). He argues that “in social action, human agents make strategic or allocative choices while simultaneously enacting (ontologically) prior understandings about the nature of the strategic situation in which they find themselves, the characteristics of identities of the players (including themselves), and the common understandings or expectations as to how the game will be played. Thus, when it comes to explaining action, rational accounts, no less than interpretive ones, must appeal to principles external to the individual agents” (1991, p. 285).

There is some variation in how different authors think about the role of norms and/or culture: some see cultural symbols and norms as resources invoked in strategic interactions (e.g., Johnson); others view them as signaling devices in games of incomplete information (e.g., Bates); still others as focal points that affect which of a number of possible equilibria prevails (Laitin, Rogowski, Greif). Without going into these differences, here it is sufficient to note that for norms to operate in any of these ways, they have to exert some independent power over individual behavior, and in this sense all of these works go well beyond traditional “instrumental rationality” assumptions. In light of such developments, it seems that the issue of preferences (exogenous/endogenous) no longer provides a clear line of demarcation between the different approaches.

and well known (Satz and Ferejohn 1994, p. 81; Bates 1997, p. 704, Bates, de Figueiredo, and Weingast 1998, p. 2), in other words, where the structure itself does some of the analytical work.

Actually, this goes back to Schelling (1960), and Elster (1989) has also long been concerned with issues of norms and culture. More recently they have been joined by a host of others including Ferejohn 1991, Ostrom 1995; Bates 1988, Gambetta 1988; Bates, de Figueiredo and Weingast (1998), Levi n.d.; 1997 and 1998; North 1990, Greif 1994; Crawford and Ostrom 1995. See also the Comparative Politics Newsletter (Summer 1997), especially the contributions by Bates, Johnson, Laitin, Rogowski.
Micro foundational versus macro historical research. The issue of micro foundations is a third one that is typically cited as distinguishing historical institutional research from rational choice. The idea is that aggregate outcomes need to be understood in term of the actions and behavior of individuals behaving strategically. This is frequently contrasted to broad macro historical research that either sees interests as structurally generated in one way or another, or stays at the level of aggregations (such as class) without regard for the way in which the strategic actions of individuals figure into the aggregation process (or both).

But this too is a false dichotomy. The issue of the “embeddedness” of interests has just been discussed; however, there is the additional question of how to think about aggregate behavior. “Micro foundational” obviously does not mean that one cannot deal with collectivities, since of course most rational choice work deals with collective actors of one sort or another. The analysis remains, nonetheless, “actor-centered” (as Scharpf puts it) in the sense that the players are defined as “any individual or composite actor that is assumed to be capable of making purposeful choices among alternative courses of action” (1997, p. 7). In some cases, this is rather unproblematical, as for example, in Scharpf’s case since for example, unions (or at least, the union leadership) and not individual workers are the relevant “players” who actually engage in the strategic bargaining that generates the policy outcomes he is interested in. In other cases, however, it is worthwhile questioning whether the collectivities to which strategic action is attributed in fact constitute “players” in the sense that Scharpf identifies.

Take the example of Rogowski’s analysis of coalitional alignments and realignments in the late nineteenth century (1987). Rogowski’s analysis hinges on the interaction of “land” (agricultural interests), “labor” and “capital”, aggregations that are seen to be acting purposefully and strategically in the face of changing conditions in international trade. To be persuasive we need to do more than impute (actor-centered) motives and strategies to these aggregations; we have to demonstrate that these actors are in fact “players” in Scharpf’s sense, that is to say, that these aggregations were cohesive and strategic. But we know from historical work — and indeed from the logic of Rogowski’s own analysis — that these

10 See also the introduction to Analytic Narratives which says that while their approach is more micro than macro (p. 16), these are complementary not competing approaches. In addition, Ferejohn and Satz (1994) argue that “rational-choice explanations are most plausible ... where the theory gets its explanatory power from structure-generated interests and not from actual individual psychology” (p. 72).

11 Scharpf’s work on economic policy outcomes in the face of the 1973 oil crisis, for example, examines the interaction of “unions”, “employers”, and “the government”. Miriam Golden explains apparently suicidal struggles on the part of unions by analyzing the behavior of “union leaders”, “employers”, and the “rank and file”. Barry Weingast examines the incentives faced by “southern states” in their interaction with “the North”. In all these cases, the relevant players are aggregations of individuals, that is to say, collectivities.
aggregations are not coherent players, each, rather, being riven by internal tensions that derive from precisely the changes in international trade that are the focus of Rogowski's analysis. Rogowski's argument itself suggests that business interests should be very much divided, depending on whether firms were oriented toward the domestic market or the international market. And we know from historical work that this was in fact the case. Similarly for labor, workers' interests frequently followed the divisions within business, which is what opened the door for "cross-class alliances" that were crucially important to the outcomes that Rogowski is interested in (Gourevitch 1986, Swenson 1989). Peter Swenson's deeply inductive, historical approach is clearly more micro foundational than Rogowski's (rational choice approach) in the sense that Swenson has gotten much closer to the actual players whose strategic interactions produced the outcomes at issue.

There is no dichotomy because taking micro foundations seriously means that we cannot be content to impute coherence to actors identified by the analyst; we have to do the empirical work to make sure that the actors to whom we attribute certain strategic behaviors were or are in fact "players" in the first place. Another way to put this is that just because a particular analysis employs a rational choice perspective does not necessarily mean that it stands on strong micro foundations; conversely, historical institutional research is not necessarily not micro foundational; quite the contrary, as we have seen.

Functional versus Historical view of institutions. It has frequently been noted that, unlike most historical institutionalism, a good deal of rational choice theory embraces a functional view of institutions (e.g., Hall and Taylor, 1996, pp. 943-944; Pierson 1996). The origins of this difference very likely go back to the different ways that the two schools approach the issue of institutions in the first place. John Zysman has put this succinctly when he notes that "rational choice institutionalists start with individuals and ask where institutions came from, whereas historical institutionalists start with institutions and ask how they affect individuals' behaviors". Indeed, much of the early and pathbreaking work in rational choice did in fact pose the question in the way that Zysman suggests, and the answer to the question of why institutions emerge and are sustained has been answered in terms of the functions that institutions perform. One example is the literature on the US Congress and the way in which congressional rules eliminate "cycling"; another is the rational choice literature on institutions in international relations, which are viewed as mechanisms by which states can reduce transaction costs and achieve joint gains in an anarchic world (e.g., Shepsle and Weingast 1981; Shepsle 1986; Moravcsik 1993).

However, as Paul Pierson has pointed out, there has been much work within rational choice that, like historical institutionalism, embraces a non-functionalist, much more historical view of institutions (1996, p. 131). North's later work (1990), for example, is precisely concerned with tracing, historically, the emergence of
different kinds of institutional arrangements that either promote or distort development. Knight too has criticized a good deal of rational choice literature for embracing either an evolutionary or a spontaneous creation view of institutions (1992: chap.1). Against more functionalist accounts, he sets his own model of institutional formation and change which places issues of distributional conflict at the center of the analysis.¹²

These works share much with the more historical view of institutions embraced by historical institutionalists. Quoting Zysman on political-economic institutions: “The institutional approach begins with the observation that markets, embedded in political and social institutions, are the creation of governments and politics” (1994, p. 244). This is close to the view that Knight embraces, and for that matter, that March and Olsen embraced; none of these works make any assumptions about the social efficiency of institutional arrangements, and all of them allow for suboptimality and inefficiency. Differences remain, of course. Here I simply wish to point out that these differences do not boil down to the commonly cited divide between rational choice’s “functionalist” and historical-institutionalist’s more “historical” approaches to institutions.

