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The historical institutionalist tradition in comparative politics commonly assigns analytical primacy to political institutions. Whereas this polity-centeredness may be quite justifiable for purposes of comparative public policy, students of comparative political economy should pay systematic attention not only to economic institutions but also to a range of economic-structural variables that lie beyond the conventional confines of institutional analysis. Providing the basis for an analysis of collective actors and their interests, such an approach is needed to account for institutional change and policy realignments within stable institutions.

FROM COMPARATIVE PUBLIC POLICY TO POLITICAL ECONOMY Putting Political Institutions in Their Place and Taking Interests Seriously

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Sven Steinmo, Kathleen Thelen, and Frank Longstreth (Eds.), *Structuring Politics: Historical Institutionalism in Comparative Analysis*. New York: Cambridge University Press, 1992. 257 pp.

R. Kent Weaver and Bert A. Rockman (Eds.), *Do Institutions Matter? Government Capabilities in the United States and Abroad*. Washington, DC: Brookings Institution, 1993. 498 pp.

The two edited volumes under review here explore the significance of institutions for the policies and politics of advanced capitalist states. The

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editors and contributors to *Structuring Politics* self-consciously situate themselves within the historical institutionalist tradition in comparative politics and seek to advance this tradition theoretically by engaging in a dialogue with rational choice institutionalism and by taking on new explanatory problems such as how and why institutions change. By contrast, *Do Institutions Matter?* seems oblivious to paradigmatic debates and focuses on a specific empirical question: To what extent and how do political institutions affect government performance?

Each volume is successful on its own terms and, taken together, they might be said to represent the state of the art in historical institutionalist theory and research. Reviewing these volumes provides an opportunity to explore the strengths and weaknesses of the historical institutionalist paradigm. To this end, my essay ranges widely and refers to other works as well. Although I emphasize the limits of historical institutionalist paradigm, I hope that my discussion also serves to clarify the premises and nature of its core arguments and shed some light on its evolution over time. My critique is meant to be constructive, and the alternative perspective that I advance builds on historical institutionalist literature.

It is commonplace these days for institutionalists as well as their critics to point out that the claim that "institutions matter" does not take us very far. In their introduction to *Do Institutions Matter?*, Weaver and Rockman (1993a, p. 5) point out quickly that the real questions are, *Which* institutions matter, and *how* do they matter? The historical institutionalist tradition encompasses at least three broad answers to Weaver and Rockman's first question (Which institutions matter?). One group of historical institutionalists holds that *state institutions* are of particular importance; another couches its arguments more broadly in terms of *political institutions*; and a third group refuses to assign analytical primacy to any particular set of institutions while insisting on the importance of what Thelen and Steinmo (1992) refer to as "intermediate-level institutions" as distinct from "macro-structures" (p. 11).

The third of these formulations brings out the conceptual issues that lurk behind the apparently empirical problem of determining which institutions matter. What exactly do we mean by *institutions*? And what variables *other than institutions* are there that might matter? The lack of conceptual elaboration in the historical institutionalist literature is striking, and the two volumes under review here do not advance the literature significantly in this respect.

As for Weaver and Rockman's second question (How do institutions matter?), we can distinguish at least four kinds of causal claims advanced by historical institutionalists independent of how they answer the first question. Perhaps the most common claim is that institutions determine *the capacity*

of governments to legislate and implement policies. A second claim is that institutions determine *the strategies of political or economic actors* by virtue of the opportunities and constraints that they provide. Third, historical institutionalists commonly assert that institutions determine *the distribution of power among political or economic actors*. And, finally, we are often told that institutions matter in an even more profound sense: They determine *who the actors are and/or how the actors conceive their interests*. Any and all of the last three claims imply that institutions matter not only to the ability of governments to carry out policies but also to the policy goals that governments pursue and to politics in the broadest sense.

I do not mean to attribute to historical institutionalists the claim that political institutions *alone* determine state capacities, actor strategies, and so forth. The disclaimers are legion; for instance, Thelen and Steinmo (1992) introduce *Structuring Politics* by asserting that “institutions constrain and refract politics but they are never the sole ‘cause’ of outcomes” (p. 3). Again, the question arises, What other variables matter? And how are these other variables, be they institutional or noninstitutional, political or nonpolitical, related to the variables that historical institutionalists emphasize? According to Thelen and Steinmo (1992), “an institutional approach does not replace attention to other variables—the players, their interests and strategies, and the distribution of power among them;” rather, “it puts these factors in context, showing how they relate to one another by drawing attention to the way political situations are structured” (pp. 12-13). Elsewhere, Steinmo (1993) suggests an alternative view when he observes that “domestic political institutions operate within—and must be understood in the context of—the broader social, economic, and political setting in which they are embedded” (p. 12). Should institutions be viewed as the context in which other variables come into play or as variables within a broader context?

Although greater conceptual clarity is needed, such questions cannot be answered in the abstract. The appropriateness of particular analytical categories and assumptions depends on the empirical questions that are being asked or, in other words, the “outcomes” to be explained. In what follows, I argue that most of the essays in *Structuring Politics* and *Do Institutions Matter?* privilege the role of political institutions and fail to engage in a sustained analysis of contemporary capitalism. This polity-centered approach might be justifiable for purposes of comparative public policy but becomes too restrictive if one wants to pursue issues in comparative political economy.

Many historical institutionalists, including contributors to *Structuring Politics* and *Do Institutions Matter?*, would no doubt agree that students of

comparative political economy need to devote more attention to economic institutions than do the essays in these two volumes. However, the study of comparative capitalism poses a more fundamental challenge to the historical institutionalist tradition for it requires us to attend to a range of variables, such as factor endowments and the concentration of capital, that cannot be characterized as institutional variables without rendering the concept of institutions vacuous. As a shorthand, I refer to these variables as *structural variables*, but I am wary of this label insofar as it implies common properties that distinguish these variables (as a group) from institutional variables.

The point of the alternative perspective that I stake out is not to deny that institutions matter but to argue (following Steinmo) that underlying structures shape the configuration and operation of political and economic institutions. This approach provides the basis for an understanding of the systemic power of capital and also enables us analyze the interests of collective actors and how these interests change over time. Both structural power relations and variable interests are of crucial importance to the politics of institutional change, a topic that Thelen and Steinmo bring to the fore in their introduction to *Structuring Politics*.

The distinction between comparative public policy and comparative political economy plays an important role in the following discussion. As Evans and Stephens (1988) put it, the comparative political economy project rests on "the conviction that economic and political development cannot fruitfully be examined in isolation from each other" (p. 740). In my view, much of what passes for comparative political economy in political science remains an exercise in comparative public policy in the sense that the dependent variable is government policy (some kind of economic policy) and variations on the dependent variable are explained by governmental institutions or processes (e.g., corporatist arrangements). Commonly, it is assumed that government policy in turn affects economic outcomes (e.g., patterns of industrial adjustment), but the economic consequences of government policy are not an object of analysis. On the other hand, this approach to the politics of economic policy assumes that economic conditions do not explain cross-national policy variations. As a matter of (my) definition, comparative political economy requires a break with at least one of these assumptions; that is, political economists must (should) either seek to explain economic outcomes or incorporate economic variables into their explanation of policy outcomes.

