POLITICAL JURISPRUDENCE, THE "NEW INSTITUTIONALISM," AND THE FUTURE OF PUBLIC LAW

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For the last quarter century public-law studies have been dominated by "political jurisprudence," which tries to understand law as a product of political forces. Critics claim this outlook, as now articulated, has generated fragmented empirical work disconnected from larger normative issues. This essay uses recent "institutionalist" or "structuralist" perspectives, based on critiques of pluralist political science and both Marxist and functionalist sociology, to propose a political approach to public law studies that can avoid such criticisms. If both empirical and normative public-law scholars took as their central concern the "dialectic of meaningful actions and structural determinants" and recast their research in several specified ways, they might be better able to describe the role of normative ideas in law and to achieve a broader empirical agenda that could ground and inform normative debates.

In a 1984 symposium, several leading political scientists specializing in public law considered what directions their subfield might take in the future. They generally agreed that a "vision of political jurisprudence," calling for analysis of law and courts as aspects of broader political processes, dominated work in the field. But they perceived a plurality of approaches under this label, some in sharp tension with others. Studies focusing on the interplay of courts and interest groups; the politics of appointive processes; courts as small-group decision makers; the implementation and consequences of judicial rulings; the socioeconomic makeup of bench and bar; statistical-attitudinal analyses of judicial voting behavior; litigative versus nonlitigative means of conflict resolution; comparative studies of legal systems; and much more were all identified as currents flowing within the broad river of political jurisprudence (Danelski 1984; O'Brien 1984; Sarat 1984; Shapiro 1984; Stumpf 1984).

Reflecting on this diversity, David O'Brien (1984, 561) argued that despite its hegemony, political jurisprudence had proven unable truly to unify the subfield into an influential intellectual discipline. He traced this inability to the approach's failure to link "normative jurisprudence and positive political analysis" adequately. Empirical work had splintered and sometimes faltered due to the absence of any theoretical structure that could draw on normative concerns to define an appropriate agenda for political jurisprudence's "descriptive enterprise."

In contrast, Martin Shapiro (1984, 543–44) opposed seeking such a synthesis. He insisted that political jurisprudence had succeeded in becoming the modern orthodoxy even in law schools. Much progress would be lost, he feared, if careful empirical work should give way to a more old-fashioned "jurisprudence of

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values" that would be "really a branch of normative political philosophy." Yet he acknowledged regretfully that many younger scholars were moving in precisely this direction.

The symposium's portrait of the subfield suggests a multiplicity of approaches that threatens to break out into renewed clashes between "behavioralist" and "normative" public-law scholars, now perhaps compounded by intergenerational divisions. This essay argues that any revival of these longstanding feuds is pointless and avoidable. Recent directions in research in a number of fields, of a sort dubbed the "new institutionalism" by James March and Johan Olsen (1984), suggest how public-law scholarship might be recast to unify many of its longstanding descriptive and normative concerns. By explicitly designing their studies as explorations of what Theda Skocpol (1984, 4) has termed "the dialectic of meaningful actions and structural determinants," political scientists with many different interests might pursue them so as to facilitate the communication and comparison of their results not only among each other but with the work of empirical political scientists and normative theorists more generally. That possibility, to be sure, is speculative, but it is worth considering the potential of these recent trends to provide more common ground for public-law scholars.

Two Roads to the New Institutionalism

It is only fair to acknowledge at the outset that much of my own previous work falls squarely within what Shapiro labels, rightly, the *jurisprudence of values.* Even so, I have come to share his concern that public-law scholarship will not flourish if all scholars focus simply on spinning out their own normative legal theories. While the subfield cannot be vital without vigorous normative debates, the ratio of new insights to reworkings of established views will be disturbingly low if disputes over values are all that is on our agenda.

But many recent efforts to reinvigorate qualitative inquiries into norms, values, and ideologies within public-law scholarship do not really represent efforts by legal scholars to be political or moral philosophers. Rather, they are attempts to integrate the study of ideas in law with *descriptive* studies of the historical evolution of political institutions and behavior. These authors regard qualitative studies of the patterns of reasoning characteristic of various strains of legal discourse as investigations into one dimension of actual political conduct—a dimension that needs to be assessed like any other if we are to build up a comprehensive empirical portrait of political life (Gordon 1984, 57–125; Smith 1985, 5–9).