Synthesis or Creative Borrowing? So where does this leave us? Should we conclude from the above that there has been a blending of these different approaches? In my view, no. What we see is a partial convergence in the issues at stake—for example as historical institutionalists have come to a deeper appreciation of micro foundations and problems of collective action, and as rational choice theorists have come to treat preferences, norms, and beliefs as a more central (also more complicated) issue than heretofore.¹³ But, as pointed out above, differences remain: in how theorists working out of these different traditions approach these issues, in how they generate the hypotheses that guide their work, and in the level at which they attempt to build theory, for example.

Rather than a full-fledged synthesis, we might instead strive for creative combinations that recognize and attempt to harness the relative strengths of each approach. This seems to be the strategy advocated by prominent proponents within each school, John Zysman from an historical institutionalist perspective, and Fritz Scharpf from a rational choice perspective. Thus, for example, Zysman argues that

¹² Moreover, in some cases a functionalist view of institutions seems perfectly warranted, because it is consistent with the historical record. In his study of the "balance rule" in antebellum America, for example, Barry Weingast provides evidence that this institution was precisely designed to perform the functions he identifies (pp. 28-30).

¹³ I am tempted to argue that the divide between rational choice and historical institutionalism has given way to a new (well, really an old) divide that cuts across both, between materialist-oriented analysis (in historical institutionalism exemplified in the work of Karl, Swenson, Immergut, Katzenelson and others; in rational choice in the work of Knight, Tsebelis, and others) versus work oriented toward ideational issues and norms (in historical institutionalism, recent work by Katzenstein and Hall; in rational choice, recent work by North and Levi).
"institutions and broad processes of social change certainly have micro-foundations. The ‘naked’ institution emerging from a state of nature by rational choice and the ‘socially embedded’ institution are one and the same, but they represent two different narratives whose perspectives highlight different processes within a common story. That is, the arguments built around institutions and historical dynamics should be consistent with notions of the ‘rational’ dynamics of individual behavior. Inconsistencies are instructive to both those who would build micro-foundations and macro-theories” (1994, p. 277). He likens the difference between the two perspectives to that between “high-level computer languages (historical narrative) and the bit-level machine language of the computer (microeconomic narrative)”, and maintains that “inherently they must work together, they must be consistent”, (1994, p. 277) and that “issues must be segmented to make appropriate use of the perspectives, not to reject the insight of one or the other as part of an ideological quarrel” (1994, p. 278; see also Ostrom 1995, esp. pp. 177-178 and Ferejohn 1991).

Scharpf (1997) elaborates in some detail how this might be achieved. He advocates the use of rational choice and game theory as a way of generating hypotheses that can discipline empirical analysis but acknowledges that “even when we can rely on models with high predictive power, they are likely to be of limited scope and will only represent certain subsets of the complex, multi arena and multilevel interactions that are characteristic of real-world processes” (1997, p. 31). This being the case, he argues that “it is usually necessary to combine several such modules into a more complete explanation” (1997, p. 31). The composite explanation of particular processes “is likely to be unique for each country but ...the modules employed in constructing it may reappear more frequently in other cases as well and thus are more likely to achieve the status of empirically tested theoretical statements. Even then, however, the linkages between these modules remain problematical.... Thus we will often depend on narrative, rather than analytical, connections between partial theories that have analytical as well as empirical support —which also means that the composite explanation itself remains vulnerable to charges of being ad hoc”. (1997, p. 32).

**Equilibrium order versus Historical process.** There is one area that nonetheless seems to distinguish these two analytic approaches, at least in emphasis, and this can be characterized in terms of the relative centrality of “equilibrium order” versus “historical process” in the analysis of political phenomena. One of the defining features of rational choice institutionalism is its assumption of
equilibrium and its view of institutions as coordinating mechanisms sustaining these equilibria (Levi 1997, p. 27; Scharpf 1997, p. 10; Shepsle 1989, p. 145). As Levi has emphasized, this does not imply an efficient equilibrium and indeed much of rational choice has been concerned with sorting out the problem of why it is that individual actors, behaving rationally, often produce suboptimal or inefficient collective outcomes. Nor does it imply that there exists a single, unique equilibrium outcome; again a central problem in rational choice is to understand the process through which one equilibrium rather than another is arrived at. And finally, this does not mean that rational choice theorists are uninterested in political change; it is just that they tend to treat this as involving a transition between equilibrium orders. Karen Orren and Stephen Skowronek put it this way: "institutional politics appears as 'normal,' as politics as usual, explicitly or implicitly opposed to an extraordinary politics, in which equilibria are upset, norms break down, and new institutions are generated" (1994, p. 316). Bates, de Figueiredo, and Weingast appear to concur, also with the implicit critique: "The greatest achievement of rational choice theory has been to provide tools for studying political outcomes in stable institutional settings...Political transitions seem to defy rational forms of analysis," (1998, p. 3) (and they address this problem by incorporating elements of an "interpretivist" approach into the analysis).

Where rational choice tends to view institutions in terms of their coordinating functions, historical institutionalists see institutions as the legacy of specific historical struggles. In embracing this view, historical institutionalism, as Orren and Skowronek point out, "brings questions of timing and temporality in politics [rather than equilibrium order] to the center of the analysis of how institutions matter" (1994, p. 312). This does not mean that historical institutionalists are uninterested in regularities and continuities in politics; it just means that the emphasis tends to be on political development as a (structured) process (e.g., Steinmo 1993, 1994). As Pierson puts it, historical institutionalism "stresses that many of the contemporary implications of...temporal processes are embedded in institutions — whether these be formal rules, policy structures, or norms" (1996, p. 126; see also Skocpol 1992, p. 58-59).

Orren and Skowronek emphasize two features of political life that have been central to historical institutional analyses. The first is that "institutions, both individually and collectively, juxtapose different logics of political order, each with

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15 Their critique of this approach bears some resemblance to Sven Steinmo’s and my critique of a punctuated equilibrium model (1992, p. 15), where we argue that, "at the moment of institutional breakdown, the logic of the [traditional] argument is reversed, from ‘institutions shape politics’ to ‘politics shape institutions’". There, we maintained that "conceiving of the relationship in this way...obscures the dynamic interaction of political strategies and institutional constraints...and the interplay of the two variables over time”.

16 As the large literature on "comparative statics" demonstrates; see the discussion in Thelen and Steinmo (1992); see also the discussion of the “feedback effects” literature below.
their own temporal underpinnings" (1994, p. 320). That is, the various institutional arrangements that make up a polity emerge at different times and out of different historical configurations. For this reason, the various "pieces" do not necessarily fit together into a coherent, self-reinforcing, let alone functional, whole. To give an example: some analysts treat the German political economy as a well-integrated "system" in which various institutional subsystems —vocational education and training, collective bargaining institutions, financial institutions, bank-industry links— form a mutually reinforcing whole. Historical institutionalists, by contrast, are more likely to be concerned with the origins rather than the functions of the various pieces, and indeed, historically oriented research has demonstrated that the evolution of the German model was highly dyssynchronous and full of unintended consequences (Streeck 1992, Manow 1997 Thelen and Kume 1997). This has important implications for how we view the operation of this "system". Wolfgang Streeck, for example, has drawn attention to the ways in which industrial relations institutions actively generate problems and pressures in other parts of the system, especially vocational education and social welfare institutions. Like in the more functionalist view, the interdependencies among the various parts of the system are central to the analysis; the difference is that in Streeck's work the frictions as well as the functional interdependencies come to the fore. The result is very much in the spirit of Orren and Skowronek's characterization: "The single presumption abandoned is that institutions are synchronized in their operations or synthetic in their effects; the more basic idea, that institutions structure change in time, is retained" (1994, p. 321).