The reader will note that *Structuring Politics* receives more critical scrutiny than does *Do Institutions Matter?* in what follows. Because it takes the institutionalist approach beyond the confines of comparative public policy, *Structuring Politics* is a more interesting book to me but also a book that is more vulnerable to my critique of historical institutionalism.

FROM STATE-CENTERED TO POLITY-CENTERED INSTITUTIONALISM

As Thelen and Steinmo (1992, p. 30) suggest, Katzenstein's edited volume *Between Power and Plenty* represents the beginning of a serious interest in political economy among advanced-country comparativists and an important moment in the emergence (or revival) of historical institutionalist thinking. To my mind, the appeal of *Between Power and Plenty* to political scientists coming of age during the 1970s derived, to a large extent, from the idea that contending Marxists and pluralist perspectives on liberal democratic politics shared a common understanding of the state as an epiphenomenal manifestation of societal forces and that the terms of the fruitless debate between them should be rejected in favor of a conception of the state as an autonomous structure and/or actor with its own distinct history and, perhaps, its own distinct interests. Most forcefully advanced by Stephen Krasner (1978a, Chap. 1; 1978b, p. 57), this idea of a "third view"—a dual rejection of Marxism and pluralism—clearly informed much of *Between Power and Plenty* and subsequent state-centric works, notably the contributions to *Bringing the State Back In* (see especially Skocpol, 1985, pp. 4-5).

The perception that the postwar expansion of state activities had been accompanied by a decline in the significance of electoral politics, parties, and parliaments also seems to have informed the rediscovery of the state during the 1970s. Significantly, the contributors to *Between Power and Plenty* did not explore the forces behind these general trends; rather, they focused their attention on variations in policy orientation and effectiveness among advanced capitalist states. The key variables invoked by these authors to explain cross-national variations might roughly be summarized as follows: the degree to which state officials form a cohesive group capable of articulating their own policy prescriptions, the ability of state officials to resist societal pressures, the ability of state officials to elicit the cooperation of private actors, and the administrative capacities of the state.

Emphasizing the politics of implementation over the politics of legislation, the state-centric approach pioneered by contributors to *Between Power and Plenty* drew on insights by students of public policy and public administration. Earlier works in these fields clearly treated government officials as political actors with a significant degree of autonomy vis-à-vis societal interests, and so the question inevitably arose whether the state-centric approach really represented the analytical breakthrough claimed by its proponents (cf. Almond, 1988). Skocpol's (1985) response to this question is instructive: The problem with most studies of "bureaucratic politics," she

argues, is that they treat government agencies individually and neglect the “structure and activities of the . . . state as a whole” (p. 4). In a similar vein, Krasner (1984) emphasizes the conceptual importance of the overall structure of state, defined as “a bureaucratic apparatus and institutionalized legal order in its totality” (p. 224).

Against this background, the absence of the state in *Structuring Politics* and *Do Institutions Matter?* is striking. It is most striking in the latter case, for *Do Institutions Matter?* sets out to investigate questions that lie at the core of the statist agenda of 10 years ago. In their introductory chapter, Weaver and Rockman (1993a, pp. 5-6) declare that the purpose of the volume is not to explain the goals that governments pursue but rather to assess their effectiveness in pursuit of their goals and to explain cross-national variations in government effectiveness. Although this sounds like an attempt to work out the meaning of *state capacity*, the concept of the state never appears in *Do Institutions Matter?*. Instead, Weaver and Rockman (1993a, 1993b) conceive the political-institutional arrangements of advanced capitalist states in terms of a number of discrete institutional variables: executive-legislative relations (presidentialist vs. parliamentary systems), electoral rules (winner-take-all system vs. proportional representation), party systems and patterns of government formation (alternating party government, single-dominant party government, coalition government, etc.), center-periphery relations (federal vs. unitary systems), the role of the judiciary, and the character and role of the bureaucracy.

That Weaver and Rockman and their contributors do not use the word *state* does not necessarily mean that they abandon the state as an object of analysis, but the choice of words does in this case, I believe, signal a shift in analytical attention. Drawing on Weber’s conception of the state as a mechanism of coercion/extraction and bureaucratic administration, the statist literature emphasizes the exercise of state power vis-à-vis society. The authors of *Do Institutions Matter?* broaden the purview of institutional analysis to include the representation of societal interests and the institutions associated with representative democracy—parties, electoral rules, and legislatures. On these grounds, it seems appropriate to speak of an abandonment of “state-centered” institutionalism in favor of a broader “polity-centered” institutionalism.

This characterization also captures the thrust of *Structuring Politics*. Taken as a whole, the volume certainly represents an abandonment of statism; most of the authors rarely speak of the state, and none of them engage in any sustained discussion of the boundaries or cohesion of the state. It is perhaps less clear that the alternative to statism proffered by *Structuring Politics* can be characterized as polity-centered institutionalism. In their introductory chapter, Thelen and Steinmo (1992) declare that

institutionalists are interested in “the whole range of state and societal institutions that shape how political actors define their interests and that structure their relations of power to other groups” (p. 2). However, the essays in *Structuring Politics* consistently focus on political institutions and emphasize their causal significance.

The emphasis on political institutions is very explicit in the essays by Rothstein and Immergut; Rothstein (1992b) sets out to show that differences in unionization across advanced capitalist countries “can to a large extent be explained by historical variation in national *political institutions*” (p. 34; emphasis in original), and Immergut (1992) argues that cross-national variations in health policy are best explained by “analyzing the political institutions in each country” (p. 58). Although Hall’s essay on the movement from Keynesianism to monetarism in Britain does discuss changing economic institutions, Hall (1992) concludes by emphasizing that the process of policy change was structured by “the institutional framework that characterizes the British polity and policy process” (p. 106). In a similar vein, King (1992) sums up his essay on work-welfare reform in Britain and the United States by stating that, in both cases, “a version of New Right ideas triumphed in the policy outcome, but this success occurred in different ways reflecting each polity’s institutions” (p. 241). Whereas Hattam’s (1992) explanation of the strategic divergence of the American and British labor movements during the late 19th century emphasizes the autonomy of the courts in the American case and their subordination to Parliament in the British case, there can be little doubt that the institutions that matter to the evolution of American employment policy in Weir’s (1992) essay are political institutions in the conventional sense. By my reckoning, only one of the seven substantive essays in *Structuring Politics*—Dunlavy’s (1992) piece on American and Prussian railroad policy during the 19th century—engages in an analysis that does not fit a polity-centered mold.

What might account for the apparent abandonment of state-centered institutionalism represented by these two volumes? In the case of *Structuring Politics*, the shift of analytical attention appears to be closely related to the editors’ and contributors’ concern with “institutional dynamism.” As Thelen and Steinmo (1992) argue, the historical institutionalist tradition has yielded powerful accounts of “cross-national differences and the persistence of patterns or policies over time within individual countries” (p. 14) but has yet to produce equally powerful accounts of why institutions differ across countries, how and why institutions change over time, and how and why policy outcomes sometimes change within relatively stable institutions.

In part, the agenda that Thelen and Steinmo set out in their introductory chapter can be seen as a direct extension of previous work in the historical

institutionalist tradition. Having established that some set of institutions matters, the question of what accounts for the existence of this set of institutions naturally arises. But Thelen and Steinmo's agenda must also be seen as a response to developments in the "real world." After all, we are living through a period in which "postwar settlements" have been challenged and have come undone in many countries. The parameters of economic policy debate have changed quite fundamentally, and this change has been accompanied by subtle and not-so-subtle institutional changes (privatization of state enterprise, decentralization of wage bargaining, increased autonomy of central banks, the growth of European Community regulation, etc.).