It is true that this focus on values and ideologies derives from several objections to how political and legal evolution have previously been depicted, including reductionist treatments of normative issues. And while many recent writings aim at better empirical accounts of legal ideas, it is also true that they can be quite valuable for normative debates. Much of this sort of descriptive work turns out, incidentally but felicitously, to dovetail nicely with emerging modes of moral argument. Even so, the claim that qualitative studies of the historical role of legal ideas can contribute to empirical political science as well as to moral philosophy deserves to be taken seriously.

A major reason for granting credibility to this claim is that there are two distinguishable roads leading to the study of ideas in law as part of the new, developmentally focused "institutional" analyses March and Olsen describe. These roads take as their points of departure two distinct and important academic strains, one in contemporary political science and one in sociology. Their convergence suggests that "new institutionalist" accounts are indeed responding to real weaknesses in current perspectives.

The road March and Olsen stress is a path of reaction against the treatment of legal and political institutions simply as epiphenomena of self-interested individual and group behavior—a treatment dominant in mainstream U.S. political science since the 1950s and visible in much of what Shapiro terms *political jurisprudence*. The other road is one trafficked mostly by neo-Marxist comparative scholars and somewhat like-minded members of the Conference on Critical Legal Studies. These writers are reacting against traditional Marxist and sociological reductions of such institutions to epiphenomena of economic relations or the functional needs of social systems. Instead, they stress the "relative autonomy" of political and legal organizations, including recurring organizations of legal ideas, from all such socioeconomic "deep structures."

As suggested above, the convergence point of both reactions is on the importance of the interrelationship between human "institutions" or "structures" and the decisions and actions of political actors. In these approaches to the study of politics, institutions are expected to shape the interests, resources, and ultimately the conduct of political actors, such as judges, governmental officials generally, party or interest-group leaders, and other identifiable persons. The actions of such persons are in turn expected to reshape those institutions more or less extensively. Ideally, then, a full account of an important political event would consider both the ways the context of "background" institutions influenced the political actions in question, and the ways in which those actions altered relevant contextual structures or institutions.

The term *institutions* as used here has a quite inclusive meaning but no more inclusive than Samuel P. Huntington's definition of institutions as "stable, valued, recurring patterns of behavior" (Huntington 1968, 12). Indeed, a focus on the "interplay of meaningful actions and structural contexts" means that political scientists might plausibly narrow Huntington's definition somewhat (Skocpol 1984, 1). They would normally attend only to relatively enduring patterns of behavior that (1) have arguable importance for human decisions that significantly shape social development and (2) appear subject to meaningful modification through such choices and conflicts. Nonetheless, this definition is broad enough to include not only fairly concrete organizations, such as governmental agencies, but also cognitive structures, such as the patterns of rhetorical legitimation characteristic of certain traditions of political discourse or the sorts of associated values found in popular "belief systems."

Thus these definitions can be useful in analyzing the elements and tendencies of legal and political outlooks or ideologies as well as behavioral regularities more narrowly defined. They also preserve at least the possibility that concrete political choices will prove to have important consequences, intended and unintended, that result not from any shaping "structural context" but from the actors' own creativity. It is their attention to the role of ideas and their assumption of the potential meaningfulness of political decisions that make these approaches well suited to speak to normative as well as empirical concerns. They can provide descriptive materials for the sorts of pragmatist moral arguments increasingly advocated by a diverse range of political theorists. Richard Rorty (1979, 1986), Alasdaire MacIntyre (1981), and Michael Walzer (1983), for example, all agree that philosophers can best assist prudential deliberations on current issues by identifying and assessing our constitutive moral traditions and their historic role in political life.

Such accounts are precisely what descriptions of legal ideologies as political "institutions" or "structures" provide. Attracted by the promise of such benefits, then, let us begin appraising the arguments for these approaches by examining the two roads to the new institutionalism in more detail.