The second claim, related to the first, is that one important source of change comes from the way that different institutional orders within a society interact, as "change along one time line affects order along the others" (Orren and Skowronek 1994, p. 321) or, put somewhat differently, as interactions and encounters among processes in different institutional realms open up possibilities for political change.17 David Stark's work on the transition to democracy and market economy in Eastern Europe provides an example of what this looks like in practice. In language that very much resonates with Orren's and Skowronek's, he argues that in Eastern Europe, "we see social change not as a transition from one order to another but as transformation —rearrangements, reconfigurations, and recombinations that yield new interweavings of the multiple social logics ..." (1998, p. 7). And like Orren and Skowronek, Stark stresses the incongruities among the multiple processes as they unfold: "... within any given country, we find ... many [transitions] occurring in different domains —political, economic, and social— and the temporality of these

17 This is what Weir (1992) draws attention to in her analysis of "collisions" between different policy streams; it is also related to Pierson's (1996) notion of "gaps" and "lags" in policy processes that produce openings for institutions to evolve in ways unanticipated by their designers. More on this below.
Thelen/Historical institutionalism in comparative politics

processes is often asynchronous and their articulation seldom harmonious" (1998, p. 81). Change in one arena affects other ongoing processes, which is what drives institutional evolution.

Paul Pierson’s analysis of the evolution of social policy in the European Union provides further examples. In one instance (the case of EU policy on gender equality), he shows how provisions adopted by the EU member states in one period—largely symbolic and without much effect—were later picked up by emergent women’s groups who were able to use these provisions to achieve gains at the EU level that had eluded them at the domestic level. Here we have a situation in which changes in the political and socioeconomic context brought new actors into the game (in Pierson’s case, women’s groups) who were able to use existing but previously latent institutions (in Pierson’s case Article 119 of the Treaty of Rome), whose new salience had important implications for political outcomes (Thelen and Steinmo 1992, p. 16).

These examples point to the importance of examining politics as a dynamic process, one that also inherently produces myriad unintended consequences as different, ongoing processes interact. Those perspectives that conceive of change as the breakdown of one equilibrium and its replacement with another do not capture this well. Nor, however, do other, alternative conceptions, for example, some versions of the “new” institutionalism in sociology in which the definition of institutions as “shared cultural scripts” blends out political struggles over which norms prevail and suggests a uniformity across the institutions of a society that make the notion of politically consequential “collisions” among different institutional orders literally impossible to conceive.

Thus, while historical institutionalists are just as interested as “other” institutionalists in the regularities of politics over time, they tend to emphasize historical process over equilibrium order, or perhaps more precisely, they tend to eschew a view of institutions as coordinating mechanisms in favor of one that

18 Compare to Orren and Skowronek’s formulation: “political institutions, both singly and in their interactions, characteristically manifest ordering patterns that are conflicting and contradictory” (1994, p. 321).

19 The concept of “institutional bricolage” which Stark and other students of Eastern Europe employ describes “an innovative process whereby new institutions differ from but resemble old ones” (Campbell 1997, p. 22; Campbell and Pedersen 1996). This echoes Orren’s and Skowronek’s point that “more often than not, we expect to find that continuities along one dimension of order and time will be folded into, and formative of, the extraordinary changes we are observing along another” (1994, p. 322).

20 Carol Heimer has criticized this work on similar grounds. Her fascinating study of neonatal intensive care units revealed not a single cultural “logic” but rather three separate sets of institutions (revolving around the formal/legal and normative claims of families, of the state, and of the medical community) that are also in more or less direct competition with each other. Her analysis illuminates the political factors that resolve these conflicts by systematically suppressing certain claims and amplifying the effects of others.
embeds institutional outcomes in concrete historical processes. This view of institutions understands path dependency as involving elements of both continuity and (structured) change; institutions are conceived in relational terms (Immergut 1992; Katznelson 1997, p. 104) and institutional arrangements cannot be viewed in isolation from the broader political and social setting in which they are embedded. This brings me to the second part of this essay, namely historical institutional approaches to path dependency.

**Path Dependency**

Two ways of thinking about path dependency—one coming out of the literature on economics and technology, the other out of the work of “new” institutional sociologists—have gained some prominence, and a brief discussion of each can provide a baseline for a discussion of the historical-institutional approach to path dependency. To preview my conclusions, I argue that both contain some insights into the mechanisms that sustain particular patterns of politics, but that some of the most prominent formulations tend to obscure the distributional consequences of political institutions and blend out important sources of dynamism in political life.

*Technological models of path dependency from economics.* The most widely invoked model of path dependency is the one that comes out of the work of economists seeking to understand technological trajectories. Mostly closely associated with the “QWERTY keyboard”, the argument developed by Paul David (1985) and elaborated by Arthur (1989) holds that certain technologies, for idiosyncratic and unpredictable reasons, can achieve an initial advantage over alternative technologies and prevail despite the fact that in the long run the alternatives would have been more efficient. The logic of the argument is perhaps well known enough to forego a more thorough treatment here (Krasner 1988; Kato 1996 and Pierson 1998 all review these arguments in detail). What political scientists have taken from this is the intuitively attractive idea that technology, like politics, involves some elements of chance (agency, choice), but once a path is taken, then it can become “locked in”, as all the relevant actors adjust their strategies to accommodate the prevailing pattern. For example, in the case of the QWERTY keyboard, firms wishing to make a profit have every incentive to conform to the prevailing standard rather than buck it.

There is no doubt that some features of politics are subject to the kinds of lock in effects to which the David/Arthur model of technological change draws attention, and—as I will discuss in more detail below—the notion of increasing
returns certainly has important applications to politics. But as a general guide to understanding political development, the model is both too contingent and too deterministic. It is too contingent in that the initial choice (call it a “critical juncture”) is seen as wide open, when in politics this kind of blank slate is a rarity, to say the least. Moreover, the emphasis in this model on the role of chance (the unpredictability of the path) is belied by the vast literature on critical junctures (discussed below) that traces divergent trajectories not to random events but to systematic differences either in antecedent conditions or in the timing, sequencing, and interaction of specific political-economic processes.

The QWERTY model is also too deterministic in that once the initial choice is made, then the argument becomes mechanical. There is one fork in the road, and after that, the path only narrows. The model correctly emphasizes coordination and learning effects, but does not deal well with the distributional effects of political institutions (Knight, Hall, Pierson). In the technology model, actors adapt to prevailing institutions or standards in the sense that they adopt and invest in them (people learn to type in a particular way, firms make products that fit with the standard) in ways that reinforce the institution. In other words, in the world of firms and users and technology, adapting to the standard means adopting it, and those who do not adapt lose and —importantly— the losers disappear (for example, as firms go out of business).

Politics of course is characterized by disagreement over goals and disparities in power, and in fact institutions often reinforce power disparities (Hall 1986, Knight 1992, Riker 1980, pp. 444-445). Moreover, the losers do not necessarily disappear and their “adaptation” can mean something very different from “embracing and reproducing” the institution, as in the technology model. For those who are disadvantaged by prevailing institutions, adapting may mean biding one’s time until conditions shift, or it may mean working within the existing framework but in pursuit of goals that are quite different from —perhaps even subversive to— those of the institution’s designers. As we will see below, such considerations provide

21 The best treatment of this is in Pierson (1998), who among other things demonstrates the necessary caution in applying the concept (by specifically addressing the question of how political institutions differ from market institutions).