As Hall (1993) argues elsewhere, a state-centric approach might provide a useful way to understand incremental policy innovation within stable institutions but does not capture the process of paradigm shifts such as the movement from Keynesianism to monetarism in Britain—"a societywide affair, mediated by the press, deeply imbricated with electoral competition, and fought in the public arena" (p. 287). Like Hall, other contributors to *Structuring Politics* emphasize the role of ideas and political leadership in their discussions of institutional change and policy innovation. By conceiving the institutional matrix of advanced capitalist states more broadly than have earlier state-centric works, these authors are able to incorporate variables that explain change into their analyses.

If this is an accurate interpretation of what lies behind the abandonment of state-centered institutionalism, *Structuring Politics* might be criticized for failing to go further in broadening the purview of institutionalist analysis. Surely, we cannot begin to understand the changes in policy paradigms and political-economic institutions alluded to here without some analysis of capitalism and its dynamics, yet only one or two of the essays in *Structuring Politics* point us in this direction.¹

THE INSTITUTIONALIST APPROACH TO COMPARATIVE POLITICAL ECONOMY

In their emphasis on political institutions, *Structuring Politics* and *Do Institution Matter?* are not entirely representative of the historical institutionalist tradition. Most obviously, perhaps, a number of recent works on indus-

1. I expect that Thelen and Steinmo would agree with the prescriptive part of this statement. In their own work, both invoke changes in the world economy to explain political change—in Thelen's (1991, 1993) case, change in union and employer strategies; in Steinmo's (1993) case, change in tax policy.

trial relations and the politics of industrial adjustment, notably Thelen's (1991) and Turner's (1992) excellent books and various contributions to Golden and Pontusson (1992), also fall within the historical institutionalist tradition. Because my objective is to take stock of the historical institutionalist tradition as a whole, this literature deserves some consideration here.

How we conceive the literature on comparative labor politics depends, in part, on how we conceive the distinction between politics and economics or, more precisely perhaps, the distinction between polity and economy. Are trade unions and, for that matter, employer associations political or economic institutions? The same question might be asked with regard to codetermination institutions, which figure prominently as causal variables in Thelen's and Turner's analyses. Thelen and Turner would no doubt argue that the question posits a false dichotomy—that unions and other industrial relations institutions ought to be conceived as *both* political and economic—and I do not wish to dispute this argument. I do wonder, however, whether Thelen's and Turner's work, and the work of other political scientists interested in political economy (including some of my own work), does not reproduce a certain polity-centeredness by equating economic institutions with industrial relations institutions or by equating economic actors with interest groups, such as unions and employer associations.

The question of what exactly is meant by *institutions* is perhaps more important than the question of distinguishing between political and economic institutions in this context. In their introduction to *Structuring Politics*, Thelen and Steinmo (1992) argue for a conception of institutions that emphasizes “intermediate-level institutions, such as party systems and the structure of economic interest groups like unions” and excludes “macrostructures such as class” (p. 11). Most historical institutionalists seem to operate with some such distinction between institutions and structures but do not spell out what it is that distinguishes institutions from structures.

I propose that *institutions* should be conceived as a certain kind of *structure*.² Like other structures, institutions constrain individuals; what distinguishes them has to do with *how* they constrain individuals. Noting that “institutions . . . have a legalistic aspect and rely on a relative clear structure of enforcement,” Levi (1990) defines the institutions that interest her as “formal arrangements for aggregating individuals and regulating their behavior through the use of explicit rules and decision processes enforced by an actor or set of actors formally recognized as possessing such power” (pp. 404-405). Unions, firms, parties, and electoral systems clearly fall within this definition of institutions whereas the distribution of wealth and the

2. It is noteworthy that Skocpol (1979), Hall (1986), and others describe their approach as “structuralist.”

sectoral composition of employment do not. Whether or not markets should be considered institutions, as Hall (1986, Chap. 2) would have it, remains an open question. One way to settle this question would be to say that markets have some institutional features (in the case of labor markets, employment contracts enforced by the state) and may be more or less institutionalized.

As a political economist, I am primarily concerned with economic-structural variables that fall outside Levi's definition of institutions, but it is important to note that political systems may also have structural features that do not qualify as institutions by this definition. For instance, the centralization of state power might be considered a structural feature that underlies various political institutions. Be that as it may, the distinction between structural variables that satisfy Levi's definition of institutions and those that do not cuts across the distinction between polity and economy.

Now, the point of all of this is the following. Whether they engage in comparative public policy or comparative political economy, historical institutionalists consistently emphasize the causal significance of institutional arrangements, as defined by Levi, and neglect the effects of other structural variables. Thelen's (1991, Chap. 6) analysis of variations in labor-management relations across industrial sectors in Germany illustrates this point. For Thelen, such variations depend on two variables: the legal rights of works councils and the organizational strength of unions. Her analysis does not consider competitive conditions in product markets, the organization of production, the structure of corporate ownership, or the degree to which firms in different sectors have engaged in investment abroad. Whether such variables provide some explanatory leverage on the outcomes that Thelen wishes to explain is, of course, an empirical question. My point here is simply that Thelen's institutionalist perspective directs her attention to other variables.

More broadly, Thelen (1991) and especially Turner (1992) tend to exaggerate the significance of firm-level bargaining between unions (or works councils) and management for industrial adjustment outcomes. As I have tried to show elsewhere (Pontusson, 1992b), corporate choices regarding technology and work organization can, in large measure, be seen as responses to the structure of constraints and opportunities set by product markets, labor markets, and capital markets. Arguably, this external context—external to the firm, that is—determines whether management will be responsive to union demands in firm-level bargaining.

Thelen's approach to cross-sectoral and cross-national variations in workplace politics is strikingly similar to Katzenstein's (1985) approach to the politics of class compromise in *Small States in World Markets*. Katzenstein first distinguishes among advanced capitalists in terms of institutional arrangements and then distinguishes among corporatist countries in terms of

the balance of power between labor and business, conceived as organized political actors. Economic openness (clearly a noninstitutional variable) plays an important role in Katzenstein's overall account of what is distinctive about small states, but the pressure of world markets is something of a "black box"—a background condition that small states have in common—rather than an object of sustained and differentiated analysis. Building on Katzenstein's analysis, Mjøset (1987) and Kurzer (1993) argue persuasively that the political consequences of openness vary significantly depending on the structure of domestic economies and the dynamics of international competition in leading export sectors.

In Katzenstein's, Thelen's, and Turner's works, the focus of attention is on direct bargaining between organizations representing labor and capital; structural power relations, specifically the systemic power of capital, receive relatively little attention. This said, let me emphasize what should already be clear: Sustained attention to (noninstitutional) structures need not entail a "class-theoretic" perspective. Sectoral analysis of industrial adjustment (including Katzenstein, 1984) represents another strand of comparative political economy that goes beyond the analytical confines of institutionalism.

WHY DO (SOME) INSTITUTIONS DESERVE ANALYTICAL PRIMACY?