The Road from Mainstream Political Science

As March and Olsen argue, since the behavioral revolution of the 1950s, much U.S. political science has analyzed politics chiefly in terms of the conduct of individuals or organized groups. Their behavior is normally portrayed as the result of rational calculations designed to advance the individual's or group's self-interest. The "behavior" of political institutions, in turn, is treated as simply the product of complexly interacting individual and group choices and actions. Such analyses tend, moreover, to regard the interests and resources of political actors, and hence much of the content and consequences of political action, as ultimately reflective of deeper social and economic forces which shape politics much more than politics shapes them. Thus the preferences and powers of political actors are often treated as exogenous "givens" in political analysis. (Although many desires may of course be described as internal to persons, those impulses are regarded as determining forces that are relatively immune to political action—so their roots are not analyzed when politics is studied.) And while such preferences or interests might be defined quite diversely, much modern political science presumes that concerns to enhance one's economic condition and political power are usually decisive (March and Olsen 1984, 735–38).

Numerous writers also assume that the political choices born of rationally self-interested calculations usually result in outcomes that are "efficient" or "functional," at least for the dominant elements in the prevailing social and political structures (March and Olsen 1984, 737). As March and Olsen fail to note, however, many critics have assaulted the political science of the 1950s for assuming too complacently that all possible "groups" latent in a society can develop organizations to protect their interests effectively. These writers hold that many significant interests are frequently left inarticulate, excluded, or systematically slighted (Connolly 1969, 13–19; Lowi 1979, 57–63; McConnell 1966, 7–8). Thus they believe the apparent systemic "efficiency" of the decisions of ruling groups is often accompanied by frustration for others, who may ultimately produce significant social ruptures. But after making these important criticisms, only a few of these analysts have gone on to challenge the prevailing picture of politics as largely a matter of self-interested individual and group behavior.

According to the view still dominant in political science, then, politics is comprehended as kind of drama at sea. It occurs on the surface, where individuals and groups sail along, rationally navigating in pursuit of booty, frequently clashing with each other in the process. Their fates, however, frequently depend on the dynamics of the largely uncontrollable economic, technological, and social forces that surround them. The student of politics is mostly concerned with making sense of the calculations and actions of the pilots and crews, but the analysis often rests on showing how they could not safely act otherwise.

In some important respects, this image of political life has only been polished by the development of rational-choice theory into a more formal paradigm for virtually all political and social, as well as economic, analyses, although certain significant departures are visible as well. Leading works in this mode have stressed the constraints that information costs impose on decision making, giving this portrait of human conduct more empirical plausibility. They have then explored the interconnections between calculations of individual interest, particularly those of political "entrepreneurs," and the creation of organized self-interested groups. Much attention has also been given to how institutions try (and often fail) to aggregate individual choices coherently under various decision rules, and how these complications affect political calculations in groups small and large. And rational-choice theorists continue to encompass more and more political behavior within such analytical frameworks (Buchanan and Tullock 1962; March and Simon 1958; Mueller 1979; Olson 1965; Riker 1962; Simon 1957).

Many of these analysts do break significantly with more traditional studies of individual and group behavior in at least two respects. They are often unconcerned about whether rational-choice models really *describe* the actual decision processes of human beings in any very precise way. For example, the economist-lawyer Richard Posner (1977, 12–14) follows Milton Friedman in maintaining that all truly scientific theories are abstractions that are "necessarily 'unrealistic' when compared directly to actual conditions." The question should be not whether a theory captures "the full complexity, richness, and confusion" of human behavior, but whether it proves to have "utility in predicting" conduct, so that it can be relied on in designing policies and political strategies. In Posner's view, rational-choice models used by economists and other public-choice theorists have been shown to have "surprising" predictive power, far more than the available alternatives; so they offer the most promising direction for social-scientific research.

Partly as a result of this diminished concern to describe social reality in all its complexity, many rational-choice analysts have also not been greatly concerned to explore how far the array of effective, organized groups in political life mirrors the "actual" structure of group interests in society. Because their models suggest that nearly all groups are products of self-interested political organizers and subsidizers offering incentives that generate group membership and activity, it is the presence, skill, and opportunities of such leaders and patrons that determine whether a potent interest group will emerge. Thus while the disadvantaged clearly face special burdens in gaining organized advocacy, there seems little point in trying to determine what groups, organized and unorganized, might reflect the "real" structure of interests in society. We can best answer questions about why certain interests are and are not represented by keeping our focus on the calculations and resources of these leading political actors (Hansen 1985, 93–95; Salisbury 1969, 11, 23–24; Walker 1983, 402–4).