22 I do not wish, of course, to deny entirely the role of chance in politics; it does however seem that, at least in terms of the establishment of broad (and, cross-nationally, broadly different) institutional configurations, the patterns that emerge are systematic and not random. Indeed, this is what the “critical junctures” literature (discussed below) is all about.

23 An example of the latter is the job classification system in American industrial relations. This system was originally imposed by employers against unions (as a way of controlling labor). Unable to change the system, emergent unions adapted their strategies to it, but sought to attach rules to these job classifications, and in doing so, they eventually turned it into a system of union control. Here we see a case in which “adapting” to the institution had the effect of transforming it altogether, so much so that it is employers who are now attacking the system, unions defending it.
insights into the reasons why, in politics, increasing returns do not necessarily result in an irrevocably “locked in” equilibrium; further choice points exist.

**Institutional Sociology.** Another strong argument about path dependency comes out of the work of the “new” institutionalism in sociology. Where economic models start with individuals or firms in the market, sociological perspectives begin with society. Institutions, in this view, are collective outcomes, but not in the sense of being the product or even the sum of individual interests. Rather, institutions are socially constructed in the sense that they embody shared cultural understandings (“shared cognitions”, “interpretive frames”) of the way the world works (Meyer and Rowen 1991; Scott 1995, p. 33; Zucker 1983, p. 5). Specific organizations come and go, but emergent institutional forms will be “isomorphic” with (i.e., compatible, resembling, and similar in logic to) existing ones because political actors extract causal designations from the world around them and these cause-and-effect understandings inform how they approach new problems (DiMaggio and Powell 1991, p. 11; Dobbin 1994). This means that even when policy makers set out to redesign institutions, they are constrained in what they can conceive of by these embedded, cultural constraints.

The strong emphasis on cognition in the “new” institutionalism in sociology gives us some powerful insights into the persistence of particular patterns of politics over time, but as DiMaggio and Powell point out, the early formulations (e.g., Meyer and colleagues) were less helpful in understanding change (1991, esp. 1, pp. 11-12). Some versions in fact make it very hard to see any forks in the road at all, for example, Zucker (1991) who argues that “...each actor fundamentally perceives and describes social reality by enacting it, and in this way transmitting it to the other actors in the social system...The young are enculturated by the previous generation, while they in turn enculturate the next generation. The grandparents do not have to be present to ensure adequate transmission of this general cultural meaning. Each generation simply believes it is describing objective reality” (1991, p. 85).

The notion of institutions as “shared scripts” sometimes obscures conflicts among groups (because the scripts are by definition shared), and the notion of “isomorphism” emphasizes continuity across time and space (because new problems are solved using the same cultural template). Yet we know from the work of Katzenstein (1996) that dominant cultural norms emerge out of concrete political conflicts in which different groups fight over which norms will prevail, from Hall’s work that dominant policy paradigms can and do shift at times, from Fligstein’s and DiMaggio’s work that organizational fields are often imposed by powerful actors, and from Stinchcombe’s work (1997) that legitimacy and not automaticity hold the key to why people follow scripts in the first place. This is why more recent

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24 Their book, *The New Institutionalism in Organizational Analysis* was partly a response to these weaknesses. See also Powell and Jones, eds. (forthcoming) which explicitly deals with institutional change.
formulations argue that while cognitive dimensions are important, they should not eclipse the strategic and political elements of action, and frequently find that when it comes to addressing questions of institutionalization and institutional change, the political part of the story (and not the cognitive) is most "germane" (DiMaggio and Powell 1991, pp. 27, 31; also Katzenstein 1996).

Both of the perspectives discussed above—the economic-technological and the institutional-sociological—provide some strong tools for understanding continuity, but by stipulating and privileging particular mechanisms of reproduction (coordination effects for the former, isomorphism for the latter) they have a hard time incorporating notions of conflict and power, and they are not particularly helpful in talking about change. Dynamism in both has to come from some exogenous shock or, as Orren and Skowronek argue for equilibrium models generally, these perspectives strongly imply that political change is not amenable to the same type of analysis we use to understand the operation of the institutions themselves (1994).

Path dependency in historical institutionalism

John Ikenberry captures nicely the essence of an historical-institutional approach to path dependency with his characterization of political development as involving "critical junctures and developmental pathways" (1994, p. 16ff). As the phrase itself implies, this involves two related but analytically distinct claims. The first involves arguments about crucial founding moments of institutional formation that send countries along broadly different developmental paths; the second suggests that institutions continue to evolve in response to changing environmental conditions and ongoing political maneuvering but in ways that are constrained by past trajectories. These two lines of argument tend to be reflected in a bifurcation of the literature in this area. Thus, we have a number of important analyses of critical junctures that explore the origins of cross-national diversity (e.g., Skocpol 1979; Berins Collier and Collier 1991, among others), and we have studies that focus more on understanding the logic and self-reinforcing properties of particular national trajectories over time, drawing out comparisons to other countries where relevant (e.g., Weir 1992, Skocpol 1992).

Or, as Krasner puts it, once a path is taken this "canalizes future developments" in the sense that institutional structure "delimits a range of possible responses to environmental incentives but does not necessarily determine any particular path" (Krasner 1989, p. 80). In other words, "critical junctures" in a country's development establish broad trajectories that are difficult to reverse, but within which further choice points exist (also Levi 1997, p. 28).

There are studies that do treat both the cross-national differences and the over-time continuities within countries. I would single out Esping-Andersen (1990), Steinmo (1993), and Pierson as especially good examples, though there are others.
Although obviously related, these two literatures have characteristic strengths and weaknesses, and I argue that both would be much enriched by a more sustained engagement of the other. The great strength of the critical junctures literature lies in the way that scholars have incorporated issues of sequencing and timing into the analysis, looking specifically at the different patterns of interaction between ongoing political processes, and the effect of these interactions on institutional and other outcomes. Where this literature has generally been weaker is in specifying the mechanisms through which the critical junctures they identify are translated into lasting political legacies. Here the policy feedback literature—which has given us many insights into the kinds of mechanisms that account for continuity over time—is very useful. However, in this second literature, strong tools for understanding continuity are not matched by equally sophisticated tools for understanding (path dependent) evolution and change. Combining the two points, I argue that greater insight into the different types of reproduction mechanisms behind different institutional arrangements holds the key to understanding what particular kinds of external events and processes are likely to produce political openings that drive institutional evolution and change of the variety that Hay (1997) calls "nonfoundingal". I develop these points one by one in the next three sections.

**Historical Institutional Analyses of Critical Junctures**

As Katznelson (1997) suggests, the macro-historical analysis of critical junctures that set countries along different developmental paths has long been the bread and butter of historical institutionalism. Rejecting a functionalist view of institutions, historical institutionalists see institutions as enduring legacies of political struggles. The classics in this genre—which have been ably reviewed by others (e.g. Ikenberry 1994)—include Moore, Gerschenkron, Lipset and Rokkan (1967), and Shefter. All of these works emphasize issues of sequencing and timing and, related to this, different patterns of interaction between ongoing political and economic processes in the formation and evolution of institutional arrangements. These studies are "configurative" as Katznelson puts it, in the sense that they attend explicitly to the way that political processes cannot be understood in isolation, but rather, and specifically, in terms of how different processes interact with each other in ways that influence outcomes, in these cases, crucial institutional outcomes.27

27 However, I would disagree with the distinction that Katznelson draws between "configurative" and "variable centered" analyses. I understand one of the great strengths of these works to be the way these authors frame the key variables in a way that captures the interactive nature of these processes.
This venerable tradition is alive and well in historical-institutional research, as a review of a few select works demonstrates. First, Ruth Berins Collier’s and David Collier’s *Shaping the Political Arena* is a landmark study that links differences in patterns of labor incorporation to variation in party and regime outcomes across a wide range of Latin American countries. As in the earlier works cited above, Berins Collier and Collier emphasize the central importance of sequencing and timing; as they put it, their study “confront[s] the interaction between a longitudinal and a cross-sectional perspective: between the unfolding over time within each country of phases of political change, and a sequence of international developments that influenced all the countries roughly in the same chronological time, but often at a different point in relation to these internal political phases” (1991, pp. 19-20). In fact, one of the central themes in the book is the way in which “common” international events or trends translate into quite different kinds of challenges in different countries as a result of how they intersect and interact with ongoing domestic processes.