The preceding discussion suggests that the historical institutionalist tradition encompasses at least three answers to the question, Which institutions matter? One answer—the most concise or narrow answer—is that state institutions matter. A second answer is that political institutions, as distinct from economic or social institutions, matter. The third answer is that institutions, as distinct from structures, matter or, to use Thelen's and Steinmo's formulation, that intermediate-level institutions matter.

None of these answers is based solely or even primarily on empirical research. Relying on case studies, the research design employed by historical institutionalists enables one to ascertain whether certain institutions matter and to explore how they matter, but this type of research design rarely enables one to determine with any confidence the influence of a particular set of institutions *relative to other institutional or noninstitutional variables*. I do not mean to impugn the legitimacy of historical and comparative case studies; for the kinds of questions that historical institutionalists ask, this may be the only viable research strategy. What I want to say is that this type of research strategy depends crucially on prior expectations of a theoretical nature (cf. Rothstein, 1992b, p. 51).

The most obvious, and perhaps the most compelling, case for assigning analytical primacy to political institutions is suggested by Nordlinger (1988) in his reply to Almond's (1988) attack on statism. Noting that "a statist perspective need not . . . suggest that the state regularly has a greater impact upon society than society upon the state," Nordlinger (p. 884) argues for according the state analytical priority on the grounds that it is the state—"its structures, performance, and policies"—that statist, and perhaps all political scientists, want to understand.

It may be appropriate to assign analytical primacy to state institutions, or to political institutions more generally, if our dependent variable is government policy (I shall return to this question), but are we satisfied with such a limited research agenda? Clearly, the editors of *Structuring Politics* and most of its contributors are not; as indicated earlier, the volume addresses a number of empirical questions that lie beyond the boundaries of comparative public policy such as why unionization rates and the balance of class power vary across countries, why different labor movements pursue different strategies, and how and why institutions change.

It is commonplace for historical institutionalists to argue the case for the analytical primacy of political institutions or intermediate-level institutions on comparativist grounds. Noting that "class differences characterize all capitalist countries and as an analytic category can be applied to all of them," Thelen and Steinmo (1992, p. 11) argue that what matters to political scientists is cross-national "differences in the salience of class to actual political behavior" and that such differences depend on the extent to which class is "reinforced and reified through state and societal institutions—party competition, union structures, and the like." In a similar vein, Rothstein (1992b) arrives at his argument that cross-national variations in union density can largely be explained in terms of political institutions—specifically, the organization and rules of unemployment insurance—by way of the observation that "differences in the development of the productive forces . . . cannot explain why Swedish workers are almost six times more organized than their French colleagues" (pp. 33-34).

This line of reasoning goes back to *Between Power and Plenty*, which is organized around the notion that advanced capitalist states pursued different policies in response to the same external shocks during the 1970s, most notably the oil crisis. In Katzenstein's (1978) introductory chapter, we again find the argument that although the Marxist approach helps us understand the "limitations which the world economy imposes," it is "not so helpful in explaining the different strategies which advanced industrial states actually pursue" (p. 13).

Two objections can be made to the comparativist case for assigning analytical primacy to political or intermediate-level institutions. First, one might object that the research program associated with historical institutionalism is biased in favor of cross-national variations. Clearly, there are important commonalities as well as variations among advanced capitalist states, and these commonalities should be explained rather than taken for granted. As comparativists, we are committed to comparison as a method of analysis but not necessarily to the proposition that cross-national variations are more important than commonalities.

This objection assumes particular salience to the extent that the current era of capitalist restructuring and political realignments is characterized by some degree of convergence of policies and/or institutional arrangements among advanced capitalist states. Whatever the merits or flaws of the case for generalized convergence, it is easy to come up with particular instances of convergence. King's (1992) important contribution to *Structuring Politics* describes one such instance: the adoption of similar work-welfare reforms in Britain and the United States despite different policy legacies and political-institutional arrangements. Rather than grappling with the question of what drives convergence, however, King focuses on how different institutional arrangements shaped the *process* of work-welfare reform in his two cases.³

The second objection to the comparativist case for assigning analytical primacy to political or intermediate-level institutions concerns the proposition that the socioeconomic institutions or structures of advanced capitalist countries are essentially the same. This proposition is typically advanced in conjunction with references to abstract Marxist categories—such as “class,” “the development of the productive forces,” and “the world economy”—and without much empirical evidence to support it. Of course, there is in fact a great deal of variation among advanced capitalist economies. Raw material endowments vary; so does the relative size of the primary, secondary, and tertiary sectors; the degree of concentration and centralization of capital; the degree of export dependence; the structure of ownership (e.g., the degree of public ownership), corporate organization, and management practices; and the list could go on.

Some of these economic-structural variables might well matter to the outcomes that political scientists and political economists want to explain. They might matter to the political salience of class (Thelen and Steinmo's question) or to union density (Rothstein's question). Also, they might well have affected the implications of the oil crisis of 1973 for particular countries

3. In a more recent version, King (in press) pursues the former problematic and adopts a more critical view of institutionalist explanations.

and hence their policy options (Katzenstein's question). My point here is that the causal significance of economic-structural variables ought to be an object of empirical analysis rather than being dismissed to clear the way for an analysis of the effects of political or intermediate-level institutions.

Remarkable in scope, Steinmo's (1993) analysis of the politics of taxation in Sweden, Britain, and the United States deserves to be noted in this context because Steinmo seeks to explain common patterns as well as variations in tax policy across his three cases. As Steinmo shows, all three countries adopted similar tax reforms during the 1980s. Steinmo attributes such common trends to the dynamics of capitalism and the tension between capitalism and democracy. Specifically, he argues that tax policy changes during the 1980s were a response to economic globalization (Chap. 6). However, Steinmo's account of cross-national variations in tax policy is couched almost entirely in terms of formal political institutions—the constraints and opportunities for political actors set by electoral systems and executive-legislative relations.

For Steinmo, economic-structural variables explain what advanced capitalist states have in common, and political-institutional variables explain why they differ. Turner's (1992) account of the politics of industrial adjustment conforms to a similar logic; for Turner, changes in technology and world markets generate pressures for change, but industrial relations institutions determine how industries in different countries respond to these pressures. From a theoretical point of view, this dualism strikes me as unsatisfactory, for why should some causal variables operate only over time and others only across cases? On empirical grounds, dualism might be defended as a "necessary evil," but before we resort to this argument, we ought to consider whether it is not the case that pressures emanating from the world economy or the economic institutions-structures of capitalism vary across cases. Steinmo's sketchy treatment of the forces behind and the effects of globalization stands in marked contrast to his careful analysis of political institutions, and the same is true of Turner's book. If the black box of economic variables had been opened, these authors might have been able to integrate their two problematics—to explain change and to explain cross-national variations—more effectively.

Returning to the question of why historical institutionalists assign analytical primacy to political or intermediate-level institutions, Rothstein's (1990) observation that "political institutions are not impersonal creations, such as social structures, but are instead shaped by conscious and deliberating, and more or less rational, human beings" (p. 317) suggests a line of reasoning that sidesteps claims about causal significance. Political institutions deserve analytical primacy, Rothstein argues, not because their causal significance is

necessarily greater than that of other variables but because these institutions are a product of human agency. Hence “institutional analysis can serve a bridge between structural and agent-oriented analysis in political science” (Rothstein, 1992b, p. 51).