These differences, however, only take to an extreme certain tendencies in earlier mainstream political science, particularly the inclination to use self-interested calculations of wealth and power maximization as the basic model of political behavior. Thus rational-choice models are not likely to satisfy those who believe that this perspective recognizes at best a narrow subset of possible human standards, and they are even more frustrating to those who believe much political science is methodologically insensitive to the claims of the oppressed. Yet for many, the rapid growth of rational-choice analyses in several disciplines suggests that this general approach to politics will eventually be able to provide more theoretically rigorous and empirically falsifiable accounts of virtually all of political and social life.

Hence we should note that William Riker (1980, 444–45) attempted a particularly relevant expansion of the rational-choice paradigm when he called for a "return to the study of institutions," out of recognition that they systematically exclude or include certain "tastes or values." He cited pioneering studies by Kenneth Shepsle (1979) on how a congressional committee system may achieve "structural equilibrium"—a condition in which it is possible to pass motions that genuinely cannot be defeated by any other alternatives—if committee jurisdictions compel the disaggregation of multidimensional issues and if certain other requirements for members' preferences are met. More generally, Riker argued that we must use the tools of rational-choice theory to explore how institutional arrangements, themselves the products of past political choices, act as "congealed tastes" that influence the kinds of values that are "feasible and likely" outcomes of current decision processes.

While such extensions of the rational-choice paradigm to include institutional analyses are important, they do not alter the approach decisively. These writers still contend that one should build up a picture of institutional structures by stressing the individual and group decisions and actions that led to their creation; and they continue to treat tastes and preferences "as given." These tastes are impulses that may be blocked or advanced by different institutional arrangements, but they are otherwise exogenous to institutions, products of other forces that in the long run "cannot be denied" (Riker 1980, 444; Riker 1982, 190). These writers also still conceive of "tastes or values" in utilitarian fashion, as preferences—usually for wealth or power—to be maximized. Hence their focus remains on individual and group calculations of how to achieve values that are thought of as externally determined and described in rather reductionist terms.

It would be more tedious than difficult to show that much scholarship in "political jurisprudence" falls within the broader behavioral approach that since the 1950s has led, among other things, to the proliferation of rational-choice analyses. A few leading examples should suffice. None could be more appropriate than the seminal exposition of that jurisprudence by Martin Shapiro (1964, 7–8), where he called for examination of institutions in terms of "the behavior of their personnel, and their places in the various decision processes." And he identified David Truman's classic effort "to analyze all government in terms of the influence and interactions of interest groups" as a chief "catalyst for the new jurisprudence." Shapiro's subsequent work consistently analyzes judicial decisions as responses to constituent claims and as expressions of power-seeking political actors, as does, for example, Stuart Scheingold's influential essay on the "politics of rights" and the growing literature linking patterns of judicial decisions with patterns of party realignment (Adamany 1980; Funston 1975; Lasser 1985; Scheingold 1974; Shapiro 1978, 1981, 1986). The many empirical analyses that connect judges' decisions to attributes such as their socioeconomic, educational, or professional backgrounds or to "values" treated as external "givens," rest on a similar picture of political actors as calculators advancing preferences or interests that are more the results of powerful sociological forces than their own deliberations (Gibson 1978; Goldman 1982; Rohde and Spaeth 1976; Schubert 1975; Tate 1981).

Recently, drawing on the newer rational-choice mode of analysis, Jeffrey Segal (1984) has employed the contemporary recognition of bounded rationality to describe how judges simplify their choice problems by relying on cues, such as certain facts about each case. At varying levels of formality, other political scientists have used the game-theoretic-style calculations involved in negotiation among multiple members of a decision-making group in the problems of aggregating choices, to clarify judicial strategies and to determine likelihood of winning coalitions in decision-making processes (Murphy 1964; Rohde 1972).

All this work is valuable, the image of politics that it elaborates is in many ways compelling, and it is at best premature to dismiss the hope that these endeavors will lead us closer to more truly scientific political studies. Nonetheless, the limitations of these approaches suggest that their underlying conceptions of politics and their paradigms for political analysis must be significantly if not fundamentally modified to be fully adequate to human experience. While March and Olsen and others canvass many objections, the root problem is that all these fairly diverse writers treat the resources, the institutional environment, and especially the very values and interests of political actors as exogenous, as determinants of political-choice situations that shape events while remaining more or less impervious to conscious human direction themselves.