Another example of this kind of work is Thomas Ertman’s *Birth of the Leviathan* which traces the origins of state institutions across a broad range of European countries from the 12th to the 18th century. Employing a logic that parallels Gerschenkron, Ertman argues that “differences in the timing of the onset of sustaining geopolitical competition go a long way toward explaining the character of state infrastructures found across the continent at the end of the 18th century” (1997, p. 26). Like Berins Collier and Collier, Ertman attends to variation in the ways in which common international forces intersect with ongoing domestic political developments. Where state-builders faced geopolitical competition early they were forced into greater concessions to the financiers, merchants, and administrators who financed and staffed the bureaucracy, resulting in patrimonial systems. Where rulers confronted geopolitical pressures later, “they found themselves in a quite different world”, where developments in education and finance made these side payments unnecessary, resulting in greater bureaucratic autonomy.

Philip Manow’s analysis of union formation in Germany and Japan (1997) is also centrally concerned with how the intersection and interactions among different processes affect institutional outcomes. Against conventional analyses that attribute the different institutional forms adopted by the two labor movements to the triumph of social democratic ideology (Germany) or to employer strategies (Japan), Manow reveals how, in both cases, the evolution of state social policy intersected with union formation and affected the latter by systematically favoring certain organizational forms over others. A complementary analysis by Thelen and Kume (1997) reveals

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that the organizational forms ultimately embraced by the two labor movements was also powerfully supported by differences in the system of vocational education and training as these emerged in the early industrial period. In both analyses, attention to the timing and sequencing of union development in relation to and in interaction with other ongoing political processes (the institutionalization of social policy and of vocational training) helps to explain the organizational forms that the labor movement ultimately adopted.

What all of these comparative historical studies share is a perspective that examines political and economic development in historical context and in terms of processes unfolding over time and in relation to each other, in a broader context in which developments in one realm impinge on and shape developments in others. Each in its own way, these works all demonstrate, as Rueschemeyer, Stephens and Stephens put it, that “Causal analysis is inherently sequence analysis” (p. 4). All of them engage in close examination of sequences and processes as they unfold, and perhaps even more importantly, as different processes (at the international and domestic levels, or different processes at the domestic level) unfold in relation to one another. In all cases, they have focused attention on variables that capture important aspects of the interactive features of ongoing political processes, and in ways that explain important differences in regime and institutional outcomes across a range of cases.

However, what most of these works tend not to emphasize or even sufficiently problematize, is how the events they analyze get translated into lasting legacies, in other words the mechanisms for what the Berins Collier and Collier call the “reproduction” of the legacy over time within a particular country. A good deal of this literature —old and new— invokes a similar language, arguing that the events they describe are important because they had the effect of “filling the political space” in ways that were difficult to reverse or alter. Pierson articulates what appears to be the logic behind many of these arguments when he suggests that feedback effects are likely to be “most consequential in issue-areas (or countries...) where interest group activity is not yet well established... because factors that give one set of organizations an initial advantage —even a small one— are likely to become self-reinforcing” (1993, pp. 602-603). However, it is not clear that this would stand up to historical scrutiny. The history of organized labor —to take one important interest group— is actually littered with organization forms that, despite some early-comer advantages, did not manage to survive, let alone dominate the

29 Berins Collier and Collier are themselves a partial exception to this and I go into their treatment of these issues below.

30 The metaphor of “political space” comes up —explicitly or implicitly— time and again in “legacies” arguments. See Lipset and Rokkan (51ff), Skocpol (1992: 52: 310), Thelen and Kume, Herrigel, Manow, Valenzuela (1979), chapter 4, Kerr (1960, p. 235).
available political space.\(^{31}\) Rather, they wither for lack of mechanisms to reproduce themselves.\(^{32}\)

Political space arguments (and related arguments about the “freezing” or “crystallization” of particular institutional configurations) obscure more than they reveal unless they are explicitly linked to complementary arguments telling us about the precise mechanisms of reproduction at work. Without this they are at best incomplete, for the cannot tell us why these patterns persisted and how they continue to dominate the “political space”. Indeed, the language of freezing and crystallization is deeply misleading since it suggests that things “stand still”, when in fact we know intuitively that for organizations like political parties or unions with roots in the 19C to survive into the 20C they have to adapt to myriad changes in the environment. The reproduction of a legacy, in short, is a dynamic process, and this is something that is not well captured in some of the dominant formulations.

With this perhaps in mind, a number of authors invoke Stinchcombe’s arguments about “sunk costs” and “vested interests” which make embarking on alternative paths costly and uncertain. But such references, while a promising starting point for the analysis, cannot themselves stand in for the analysis; put differently, these concepts need to be applied, not just invoked. Among other things, we need to know who, exactly, is invested in particular institutional arrangements, how, exactly that investment is sustained over time, as well perhaps as how those who are not invested in the institutions are kept out. Attending to these issues is likely to generate insights into differences in the mechanisms of reproduction that sustain different kinds of institutional arrangements, or even the same kinds of institutions in different contexts.

Within the critical junctures literature, Berins Collier and Collier stand out in explicitly drawing attention to the issue of the reproduction of critical junctures legacies, as well as the related matter of a legacy’s duration (see especially pp. 31-34). What Berins Collier and Collier explicitly emphasize are differences in the forms that labor incorporation takes across their several countries, and how this affects the different political trajectories of these countries after incorporation. What is implicit in their analysis, but perhaps not as developed as it might be, is that these different patterns of incorporation are also associated with different mechanisms of reproduction.\(^{33}\)

\(^{31}\) For example, materials-based unions in Sweden and skill-based unions in Germany.

\(^{32}\) It does not help, of course, to argue that those groups that got institutionalized first were the ones that “stuck” because that begs the question why some got institutionalized and others not.

\(^{33}\) Given the considerable accomplishments of the volume, it is hard to argue that the authors should have done even more. Still, after raising the tantalizing idea that “it is useful to specify the distinctive features of the mechanism of reproduction” (p. 37) they fall back on Stinchcombe and assert simply that the reproduction of the legacies they track “involve the processes analyzed by Stinchcombe” (p. 37).
To give an example, the pattern of labor incorporation that the authors describe for Mexico is one that is based on the ability of the labor-mobilizing party (PRI) to use the resources of the state to maintain the link to labor at all levels. The legacy of this type of labor incorporation, in other words, is reproduced through patronage, and indeed a form of patronage that reaches deep into society, so that ordinary workers (who may owe their jobs to the governing PRI party) are also materially invested in this system. This pattern of reproduction is quite different from, say, Brazil, where as Berins Collier and Collier show, labor incorporation involved harassment, repression, and coercion. The “legacy” of this form of labor incorporation, as I understand the argument, is precisely that ordinary workers did not become invested in the system (though other groups such as the traditional oligarchy of course, were, and that in fact their investment hinged on continued exclusion of labor). In other words, in Brazil, the legacy of state incorporation is labor’s alienation, and one of the things that sustained this legacy is that in those junctures when it might have been possible to bring labor into the game (e.g., under Goulart), the military intervened and reimposed labor exclusion.