What is interesting about institutions, according to Rothstein, has to do with their status as dependent rather than independent variables. His argument on this score does not really justify assigning analytical primacy to institutions and, indeed, may not be intended to do so. What Rothstein really wants to say, it seems, is that *processes of institutional design* deserve our attention. But why are processes of institutional design more interesting, from a theoretical point of view, than other processes of structural change? Again, it is crucial to recognize that social, economic, and political structures, not just institutional arrangements, vary across capitalist countries and over time. Such variations must also be viewed as results of human action. To be sure, large-scale processes of structural change, such as democratization or the formation of market economies, are different from the process of institutional design as Rothstein conceives it. In cases of institutional design, there are fewer actors involved and the consequences of their choices are perhaps better understood by the actors themselves, but human agency also operates in large-scale processes of structural change. Although Germans may never have chosen their particular type of capitalism, they did make choices that shaped the structure of German capitalism. Also, the process of structural change need not be as large scale as this example suggests. Consider the sale of council housing by British governments during the 1980s; in my terms, this represents a case of a deliberate attempt to reshape the structural, as distinct from the institutional, parameters of electoral competition (cf. Garrett, 1993).

Rothstein's own contribution to *Structuring Politics* suggests that the notion of institutions as rational constructs must be qualified. Rothstein (1992b) argues that the Ghent system of unemployment insurance (public subsidies to union-administered funds) provides selective incentives for individuals to join unions and demonstrates that countries that adopted the Ghent system ended up with higher unionization rates than did countries with compulsory, state-administered unemployment. However, Sweden is the only case in which the Ghent system was introduced by a labor party in government. Elsewhere, labor movements failed to see the strategic advantages of the Ghent system, and conservative parties introduced the Ghent system despite the fact that it favored organized labor and its political allies. Except in Sweden, ideological or tactical considerations apparently overrode long-term strategic considerations in the process of institutional design—or political actors simply misunderstood the long-term implications of their

institutional choices. To approach the question of institutional formation and change by focusing on cases of deliberate and successful strategic behavior would appear to be quite arbitrary and possibly misleading.

In sum, there may be sound reasons for students of comparative public policy to assign analytical primacy to political institutions, but these reasons do not hold for the broader research agenda set out in *Structuring Politics*. Neither comparativist considerations nor the structure-agency problematic provide a satisfactory justification for focusing on institutions at the expense of other structures.

INSTITUTIONS AND GOVERNMENT CAPABILITIES

As indicated previously, *Do Institutions Matter?* remains entirely within the tradition of comparative public policy. Indeed, the editors restrict their analytical concerns further by disavowing any intention to explain the goals that governments pursue and focusing entirely on the question of government "capabilities." Given the questions that the editors and contributors ask, what is interesting and most commendable about this volume is not that it affirms that political institutions do indeed matter but rather the many findings that qualify, and even challenge, the explanatory significance of political institutions.

Do Institutions Matter? is set up to test the significance of the differences between presidentialist and parliamentary systems for government performance. All the empirical chapters compare the United States and/or other countries with presidentialist systems to at least two countries with parliamentary systems and seek to determine whether the parliamentary cases exhibit similar government capabilities and whether the United States and/or other presidentialist cases differ from the parliamentary cases. (Vogel, 1993, p. 238, articulates the logic of this research design most clearly.)

In their chapter on the capacity of governments to impose benefit cuts on pensioners, Pierson and Weaver (1993) find that cuts have been greatest in the United Kingdom and lowest in Canada and that the United States falls between the other two cases. Whereas the chapter on targeting resources for industrial purposes does not yield such a continuum of outcomes, Krauss and Pierre (1993) emphasize differences between their two parliamentary cases, Sweden and Japan, and find no common features that distinguish these cases from the American case. Schick's (1993) chapter on government efforts to curtail budget deficits treats Sweden as a success and the Netherlands and the United States as failures, and Milner's (1993) chapter on trade policy shows that Britain, France, and the United Kingdom have pursued very similar policies.

Vogel (1993) does establish that British and Japanese environmental policymakers were less responsive to diffuse interests than were their American counterparts during the 1975-1988 period, but this pattern does not hold for his earlier periods (1945-1967 and 1968-1973). Employing a different research design, Lijphart, Rogowski, and Weaver (1993) also provide some support for the proposition that executive-legislative relations matter by showing that "semi-presidentialist" regimes (Finland, France, and Switzerland) manage ethnic and racial conflict more effectively than do parliamentary regimes, but the United States does no better than many parliamentary regimes on their measures of conflict. Because they engage in qualitative comparisons of more than three countries, Feigenbaum, Samuels, and Weaver's (1993) chapter on energy policy and Gunther and Mughan's (1993) chapter on cleavage management are less readily summarized in these terms. Suffice it to say that neither chapter yields consistent differences between presidentialist and parliamentary systems.

Weaver and Rockman (1993b) conclude that "second-tier institutional arrangements influence government capabilities at least as much as do the separation or fusion of executive and legislative power" (p. 449). By "second-tier arrangements," they mean electoral rules (proportional representation vs. winner-take-all system) and the "rules and norms guiding the formation of governments" (alternating party government, coalition government, and single-party dominance).

What Weaver and Rockman call "third-tier variables," which include noninstitutional variables, also figure prominently in the substantive chapters. For instance, the chapter by Feigenbaum et al. (1993, pp. 102-103) emphasizes the significance of federalism versus unitary government, the character of the bureaucracy, and the role of the judiciary; Pierson and Weaver (1993, p. 148) suggest that the length of electoral cycles matters greatly to the ability of governments to impose losses; Krauss and Pierre (1993, p. 173) emphasize the organization of business in accounting for the success of Japanese industrial policy; and Milner (1993, p. 367) points out that international institutions represent a common constraint on trade policy.

Beyond institutional variables, Feigenbaum et al. (1993) observe that cross-national differences in resource endowments have had a major impact on innovation in the realm of energy policy, and Vogel (1993) argues persuasively that the content and intensity of public opinion are the most important variables in determining "cross-national and intertemporal differences in environmental policy" (p. 265). The two chapters on the management of ethnic and racial cleavages converge on the proposition that the nature of intergroup relations, and the demands articulated by ethnic elites, matter as much as do political institutions. As Gunther and Mughan (1993) put it,

“certain types of electoral systems may facilitate certain kinds of elite behavior, but whether elites actually behave in accord with the incentives implied by these institutional relationships is a function of a variety of other variables, most of which involve elite political culture, calculations, tactics, and ultimate objectives.” Indeed, Gunther and Mughan add, “the choice of electoral rules can itself reflect elite culture and aspirations” (p. 293).