As March and Olsen note, experience seriously challenges this picture. Political institutions appear to be "more than simply mirrors of social forces." They are themselves created by past human political decisions that were in some measure discretionary, and to some degree they are alterable by future ones. They also have a kind of life of their own. They influence the self-conception of those who occupy roles defined by them in ways that can give those persons distinctively "institutional" perspectives. Hence such institutions can play a part in affecting the political behavior that reshapes them in turn—making them appropriate as units of analysis in their own right.

The role of institutions, moreover, goes well beyond providing the rules governing decision-making situations in the manner Riker stresses. It influences the relative resources and the senses of purpose and principle that political actors possess. And sometimes, at least, those purposes and principles may be better described as conceptions of duty or inherently meaningful action than as egoistic preferences. Correspondingly, the behavior they alter may serve other values than economic or systemic functionality (March and Olsen 1984, 738–42).

These criticisms can be made more concrete by placing them in the context of public-law research. It is unquestionably of some value to know that, for example, the votes of moderate justices in search-and-seizure cases are significantly correlated with facts such as the place of the seizure, the extent of the search, and the presence or absence of a warrant, and that judicial votes on civil-liberties and economic issues can to a large degree be linked to judicial attributes such as age, educational and professional background, and partisanship (Segal 1986, 942; Tate 1981, 355, 359–63). But we might learn more about the crucial factors in judicial politics by also asking if there are established police practices, or inherited values, that lead justices to think searches in certain places are more problematic; or if we identify the content and sources of the typical experiences influencing judicial attitudes that attributes like educational and professional background signal; or if we study the institutional constraints on the sorts of justices that are likely to be sitting on the bench at a given time. We might also wish to consider whether judicial voting patterns affect the types of searches police conduct; whether prevailing popular notions about the privacy of certain locations do so; whether judicial decisions on various economic and civil-liberties issues assist or alter the institutions that shape and select justices with certain attitudes; and other questions of this order.

Obviously, such issues are so complex as to be largely beyond the scope of any rigorous, essay-length study of judicial decision making. But the "new institutionalist" argument is that we must not therefore displace them from the discipline's research agenda, declining even to suggest how narrower findings might have *implications* for these broader questions, not to speak of pursuing the latter directly. When limited inquiries dominate the field of research, too many of the decisive elements in politics are left unexplored. Yet in political life, many economic currents and even political actors' own purposeful commitments are affected by relatively enduring legacies of past political choices. If these elements are left permanently at the margins of analysis, the result can be a restricted, atemporal view of politics that says little about the factors it holds itself to be most potent in political life. Political action, such as judicial decision making, then inevitably seems a tedious, crassly self-interested, and rather ineffectual game among programmed players.

Adherents of rational-choice perspectives might reply that this "new institutionalism" stresses the importance of background structures and apparently inefficient historical processes *over* the intelligible free choices of identifiable political actors, thereby raising the specters of determinism, blind chaos, or both. Yet ironically, the writers who have followed the second road to the new institutionalism have done so precisely because they believe it finds more room for meaningful political decisions than the deterministic outlooks with which they began. Before further describing and assessing the implications of the new institutionalism for public law, let us trace this second road.

The Road from Historical and Sociological Determinisms

The contention that interest-group theories of politics and later rational-choice models all neglect how resources and values arise historically is, of course, not new. Numerous schools of thought endorse these accusations. The perspectives that have most influenced political science are probably Marxist historical analysis and the non-Marxist "structural-functionalist" sociology elaborated by the U.S. students of Max Weber, Talcott Parsons, and Robert Merton, as adapted by leading political scientists, notably David Easton and Gabriel Almond. Common to both these schools is an effort to go beyond the surface of individual and group conduct to identification and analysis of the deeper social and economic forces that—interest-group analysts acknowledge—seem to drive human political life. Marxist and non-Marxist sociologies identify these forces differently, however.

In its most bare-bones versions, Marxist historicism provides the famous argument that relationships to the means of production are the root determinants of the prevailing modes of social and political organization and the processes of conflict and change they display. Political actors can be placed into classes according to where they stand in relation to the dominant productive forms, and their conduct (and that of the institutions they create to further their interests) will ultimately be explicable in terms of the economically based imperatives facing those thus situated (Tucker 1978, 473–83, 487).