The general point I wish to make is that the different forms of labor incorporation to which Berins Collier and Collier have drawn attention rest on different mechanisms of reproduction, at the extreme, broad-based patronage in Mexico versus repression and control in Brazil). These considerations will likely be important elements in any full understanding of differences in the duration of the various legacies (for as Berins Collier and Collier point out some critical junctures produce very stable regimes whereas others seem to contain the seeds of their own destruction, p. 34), but also —maybe more importantly— to understanding exactly what kinds of events or processes have the capacity to undermine the legacy in different countries.34

Summing up, the critical junctures literature has taught us a great deal about the politics of institutional formation and the importance of the timing, sequencing and interaction of ongoing political processes in accounting for cross-national variation. Where most of these analyses have been somewhat less explicit, however, is in answering the question of what sustains the institutional arrangements that emerge from these critical junctures. The issue of continuity over time and feedback mechanisms that sustain institutions, dynamically, are at the center of another, related body of work, to which I now turn.

34 In the Mexican case, for example, the reproduction of the legacy should be especially vulnerable to developments that make it hard for the PRI to continue to deliver patronage benefits. And in fact we do find that ongoing economic crisis over the past years has shaken the hegemonic position of the PRI, interfering as it does with the party’s use of state resources to shore up political support (Berins Collier and Collier 1991, p. 759).
Feedback effects

The literature on policy feedback in historical institutionalism has been elegantly and thoroughly summarized by others and so I will forgo a comprehensive survey in favor of a more thematic discussion. This literature follows on Krasner's observation that "path dependent patterns are characterized by self-reinforcing positive feedback" (1988, p. 83). As Ikenberry points out, the literature in this area points to two broad types of feedback mechanisms (though, as the examples below indicate, many analyses combine elements of both). One set of mechanisms, which Ikenberry refers to as "functional", seems perhaps better described as incentive structure or coordination effects (see also North, 1990). What this means is that once a set of institutions is in place, actors adapt their strategies in ways that reflect but also reinforce the "logic" of the system. Zysman captures the essence of these arguments when he states that "the institutional structure...induces particular kinds of... behavior by constraining and by laying out a logic to the market and policy-making process" (1993, p. 243). A few examples from recent work can illustrate.

First, Wolfgang Streeck's work on the political economy of Germany has shown how the existence of particular institutional arrangements (a national system for vocational education and training and centralized collective bargaining, for example) has affected firm strategies in ways that reflect but also actively reinforce these institutions. These arrangements, as he puts it, "force and facilitate" the pursuit of strategies based on high-skill, high-value added production. As business adapts its strategies to the institutional incentives and constraints they face, this encourages further movement along this trajectory, as firms come to depend on the existence of these institutions for their continued success in international markets (Katzenstein ed., 1989). Hall and Soskice (forthcoming) take this argument a step further by suggesting that the presence of certain institutions (e.g., strong works councils) can raise the returns to the presence of other, complementary institutions (e.g., strong bank-industry links). They use this argument to explain why certain kinds of labor market arrangements tend to be associated in many advanced industrial economies with certain kinds of financial arrangements (especially "patient capital").

35 See especially the review articles by Pierson (1993) and Ikenberry (1994). Among other things, Pierson works out a typology of different types of policy feedback mechanisms, drawing distinctions among those that operate at the elite versus mass level, and those that affect material incentives versus those that operate through what he calls "interpretive effects".

36 As Krasner (1988, p. 84) and Pierson (1997) point out, many of these arguments could be put in the language of "increasing returns", here understood simply as a situation in which once a particular path is chosen, actors adapt to the existing institutions in ways that push them further along that trajectory (and in so doing, also render the "path not chosen" increasingly remote) (also Levi 1997).

37 Esping-Anderson's "conservative-corporatist" welfare state provides another example of this. Since this type of welfare state is premised on the notion of a "single breadwinner", family
Ben Schneider's analysis (forthcoming) of the developmental state in Latin America provides another example. In this case, Schneider shows how the structures and policies of developmental states have the effect of fragmenting business interests. Where states distribute economic benefits on a discretionary basis, firms orient their strategies toward direct, individual appeals to the government. State activity of this sort generates very weak incentives for firms to engage in collective action, resulting in anemic business associations. Jonah Levy (1998) comes to similar conclusions for France. He shows how the traditionally dominant role of the state in the French political economy actively discouraged the emergence of strong intermediate (political-economic) associations. In what he calls "Tocqueville's revenge", recent attempts by the state to withdraw from its traditionally pivotal role in economic life have failed for lack of viable associations that are capable of stepping into the regulatory void created by the retreat of the state. In other words, historically speaking, the more the French state compensated for France's weak associations, the less able they were to do anything else.

Stephen Vogel's analysis of the politics of deregulation in the political economies of the advanced industrial countries also tracks the path-dependent evolution of institutions. As he puts it, his study looks at "how political-economic institutions shape policy choices and also about how these choices in turn reshape the institutions" (1997, p. 9), and he identifies feedback mechanisms at both the ideational and the structural levels. Vogel shows how the putatively common trend toward deregulation has in fact played out rather differently across the five countries he examines and in fact, contrary to the rhetoric, regulatory reform has very often combined liberalization with various forms of re-regulation. Against contemporary theories predicting cross-national convergence in the face of globalization, Vogel finds that what individual governments have actually done reinforces distinctive national trajectories based on different underlying ideas about the appropriate role of the state in the market and structural features of the political-economic context. The picture that emerges from his analysis is one of evolution and change, but moving along (nationally specific) well-worn paths, as the search for solutions to new international pressures is structured by prevailing domestic institutions.

The second feedback mechanism identified by Ikenberry has to do with the distributional effects of institutions, and here the idea is that institutions are not neutral (coordinating mechanisms) but in fact reflect and also reproduce and magnify particular patterns of power distribution in politics (see especially Pierson 1998). Among other things, this body of work emphasizes how political arrangements and policy feedbacks actively facilitate the organization and structures adapt to the incentives and disincentives it embodies, and this is one reason why female labor market participation in such economies is low by international standards.

Zysman, too, has noted the tendency toward the "accommodation of French economic and social life to its central administrative and political structures" (1993, p. 275).
empowerment of certain groups while at the same time actively disarticulating and marginalizing others. The distributional biases in particular institutions or policies “feed back” in ways so that, as Weir puts it, “over time, some avenues of policy become increasingly blocked, if not entirely cut off” as “decisions at one point in time can restrict future possibilities by sending policy off onto particular tracks” (1992, pp. 18, 19).

Some of the best work in this area has been done by Theda Skocpol and her collaborators, and Skocpol’s celebrated book, Protecting Soldiers and Mothers is an important recent contribution which reiterates but also elaborates some of the main themes of Skocpol’s earlier work. Skocpol explicitly problematizes the issue of interest formation, arguing that institutions “shape the capacities of groups to achieve self-consciousness, to organize, to make alliances”. For example, drawing on work by Shefter, Skocpol shows how the fragmentation of the state as well as the organization of party competition along patronage lines actively mediated against the development of a unified working class that could then spearhead the movement for comprehensive social policies in the United States. At the same time, the policies the government did devise powerfully shaped future possibilities for more comprehensive schemes. Specifically, the policy of granting social benefits to Civil War veterans contributed to the emergence of a self conscious interest group (veterans and their widows), also endowing it with material and ideational resources that threw up barriers to other groups who might appeal to the state for protection on other grounds.