The problem with *Do Institutions Matter?* is that its research design does not enable us to determine the relative significance of second-tier institutions and these other variables. This would require us to compare, say, at least two country cases that combine parliamentary government with proportional representation to at least one case that combines parliamentary government with winner-take-all elections. Taken as a whole, the principal contribution of *Do Institutions Matter?* is to demonstrate that executive-legislative relations are *not* the crucial variable explaining differences in government capabilities.⁴

Why don't constitutional arrangements matter more? The editors and contributors to *Do Institutions Matter?* answer this question in terms of the “bidirectionality of institutional effects” (Weaver & Rockman, 1993, p. 460) and trade-offs among distinct governmental capabilities. “Bidirectionality” here means that institutional arrangements create both opportunities and risks for political actors. As Pierson and Weaver (1993, p. 113) argue in the case of pension cuts, the concentration of power implied by a parliamentary system enables the government in power to impose losses but also renders it more accountable for its actions. Which of these institutional effects will prevail is contingent on noninstitutional variables such as the margin of victory in the previous election and the credibility of opposition parties. In a somewhat different vein, Feigenbaum et al. (1993, p. 101) argue that the more open and fragmented nature of the American political system is a source of policy innovation but is also a constraint on coordination and implementation of new policies. And finally, Vogel (1993) argues that “the same institutional fragmentation of political power that hindered a coordinated effort to address environmental issues in the United States [during the 1950s and 1960s] also served to make it possible for environmental issues to become a part of the political agenda [during the 1970s and 1980s]” (p. 250).

The bottom line seems to be, as Feigenbaum et al. (1993) put it, that “institutional structures make a difference, but frequently their effectiveness can be judged only within a broad societal context” (p. 88). How, then, should the societal context that determines the effects of political institutions be conceived? On this question, *Do Institutions Matter?* has very little to offer.

4. See Immergut (1992) and Steinmo (1989, 1993) for contrary arguments and findings.

Needless to say, perhaps, my own approach to the comparative study of economic policy would emphasize the capitalist nature of economic institutions and noninstitutional structures.⁵

INSTITUTIONS AND INTERESTS

In their introduction to *Structuring Politics*, Thelen and Steinmo argue that the differences between historical institutionalism and rational choice institutionalism pivot on the issue of preference formation. According to Thelen and Steinmo (1992), rational choice institutionalists take the preferences of political actors as given—they “in effect ‘bracket’ the issue of preference formation theoretically”—and treat institutions as “features of a *strategic context*, imposing constraints on self-interested behavior.” By contrast, “historical institutionalists take the question of how individuals and groups define their self-interest as problematical” and “argue that not just the *strategies* but also the *goals* actors pursue are shaped by the institutional context” (pp. 7-8; emphases in original).

The theme that institutions shape interests also figures prominently in Hall’s (1986) *Governing the Economy*. Hall actually advances several different formulations on this theme. At one point, he observes that “if the interests and behavior of the working class in Britain and France have often been somewhat different . . . those differences can be traced, at least in part, to *historical variations in class organizations*” (p. 14). In a somewhat different vein, Hall suggests that the “*organization of policy-making*” affects not only “the degree of power that any one set of actors has over policy outcomes” but also “an actor’s definition of his own interests” (p. 19). Finally, and most broadly, Hall affirms that “the very interests of the actors themselves are critically affected by the organization of the *economic and political structures* within which they operate” (p. 233; all emphases added).

I would certainly not want to contest the claim that the organization of economic structures affects the nature and interests of economic actors, and I am entirely convinced that whether workers are organized into craft unions or industrial unions affects the way that they conceive their interests and the goals that unions pursue (cf. Pontusson, 1992a). The more original, but also more problematic, claim in this literature is that the interests and goals of similarly constituted interest groups—say, industrial unions representing the same categories of workers—vary significantly depending on institutional

5. For other policies, it might be necessary to specify the societal context differently. For instance, the comparative study of social policy needs to be situated in the context of demographic structure and gender relations as well as capitalism.

variables such as whether or not policy making is organized on a corporatist basis, whether elections are contested on the basis of proportional representation or a winner-take-all system, or whether laws provide unions with codetermination rights at the firm level.

To nail down this kind of argument, one would have to provide some theoretical specification of what the interests of particular actors would be in the absence of a given set of institutions or, alternatively, to engage in a carefully constructed comparative analysis of interest formation in different institutional settings. Hattam's (1992) discussion of the English and American labor movements during the late 19th century might be construed as an analysis of how institutions shape the interests or goals of social actors, but this is the only contribution to *Structuring Politics* that remotely fits the bill, and Hattam herself conceives the divergence between her two cases in terms of the pursuit of alternative *strategies* (i.e., a choice between legislative and marketplace strategies to advance worker interests). Elsewhere, Steinmo (1989, 1993) argues that corporatism provides the context in which Swedish labor, business, and state officials "define their tax policy goals" and shows that British labor, business, and state officials, operating in a different institutional context, have pursued different tax policy goals. This raises the question of what exactly is meant by *interests*. Are *interests* and *policy goals* (or preferences) coterminous? In many cases, it seems more appropriate to conceive policy goals as *means* to achieve interests. For instance, we might argue that union members in all countries have an interest in being covered by some form of health insurance. Some labor movements have sought to satisfy this interest by means of public health insurance, whereas others have pursued private (negotiated) solutions.⁶

The absence of any systematic attention to interest formation in the historical institutionalist tradition seems to derive from a theoretical perspective that posits broadly defined economic actors such as labor and capital, assumes their interests to be more or less self-evident, and then observes that the organizations representing these interests pursue different policy goals in different countries. If Swedish labor pursues policy goals different from those of British labor, this must be because political institutions differ.

The contrast between Swedish and Austrian employment performance brings out the limits of this approach. By all accounts, Sweden and Austria are the two most corporatist political economies in Western Europe, distinguished by centralized wage bargaining and corporatist policy making arrangements. It is a commonplace that both countries successfully avoided mass unemployment during the 1980s but less well known that they differ markedly with respect to other aspects of employment performance. Whereas

6. Immergut's (1992) essay on health care reform in Sweden, France, and Switzerland simply sidesteps this issue.

Sweden has the highest rate of labor force participation of all the Organization for Economic Cooperation and Development (OECD) countries (80%), Austria ranks as a middling country on this score (63%). And whereas Sweden ranks at the top of the OECD league on wage compression among manual workers in industry, Austria is the lowest-ranking European OECD country on this score.⁷

In large measure, these differences can be attributed to government policy and the wage bargaining stance adopted by national unions. One would be hard put to argue that Swedish unions have been so much more influential than have Austrian unions. More plausibly, Swedish unions have pursued different goals within a similar institutional setting. If this is so, the crucial question seems to be, Whose interests or, more precisely, *which worker interests do unions represent?*

This type of question resonates well with historical institutionalist thinking in that it implies that the organization of interests matters and that organizations cannot be “reduced” to the classes that they represent. On the other hand, one cannot begin to answer the question without engaging in an analysis of the composition of the working class and the interests of its various segments and thus move beyond an analysis of formal institutions. If “crude Marxists” (whoever they are) commonly reduce interest groups to classes, historical institutionalists all too commonly commit the opposite fallacy by substituting interest groups for classes.

Thelen and Steinmo (1992) argue that traditional interest-group theories “often missed crucial elements of the playing field” by focusing on the “characteristics and preferences of the pressure groups themselves” and therefore failed to explain why groups with “similar organizational characteristics . . . and preferences could not always influence policy in the same way or to the same extent in different national contexts” (p. 5). The contrast between Swedish and Austrian employment performance suggests that something might be gained by attending to “the groups themselves” and by situating them in a structural as well as institutional context.⁸

INSTITUTIONAL FORMATION AND CHANGE

The problem of explaining institutions—why they differ across countries and how they change over time—brings out the limits of institutionalist

7. These contrasts are documented and their significance for the literature on corporatism is discussed by several contributors to Pekkarinen, Pohjola, and Rowthorn (1992)

8. See Pontusson (1994) for an attempt to explain the previously noted differences between Swedish and Austrian employment performance.

analysis most clearly. As noted previously, Thelen and Steinmo (1992) place issues of institutional dynamism at the top of the historical institutionalist research agenda. However, only some of the contributors to *Structuring Politics* (Rothstein, Dunlavy, and Hattam) engage in a sustained analysis of institutional formation and change; others (Hall, King, and Weir) instead deal with institutional dynamism by looking at policy change within a relatively stable institutional framework. Moreover, those contributors that do address the problem of institutional formation and change adopt very different approaches.