In contrast, Parsonian "systems analysis" or "structural-functionalism" refuses to give such utter preeminence to economic relations. It argues instead that human life should be analyzed in terms of the multiple, interlocking social, cultural, and psychic "systems" that organize activities in ways that prove to be more or less enduring. On this view, all systems with any prospect of survival share certain formal characteristics and behaviors, various functional necessities and modes of adaptation to meet those necessities. Among these are certainly functions of resource production but also functions of socialization (or "pattern maintenance"), coordination (or "integration"), and "goal attainment" more generally. While studies of individual and group behavior can be included within such a framework, these types of systemic structures and functional needs become the focal points of sociological analysis. Within political science, David Easton (1953) influentially modified Parsonian systems theory by laying greater stress on the distinctive importance of the "political system" and on "input-output" analysis, while Gabriel Almond (1960) emphasized the adaptive features of systems in order to elaborate a less static, more developmental version of structural-functionalism. But all these writers shared a concern to provide theories that would explain behavior in terms of the needs of deeper social structures without deriving all behavior from the prevailing mechanisms of economic production (Barbrook 1975, 40–67; Parsons 1951; Skocpol 1984, 2–4).

Despite that difference, recent scholars with historical, sociological, and often Marxist sympathies have rejected both these older Marxist and non-Marxist alternatives to individual and group behavioral analysis. Again their criticisms are many, but the basic objection here is that these positions go too far, treating history, politics, ideologies, and institutional behavior as *entirely* dictated by economic or systemic necessities. Instead, many contemporary writers, like March and Olsen, stress the "relative autonomy" of at least some aspects of political action from such deeper structures, and they perceive many fairly long-lived social systems and processes of historical change as tragically inefficient, nonfunctional, indeed oppressive.

Different scholars make these points in different ways and to different degrees. Within political sociology and political science, Theda Skocpol (1979) and Stephen Skowronek (1982) have provided influential analyses of how *state* institutions have played a relatively independent role in shaping political development during the French, Russian, and Chinese revolutions in Skocpol's account and during the more peaceful and piecemeal institutional adaptation to industrialization in the United States described by Skowronek. Inspired in part by Antonio Gramsci, many historically minded members of the Conference on Critical Legal Studies have instead emphasized how legal ideologies, like political ideologies conceived more broadly, are "relatively autonomous" from economic and systemic "necessities." Hence, legal doctrines merit careful study. In the words of Mark Tushnet (1981, 30–31), they form "groupings of ideas connected by repeated association," which may be burdened by "internal tensions." Those tensions can generate some processes of adjustment and change that are largely independent of outside forces. Any adjustments made may in turn shape the choices and behavior of, at least, legal actors within the political system. People, it is assumed, are psychically driven to "interpret the material conditions of their existence in ways that make their experience coherent," and they may at times seek to alter those conditions in order to make coherent interpretations more possible.

Critical legal scholars disagree on just how "autonomous" legal ideologies are. Tushnet (1981, 30) remains close to traditional Marxist perspectives by arguing that "we can still expect the law to embrace positions that are required by the interests of the ruling class as a whole, even if they are inconsistent with the interests of individual members of that class. The law remains linked to the relations of production directly through the political perception of advanced segments of the ruling class and indirectly through the political principles that are ultimately rooted in those relations."

But Robert Gordon (1984, 101) breaks more fully with Marxism, as indicated by his influential essay, "Critical Legal Histories," which attacks both Marxist and Parsonian forms of "evolutionary functionalism" in legal history precisely as contemporary political scientists and sociologists attack its counterparts in their disciplines. Gordon insists that "causal relations between changes in legal and social forms" are "radically underdetermined." Legal forms and practices therefore cannot be adequately grasped as "objective" responses to "objective" historical processes, not even evolving relations of production. Law is indeed a "product of political conflict," but it is not simply a mirror or reflex of that conflict. Again, legal ideologies are "relatively autonomous" structures with their own peculiar internal character, so that they sometimes act as "independent variables" that transcend and actually help "shape the content of the immediate self-interest of social groups." Judges may decide in part out of concern to mitigate internal tensions in legal doctrines; parties to a case may be influenced by what the law suggests their legitimate claims are. Partly as a result, there can be no confident expectation that the decisions of legal actors or institutions will always be "functional" for their material interests, or even for their own survival: the quest for ideological consistency can lead to behavior that is counterproductive by these standards. Thus these legal and sociological critics of functionalist accounts arrive, like March and Olsen, at a belief in the importance of various relatively enduring political and intellectual institutions in human life beyond economic relations and social "systems"; but they do so as much to break free of rigid historical determinisms as to identify overlooked constraints on political choices.