To take another example, Esping-Anderson draws attention to the “decommodifying” effects of the universal welfare states, and in doing so shows how these arrangements actively shore up the power of the political and economic organizations of the working class. The resulting pattern of politics contrasts sharply with those in the United States, where as Skocpol and also Katznelson emphasize, political institutions and government policy have if anything operated to disarticulate working class organizations and to disempower working class interests. Rather than taking the interests of political actors as given, all these works step back to ask the in some ways more important question of how groups got constituted in the particular ways they did in the first place, then to consider how this affects the ways in which they pursue but also understand their interests. As Hall (1993, p. 51) puts it: “The social construction of identities in other words is necessarily prior to more obvious conception of interest: a ‘we’ needs to be established before its interests can be

39 This is a major theme in Rueschemeyer, Stephens and Stephens 1992, Weir 1992, and Esping-Anderson 1990, as well.

40 These insights echo a major theme in Capitalist Development and Democracy, in which Rueschemeyer, Huber Stephens and Stephens demonstrate though comparative analysis that the capacity of classes to act collectively varies considerably across countries and depends heavily on the “social construction of class interests” though unlike Skocpol, they emphasize how this is shaped by the structure of the economy and international factors rather than state structures (pp. 53-54).
articulated". Margaret Weir has brought these insights to bear on the issue of coalition formation: "By channeling the way groups interact in politics and policy making, ... institutions greatly affect the possibilities for diverse groups to recognize common interests and construct political alliances" (1992, p. 24). The result is that, in some institutional settings, groups with the same "objective material interests" cannot find common cause.

Finally, Terry Karl's study of what she calls "petro-states" paints an especially vivid portrait of path dependency that emphasizes the power-distributional biases of institutions. In countries as diverse in regime type, social structure and culture as Venezuela, Iran, Nigeria, Algeria, and Indonesia, Karl finds that the adaptation of political-economic institutions to the oil economy produces pathologies that actively reinforce the dependence of these economies on oil, despite explicit efforts on the part of many governments to use oil revenues to fuel more balanced economic development. Her view of path dependency stresses political-distributional feedback effects, arguing that the incentives embedded in political-economic institutions are "above all else...the reflection and product of power relations, either actual or anticipated, at a given point in time...they tend to persist even when power relations and their accompanying ideologies have begun to change, and they cannot be changed at will — even when there is widespread understanding that they are sub-optimal or outright should be altered" (1997, p. xvi). In her cases, both societal and state institutions are irresistibly drawn to organize themselves in ways that perpetuate rather than transcend dependence on oil (e.g., the domestic bourgeoisie shifts its activities to those linked to oil — where the money is — and the state becomes the center of rent-seeking behavior so that state jurisdiction expands massively with the expansion of oil and oil-related activities even as state autonomy and authority atrophy). For one case (Venezuela) Karl examines specific choice points in detail and shows how the various decisions "demonstrate that there was never an equal probability that other choices would be made in their place; that each decision was related and grew from the previous one; and that, except during uncertain moments of regime change, the range of choice narrowed from one decision to another as Venezuela moved further into its oil-led trajectory" (1997, p. 226). In the end, the perverse and paradoxical effect is that oil revenues — far from leading the way to development — systematically perpetuates a dependence on oil and a failure to develop.

These works have taught us a great deal about the kinds of dynamic processes that help us explain how stable patterns of politics persist and indeed reproduce themselves over time (Ikenberry's "developmental pathways"). However, the strong emphasis in most of these works on the mechanisms through which previous patterns are reproduced downplays the factors that might tell us how they can be changed. The language of "lock in" often obscures the fact that, because institutions are embedded in a context that is constantly changing, stability — far
from being automatic—may have to be actively sustained, politically. Where the context is changing, those who are invested in particular institutions will reevaluate their investment in light of these changes and as Hall and Soskice (forthcoming) and Streeck (forthcoming) note, changes in one institutional arena can reverberate, provoking changes in other, complementary institutions. These considerations lead us, finally, to a discussion of institutional evolution and political change.

**Institutional Evolution and Political Change**

If positive feedback effects were the whole story, then prediction would be easy, since we could simply read the outcomes off the institutional configuration. But what we know about political life is that institutions also evolve in important ways, and this is where Orren and Skowronek’s arguments about temporality and the unfolding of different processes over time become important. Above I argued that two alternative views of path dependency are overly deterministic, a weakness that goes back at least in part to the fact that both of them stipulate at a very high level of abstraction particular reproduction mechanisms that blend out questions of conflict and make it difficult to talk about change. Orren and Skowronek, by contrast, present a more dynamic alternative, and one—very importantly—that focuses on the incongruities and intersections between different processes and institutional logics as they unfold over time. Illustrations of what this looks like in practice can be found in recent work by Margaret Weir and Paul Pierson, among others. In her study of US welfare policy, Weir shows how the unexpected “collision” of two (previously) unconnected policy streams in the 1960s—the “War on Poverty” and the civil rights movement—had a profound impact on the evolution of employment policy in the United States, turning it in a direction that policy makers did not originally intend. Similarly, in his study of the evolution of the European Union, Pierson shows how “gaps” (between different levels of action—domestic and European-level) and “lags” (produced by disjunctures between short and long term events and considerations) created openings which allowed non-state actors (in his cases, women’s groups and EU bureaucrats) to influence institutional development in ways that the EU member states did not anticipate and could not control.

A problem with Orren’s and Skowronek’s characterization, however, is that it is in some ways too fluid. The intersection and interaction of different processes unfolding in time (to which Orren and Skowronek draw attention) is certainly an important feature of political life; likewise the kinds of “collisions,” “gaps,” and “lags” to which Weir and Pierson have directed our attention are also clearly

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41 Barry Weingast’s analysis of antebellum America is a good case in point, for his characterization suggests that the balance rule was not at all automatic, but had to be actively nurtured in light of changing external conditions.
pervasive in politics. But what we need to know is which interactions, and which collisions are likely to be politically consequential, which of these in other words has the potential to disrupt the feedback mechanisms that reproduce stable patterns over time and produce political openings for institutional evolution and change. In what remains, I would like to suggest that the kinds of openings that particular institutional configurations offer depends on the particular mechanisms of reproduction that sustain them.

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 kinds of processes that are likely to disrupt them will also be different, though predictable. Take the case of the welfare state. Esping-Anderson’s 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); they also rely on different mechanisms of reproduction and will therefore be differently affected by specific other “external” trends. To take one aspect, changes in gender relations and family structures are likely to reinforce elements of the universalistic and liberal welfare states (which both —though in different ways— support a high level of labor force participation by women), but 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.32

Universal welfare states, on the other hand, may be especially susceptible to other kinds of pressures. Bo Rothstein’s analysis of the universal welfare state, for example, suggests that middle class support is crucial (because this is the pivotal group electorally speaking) but that —unlike the working class— the middle classes are neither clear material beneficiaries nor clear losers in the universal welfare state. Rothstein argues that the support of the middle class is premised on their “contingent consent” which hinges on their belief that the system is fair in the sense that they are not shouldering an undue burden, but rather that all citizens are contributing their fair shares to the system. This type of analysis brings us to the issues flagged above, concerning who has “vested interests” in particular institutions and what sustains these investments over time. Rothstein’s study reveals that the foundations of working class support for the welfare state (which includes a strong material component) and that of the middle class (which rests on contingent consent) are fundamentally different; both are invested, but in rather different ways. The coalition behind the universal welfare state, far from being self-reinforcing as in increasing

32 See also Paul Pierson’s comparative analysis of welfare state retrenchment in Britain and the United States. But in contrast to this, the point that I wish to emphasize here is that it is not just a question of whether policies are more or less locked in, but rather the different ways in which the policies are reproduced, which makes them vulnerable to different kinds of pressures.
returns arguments, may in fact have to be politically re-invented from time to time, as changes in the environment shift and as these various groups re-evaluate their investment in light of these changes. As Rothstein points out, one development that could complicate the reproduction of the system is the emergence of individualized private sector social services, for these open up possibilities for those who have the resources to do so to opt out of the standardized universalistic programs. The growth of such alternatives could then upset the contingent consent of the middle classes, who would resent paying (privately) for their own more individualized services while also shouldering the burden of the standardized system on which they no longer themselves draw. In short, a rise in individualism associated with demands for more “choice” and less “standardization” is potentially subversive to the universal welfare state, given its particular material and moral foundations.