Billed as an exemplary case study of institutional innovation by the editors, Rothstein's contribution to *Structuring Politics* demonstrates that (a) countries that adopted the Ghent system of unemployment insurance ended up with unusually high levels of union density and that (b) Sweden is the only case in which the Ghent system was introduced by a labor party in government. In view of these findings, "the question that has to be answered" for Rothstein (1992b, p. 48) is whether the Swedish experience represents a case of deliberate institutional design. Rothstein answers this question in the affirmative, showing that Gustav Möller, the minister in charge of the unemployment insurance reform of 1934, believed that a Ghent-type system would promote unionization and used this argument to persuade union leaders and Social Democratic members of Parliament to support a compromise with the liberals on the specifics of the reform legislation.

The question of intentionality is certainly interesting, but does Rothstein's answer to this question constitute an explanation of why Sweden ended up with the Ghent system? What needs to be explained is how Möller, and the leadership of the Swedish labor movement in general, came to understand that the Ghent system would serve labor's organizational interests or, conversely, why other labor movements failed to understand its benefits. This would presumably involve a discussion of the formative experiences of different labor movements and their relations to the state.⁹

An adequate explanation of the unemployment insurance reform of 1934 must also show how it was possible for the Swedish labor movement to legislate its preferred solution to unemployment insurance at this particular time. The tactical maneuvering surrounding the 1934 legislation must be situated in a broader context for this purpose. After all, unemployment insurance was but one of a series of institutional reforms that accompanied the ascendancy of Swedish Social Democracy from 1932 through the immediate postwar period. The class-coalitional realignments that enabled the Social Democrats to consolidate control of the government are well-known

9. Rothstein (1992a) engages in such a discussion; see also King and Rothstein (1993).

and need not be rehashed here. What needs to be noted, in view of Rothstein's particular concerns, is the fact that the rate of unionization among industrial workers increased from 41% in 1920 to 83% in 1940 (Kjellberg, 1983, p. 270). Whereas the unemployment insurance reform did not have any effect on union membership during the 1930s by Rothstein's own account, the growth of unions during the interwar period is bound to have had some effect on the politics of unemployment insurance and social reform more generally. Rothstein's account of the 1934 unemployment insurance reform sidesteps the problem of modeling the complex causality involved here.

In contrast to Rothstein's chapter, Hattam's and Dunlavy's contributions to *Structuring Politics* are not concerned with the creation of a particular institution; rather, they deal with changes in relations among institutions and the salience of particular institutions (i.e., changes in the overall configuration of institutional arrangements). As indicated earlier, Hattam (1992) seeks to explain the strategic divergence of the British and American labor movements during the late 19th century in terms of the opportunities to override judicial resistance to union organizing by legislative means in the British case and the lack of such opportunities in the American case. Hattam points out that constitutional arrangements alone cannot explain the strategic divergence of the two labor movements because the British judiciary was subordinate and the American judiciary was autonomous all along. Hattam argues that the courts and their constitutional standing became a salient variable only as workers came to be organized in unions and engaged in collective action on a class basis as opposed to an earlier labor politics based on the distinction between "producing" and "nonproducing" classes. In other words, the institutional framework regulating labor-capital relations changed as new forms of labor organization emerged. Why, then, did the organization and demands of labor change during the last decades of the 19th century? Noting differences among American working-class organizations, Hattam (1992) suggests that "different ideologies and interpretive frames played a considerable role in shaping workers' demands" (p. 177), but if a shift to class-based labor organization occurred in Britain as well as in the United States at this time, the underlying process of industrialization and proletarianization must play an important explanatory role.

Dunlavy's (1992) analysis begins with the observation that government participation in and regulation of railroad development was much more pronounced in the United States than it was in Prussia prior to 1850. Dunlavy proceeds to observe that railroad policy changed in both Prussia and the United States during the 1850s, but it is clear from her account that it was not just policy that changed. It seems more appropriate to speak of a change in the institutional framework of railroad policy. In Prussia, the state became

directly involved in financing railroads and adopted a more regulatory posture. In the United States, by contrast, the state governments divested themselves of railroad assets, and the “seat of regulatory power” (p. 139) shifted to the courts and the federal government.

Dunlavy argues that political liberalization in the wake of the revolution of 1848 cleared the way for increasing state expenditures on railroad development in Prussia but that, at a deeper level of causation, institutional changes in both Prussia and the United States derived from the nature of the railroad development itself—specifically, its capital intensity and increasing geographic scale. In the United States, state legislatures lost the ability to regulate the railroad industry as it became the first interstate business.

In Dunlavy’s account, and perhaps in Hattam’s as well, changes in socioeconomic context brought about changes in political institutions and the relations among them. Whereas Dunlavy at times seems to treat institutional change as a product of impersonal forces (capital requirements and geographic scale), Rothstein treats institutional change as a product of the insight and will of a single individual (Möller). From a theoretical point of view, neither perspective seems entirely satisfactory. Must we really choose between them? To my mind, the notion of *collective actors*, conceived as groups of individuals who share certain interests and who are tied to each other by more or less formal and more or less permanent organizations, provides the most obvious way to steer a course between the Scylla of “structural determinism” and the Charybdis of “voluntarist contingency.”

The statist literature commonly treats state officials as a collective actor who engages in institutional reform in response to external shocks or the malfunctioning of existing institutions (cf. Krasner, 1984). As Hall’s (1993) analysis of the recent paradigm shift in British economic policy suggests, this model of institutional change does not adequately capture the process of large-scale institutional realignment in advanced (liberal-democratic) capitalism. I shall not develop this critique of statism further here; rather, I want to sketch an alternative approach that draws on Gourevitch’s (1986) comparative analysis of policy responses to economic crisis.

For Gourevitch, institutional arrangements always have a policy content and benefit some economic interests at the expense of others. Institutional configurations—let us call them *policy regimes*—are a product of struggles and alliances among collective actors defined by economic interests (typically but not necessarily producer interests), and institutional realignments occur when the interests and/or the power of relevant actors change. Along with his emphasis on cross-class alliances, the most important feature of

Gourevitch's approach is the proposition that the interests of producer groups (or class fractions) are variable and therefore must be specified in a particular historical context.¹⁰

Marketplace conditions may alter the interests of producer groups. Also, long-term changes in production technology and organization may alter the interests and policy preferences of specific groups of industrialists, financial institutions, workers, or farmers. On the first point, Gourevitch suggests that steel and other capital-intensive industries tended to be most hostile to labor and engineering industries more accommodative of labor demands during the 1920s, but the world economic collapse of 1929-1931 reversed this lineup as heavy industry sought government protection against foreign competition while engineering firms sought to maintain their competitiveness by cutting wage costs.