The Lessons for Public Law: An Appraisal of the "New Institutionalism"

Strengths

While not every protest against dominant perspectives deserves to be sustained, it is not likely that many scholars would feel obliged to move in the same direction unless there were something to their feelings. I believe the basic critiques advanced by those on each road to the new institutionalism are correct, whether or not they really point toward that destination. Those reacting to individual and group behavioralism and rational-choice theory are right to insist that analyses of politics should explore how relatively enduring structures of human conduct have shaped the existing array of resources, rules, and values, instead of simply taking that array as given.

As just suggested, this point is strikingly supported by the study of public law—for controversial, politically charged judicial decisions in the past may later determine the types of litigants that can get *into* court at all, as well as the very types of claims or rights persons believe they are entitled to assert, morally as well as legally (Orren 1976; Gordon 1984, 109–13). Obviously, no group is likely greatly to influence an institution that will not attend to its voice. And many groups' sense of their nature and purposes may be significantly affected by how far the legal system legitimates, for example, the permissibility of religious dissent, or of unregulated production and exchange. Thus, accounts of self-interested rational calculations and the behavioral regularities they are thought to generate will have limited explanatory power if they are not sensitive to how the legacies of past decisions lead people to think their interests should be so defined. Neglect of these factors may also prevent us from seeing how social definitions of interests appear much less rational, and much more vulnerable to alteration over time, when their origins are identified.

Those reacting against deterministic historical sociologies also have a strong, if less decisive, case. As even admirers of Marx and Parsons have felt compelled to conclude, the history of human societies simply cannot be adequately captured by the paradigms of economic or functional necessity alone. Again, for anyone immersed in public-law materials it seems all too evident that judicial decision makers often have a significant range of choice on how to advance the interests and principles they perceive as imperative. And while they always hope to reach results that will be beneficial at least for some, they often fail to decide in ways that serve the interests of the ruling class, of economic development, or of systemic adaptation. It is difficult to see much that is functional in Roger Taney's *Dred Scott* opinion, for example; and while it tried rather ineptly to assist the southern white ruling class, it was hardly cheering news for U.S. capitalists (Dred Scott v. Sandford, 19 Howard 393 [1857]; Fehrenbacher 1978, 340–414, 551–61). Many modern decisions, like the courts' much decried insistence on stopping a nearly completed dam to save the snail darter in *Tennessee Valley Authority v. Hill* (437 U.S. 153 [1978]), again obviously please certain groups, but they do not so clearly further either dominant economic interests or systemic efficiency (Dworkin 1986, 20–23).

If neither accounts of rationally self-interested behavior nor the leading deterministic sociologies seem adequate to describe political life, it indeed seems wise to identify other relatively lasting structures or patterns of behavior, institutions of various kinds, that shape and constrain political choices and conflicts. The alternative would be to fall into a simple recitation of events, an approach favored by some journalists and historical purists, but one that cannot shed much light on deeper regularities or causes in human affairs. Various political scientists and sociologists have noted that "new institutionalist" approaches instead resemble the sort of history advocated by Fernand Braudel (1980) in his influential chapter, "History and the Social Sciences: The *longue durée*" (Skocpol 1984, xii, 394; Mayhew 1986, 9). Braudel called for students of human affairs to turn from their concentration on the immediate, surface politics of kings, generals, and dramatic events to elements that operate over longer time spans, to middle- and long-term patterns and "structures" that condition those actions. As examples of this sort of history and social science, studies of the *longue durée*, Braudel (1980, 31) mentioned "geographical frameworks, certain biological realities, certain limits of productivity," and even "spiritual constraints: mental frameworks too can form prisons of the *longue durée*." This list, it should be stressed, defines the historian's task as significantly broader than that posed by new institutionalists in political science and sociology. They typically focus on humanly created structures—including "normative orders"—so they consider only a portion of Braudel's "middle-term" factors (March and Olsen 1984, 243–44). Even so, the methodology Braudel describes is indeed parallel.