Understanding the different mechanisms for reproduction that sustain different institutions is also the key to understanding why it is that common international trends frequently have such different domestic consequences, disrupting previously stable patterns in some countries while washing over others seemingly without effect. As many authors have noted, the political-economic institutions of the industrial countries have proven surprisingly resilient in the face of “globalization” pressures, and this certainly speaks for the strong feedback mechanisms at work. At the same time, however, certain countries have in fact seen important changes. Prominent examples are wage bargaining institutions in Denmark and Sweden, both of which have experienced substantial reconfiguration in the last 15 years. How do we make sense of the special (in some ways, unique) vulnerability of the Swedish and Danish systems of wage bargaining to these putatively common international trends? The crucial starting point for any such analysis has to look first at what ideological and material foundations sustained these institutions in the first place (Swenson WZB). Both the Danish and the Swedish models of the 1960s and 1970s were characterized by a high degree of egalitarianism which rested on (and also reproduced) a particular coalition of interests and a set of ideational claims (what Swenson calls a “moral economy”) supporting egalitarianism. These arrangements were highly resilient in the face of a number of important changes—both domestic and international—through the 1970s, but they began to unravel in the face of market changes in the 1980s that systematically enhanced the bargaining power of skilled over unskilled workers (Pontusson and Swenson). Specifically, the trend toward “diversified quality production” was deeply subversive to systems like the Danish and the Swedish ones, which were premised on a high degree of egalitarianism because it encouraged the development of shop-floor structures that were completely at odds with the overarching national wage bargaining institutions. By contrast, the trend toward “diversified quality production” did not subvert, and if anything, reinforced key institutions in the German political economy, since these were premised on a very different foundation.
and different mechanisms of reproduction, which among other things accommodated (indeed nurtured) certain kinds of inequalities between skilled and unskilled workers.\footnote{See also Locke and Thelen 1995. In a similar vein, Barry Weingast (1998) has shown how political stability in antebellum America was able to survive a number of crises over the course of many years. It proved vulnerable, however, specifically to one development —unbalanced growth of North and South—for this upset the capacity of political parties to credibly promise (and then deliver) the continued operation of the balance rule, on which the reproduction of that particular pattern of politics had rested.}

Take as a final example the stability of party systems. As argued above, it may be true—as Lipset and Rokkan note—that party systems get “frozen” at particular junctures, but at the same time we know from Shefter’s work (1977) that the mechanisms through which the stability of different party systems is reproduced vary. Shefter distinguishes between patronage-based and programmatic parties, a distinction that clearly makes a difference to the kinds of events that are likely to disrupt stable patterns of politics. So, for instance, throughout the post-war period, both Italy and Sweden had very stable party systems that revolved around the dominance of a single hegemonic party, the Christian Democrats in Italy and the Social Democrats in Sweden. In Sweden, the system could “absorb” a defeat of the Social Democrats at the polls (as in 1976, for example) without this provoking a full-scale breakdown of the party system. In Italy, however, the defeat of the Christian Democrats (not coincidentally, precipitated by corruption scandals) prevented the party from continuing to use state resources to shore up its political support, and so the crisis of the Italian Christian Democrats created a massive “opening” which quickly brought about a complete reconfiguration of the political landscape.

In all of these cases, the key to understanding moments in which fundamental political change is possible requires an analysis of the particular mechanisms through which the previous patterns were sustained and reproduced. Against equilibrium and other models which separate the question of stability from the question of change and propose that they require different analytic tools, the previous examples suggest just the opposite, namely that an understanding of political change is inseparable from and indeed rests on an analysis of the foundations of political stability (Skowronek 1995, p. 96; Orren and Skowronek 1994, pp. 329-330).
Conclusion

Peter Gourevitch is responsible for the memorable aphorism that for comparativists, happiness is a crisis that hits a lot of countries, for it is in moments of crisis that the elements that previously held a system together come into full relief. This is essentially what I have been arguing here. My point has been that it is possible to do better than to separate questions of institutional reproduction from those of institutional change, and to resign ourselves to the idea that each requires an entirely different toolkit from the other. Instead, drawing together insights from the critical junctures literature and the literature on path dependency and policy feedbacks, I have argued that the key to understanding institutional evolution and change lies precisely in specifying more precisely than heretofore the reproduction and feedback mechanisms on which particular institutions rest. I take from Orren and Skowronek the important insight that much of what moves politics is the intersection and interaction of different ongoing processes, though I qualify this somewhat by arguing that only some of the resulting “collisions” are likely to be politically consequential, specifically those that interfere with the reproduction mechanisms at work in particular cases.

Attention to these matters will provide insights into some of the provocative issues raised but not necessarily fully answered by recent historical-institutional work. These include the important issue of differences in the duration of critical junctures legacies (Berins Collier and Collier) as well as the related question of why some institutional legacies seem to contain the seeds of their own destruction (as in Skocpol’s Civil War benefits, or Weir’s coalition for employment policy in the United States). Understanding these issues will require work that is, as Skocpol puts it, “genuinely historical” in the sense that it looks at processes and tracks their unfolding, also in relation to one another, over time. The link between the critical junctures literature (on institutional formation) and the feedback literature (on institutional reproduction) is thus clear: knowing how institutions were constructed provides insights into how they might come apart as well.

Functionalist perspectives will not take us far, since they skirt the issue of the origins of institutions and the all important matter of the material and ideological coalitions on which are founded. This does not mean that borrowing from other perspectives is impossible; on the contrary it may be quite fruitful. One can imagine conceiving and analyzing consequential policy “collisions” as “nested games”, for example, employing some of the tools of rational choice to help sort out the logic of the situation and the responses of the actors. This could certainly form one of the “modules” as Scharpf puts it, in a more comprehensive analysis. It will not, however, substitute for the kind of process-oriented analysis that is characteristic of
historical institutionalism, which among other things is the only way to understand how it is that some games came to be nested within others in the first place.

Many of the insights from the recent "feedback" literature will certainly play a role as well, but concepts such as "sunk costs" and "vested interests" will have to be applied with more precision and care than heretofore, paying particular attention to who, exactly, is invested in particular institutions, and what sustains the link to the existing institutions. The traditional focus in institutional research on continuity and stability is maintained, but it needs to be combined with more sustained attention to what precise mechanisms sustain that stability, for it is there that we will find clues as well into the particular kinds of external processes that contain the potential for producing political opening and change. Attention to the different mechanisms of reproduction will also lend insight into the different ways that countries are affected by putatively common international forces and trends. In short, a more precise specification of the reproduction mechanisms that lie behind particular institutions holds the key to understanding important elements of stability and change in political life.
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