Let me illustrate the point about long-term changes in technology and production with reference to the Swedish case. Since the early 1980s, Swedish employers have engaged in a more or less successful campaign to decentralize wage bargaining and thereby undermine the ability of the unions to pursue a solidaristic wage policy (see Pontusson & Swenson, 1994). What is interesting about this development, which has far-reaching implications for the overall institutional configuration of the Swedish political economy, is that the engineering employers that have spearheaded the campaign to decentralize wage bargaining played an important role in the institutionalization of peak-level bargaining and solidaristic wage norms during the 1950s. The key question becomes, Why have the institutional preferences of Swedish engineering employers changed?

My answer proceeds from the observation that employers have two distinct and sometimes contradictory interests with respect to wage formation: On one hand, they have an interest in wage restraint and, on the other hand, they have an interest in being able to use wages as a means to recruit and motivate workers. Which of these interests dominates depends on both labor market conditions and production strategies. During the expansion of Fordist mass production during the postwar period, the employer interest in wage restraint prevailed because management control over labor was vested in production technology. As the viability of Fordist mass production strate-

10. Gourevitch (1986) uses the term "situational interest" (p. 87) to convey this idea. Although I agree with Gourevitch's emphasis on the variability of interests, I believe that he mistakenly jumps to the conclusion that one cannot theorize about interests; I believe that this weakens his analysis considerably. Most of the time, Gourevitch seems to derive the interests of any particular producer group inductively from statements or behavior by its organizations. This preempts any serious attention to the question of how interests are translated into policy preferences.

gies declined during the 1970s and 1980s and engineering employers pursued work reorganization that involved increased reliance on worker skills and motivation to achieve quality and productivity improvements, wage flexibility became more important to these employers and the rigidities of centralized wage bargaining became more irksome. This argument explains why some Swedish employers have been more militant in their pursuit of decentralization than have others and why employers in other advanced capitalist countries have also engaged in efforts to redesign wage bargaining institutions for the purpose of enhancing wage flexibility.

The growing interest in the politics of capital, or organized business, among students of comparative political economy (e.g., Kurzer, 1993; Swenson, 1991) clearly reflects the dominant role that business actors have come to assume during the current era of policy realignments and institutional change. By and large, it would appear that institutional changes have occurred in those countries (and policy arenas) where significant segments of the business community have turned against the postwar settlement and that postwar arrangements have persisted where they have suited dominant business interests. The systemic power of capital, embedded in the structures of capitalism, provides the most obvious explanation of this pattern, which is not readily explicable within the institutionalist framework of the corporatism literature.

Equally important, the Swedish case illustrates how an analysis of interest formation enables us to link the politics of institutional change to the dynamics of capitalism. This is not to deny that existing institutions influence the politics of institutional change and thus affect the character of emergent institutions. The point is that to understand the impetus for change, we need to analyze the process whereby "extra-institutional" forces reshape the interests of powerful actors.¹¹

As noted earlier, several contributors to *Structuring Politics* emphasize the role of ideas in their discussion of policy change within more or less stable institutions (Hall, King, and Weir), and this represents an important theme that distinguishes *Structuring Politics* and other recent work from the "classics" of the historical institutionalist tradition. For these authors, ideas at times seem to assume the role that interests assume in the approach to institutional change that I have set out. I do not wish to enter into this debate here, but it would be a mistake, I think, to substitute ideas for interests. I am

11. For the Swedish case, Pontusson and Swenson (1994) integrate the preceding argument about employer interests with an analysis of what Thelen (1991) refers to as political or strategic "maneuvering within institutions" (an expression that also appears in Thelen & Steinmo, 1992, p. 17).

inclined to think that Hall (1993) goes too far when he argues that “the play of ideas was as important . . . as was the contest for power” (p. 289) for the shift from a Keynesian to a monetarist policy paradigm in Britain during the late 1970s; but even if Hall is right, the the following point still holds: Ideas may be very important to the politics of change, but interests are also important and must be an object of serious analysis.

CONCLUSION

A major strand of the historical institutionalist tradition—arguably the dominant strand—assigns analytical primacy to state institutions or political institutions more broadly conceived. A clean break with the polity-centeredness that characterizes much of this literature would, I think, serve to advance the research agenda of comparative political economy. If you wish, I advocate that political economists should go beyond the comparative study of advanced capitalist states or industrial relations to engage in the comparative study of advanced capitalism. This means that we should pay more systematic attention not only to economic institutions but also to a range of economic-structural variables that lie beyond the conventional confines of institutional analysis.

Why is such a twofold shift in analytical attention (from capitalist states to capitalism and from institutions to structures) either necessary or desirable? What is to be gained? The proposition that we should move from the study of capitalist states to the study of capitalism actually misses my main point, which is that we need to engage in a comparative study of advanced capitalism to advance the comparative study of advanced capitalist states (and industrial relations). The analytical shift that I propose has two major merits: First, it serves to uncover structural power relations and, second, it provides the basis for an analysis of economic interests and the forces that shape them. As I have tried to indicate, these considerations enable us to get a better handle on the politics of institutional change, but I would argue that they also shed significant light on how existing institutions operate and why their effects sometimes change over time.

I recognize fully that the alternative approach that I have staked out needs further elaboration to be truly compelling. The preceding discussion raises a number of questions about what we mean by *capitalism* and how we might study it, and it does not settle any of these questions. To illustrate the kinds of questions I have in mind, any analysis of capitalism must deal with the

problem of the weight to be assigned, on one hand, to markets and the dynamics of intercapitalist competition and, on the other hand, to relations of production and the dynamics of labor-capital conflict. And however one resolves this problem, the question of the appropriate level of analysis remains. Should capitalism be studied at the level of sectors and sectoral governance regimes, national models of accumulation and regulation, or the world economy and its regions?

As it stands, my alternative approach is vulnerable to the objection that it simply broadens the number of variables that students of comparative political economy must take into account (i.e., that "everything matters"). At the same time, I imagine that some readers might be preparing to charge me with "economic reductionism" or "structural determinism." In response to the first objection, I would say that we need to specify the institutions and structures of capitalism that are most salient, but then the question immediately arises, Salient for what purpose? The question of causal significance cannot be treated simply as an empirical problem; any research strategy, and especially one that relies on comparative case studies, presupposes certain theoretical assumptions. Yet such assumptions also cannot be made in the abstract; the question of analytical primacy (or salience) depends on what it is that we want to explain.

Let me close with some brief comments on the question of economic or structural determinism. First, I want to reiterate that institutions, as conceived here and in most of the literature, are a kind of structure and that the distinction between institutional and noninstitutional structures cuts across the distinction between economy and polity. Moreover, I would not want to state that noninstitutional structures are, in general, "more important" than institutional structures. In many instances, structures influence political or economic actors through institutional arrangements, and it is both pointless and impossible to assign separate explanatory weight to these variables. As Gourevitch (1986, Chap. 3) argues, the crucial analytical issue is not which variables matter most in any particular instance but rather where we begin and how we organize our analysis.

Finally, I have tried to suggest that issues of choice and contingency remain as one shifts attention from institutions to structures. The dangers of structural determinism are neither more nor less serious than are the dangers of institutional determinism. In emphasizing the need to take into account structural variables that cannot be conceived as institutions, I do not mean to deny the importance of actors; quite the contrary, the point is to prepare the ground, analytically, for a more systematic treatment of collective actors and their interests.

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