Problems

But the similarity of the "new institutionalism" to Braudel's history suggests two basic difficulties the approach faces, at least as a model for public-law scholarship. The first problem is one sometimes held to characterize the work of many followers of Braudel, and it returns us to a point raised by the two contrasting roads to the new institutionalism: the question of whether these analyses *increase* or *decrease* our sense of the discretionary nature and the significance of political action. Histories focused on the *longue* *durée* are sometimes said to treat the individual conduct featured in traditional political and legal histories as unimportant—once again, epiphenomenal. Perhaps that is the truth about such conduct; but if it is, then while the new institutionalism would restore the study of a wider range of specifically political structures, it would still downgrade the significance of the choices of human political actors. We would, for example, place little stress on the existence and importance of genuine judicial discretion in decision making—the phenomenon that is traditionally at the heart of public-law analyses, whatever their methodological differences. Instead, we would tend to speak simply of "institutions" and "structures" acting in the legal world, in a way that many find far too full of reification and anthropomorphism to be plausible (Easton 1981, 316).

While some "new institutionalist" writings do seem to neglect the significance of individual choice (particularly in favor of "the state"), there is no need for the approach to be taken so far. The new institutionalism requires us only to stress how background structures *shape* values and interests, not to speak as if they have interests of their own. Most of our experience certainly suggests that human actions such as judicial decisions are indeed influenced by a great range of structural contexts—by the actor's position within state agencies or political parties, by economic relations, by ideological outlooks, by enduring ethnic alliances, and so on. But the result is often that actors are faced with so many conflicting imperatives that they retain significant room for choice, even if many of their alternatives are fairly grim.

I think in fact that if the study of politics, including the politics of law, is to seem worth pursuing as a distinctive discipline, it must begin by assuming that what have traditionally been thought of as political actions *can* be independently important forces in reshaping the world—though of course its investigations must bear this assumption out. As Riker has argued, in searching for patterns and regularities, we certainly must attend to "structural and cultural constraints" that act as "unstable constants" to help us make political actions somewhat more comprehensible and even predictable. But our accounts should at a minimum leave open the possibility that the outcomes of decisions, which may significantly alter background structures and constraints, are in part traceable to the creative political skill, judgment, and artistry of the actors involved, as well as to the forces that have shaped them and the situations they confront (Riker 1980, 445).

As suggested above, Riker's rational-choice approach falters in large part because it does not take this last point far enough: it fails to recognize that the very values of political actors may be altered by deliberate reflection and choice, as well as by the mutable political institutions that shape the formation of values and beliefs more broadly. While such alterations may be rare, they should not therefore be dismissed as trivial "outliers," for they may be among the most decisive of political events. In this regard, it is useful to note one feature of the legal perspectives that have been most skeptical about the power of past legal doctrines to determine decisions, the legal realism of the 1930s and the critical legal scholarship of today. When they proceed to describe judicial decision making in ways free of "legalistic" biases, such writers end up attributing to "strong" judges significant creative capacities, both to alter the messages conveyed by legal institutions and to transform their own inherited beliefs over time. These "strong" judges are often credited with pivotal, lasting changes in the direction of the law's evolution (Carter 1985; Llewellyn 1960, 70–74).

That point brings us, however, to the second and more basic problem of these recent approaches. If we emphasize the "relative autonomy" of political actions from any particular structural determinant or array of determinants and if we also assume that political actions can play a significant role in altering many of these structures, or "unstable constants," it follows that most of these background structures should generally be treated as not wholly reducible to others. Apart from any other factor, the intervention of unpredictable human acts will probably lead each institution that is vulnerable to such acts to develop distinctively. Thus each will display some "relative autonomy" of its own: state agencies will be influenced by, but still relatively independent of, economic forces, and vice versa; political and religious ideologies will bear a similar relation to legal institutions; family structures will be similarly related to such belief systems, and so on. In short, as we travel these roads, "relative autonomy" soon becomes ubiquitous. Our picture of politics is expanded to include the whole man-made world, filled with multiple structures and political actions, all mutually influential, but none simply expressions of any others.

And that picture, of course, may seem so hopelessly complex and foggy as to be impossible to bring into focus. Everything is somehow connected to everything else, but we seem to have little purchase on what structures are more or less important and to what degree. Such an account may be better than very particularistic narrative histories that make no effort to discern enduring structures, patterns, and behavioral regularities at all; but it seems incapable of approaching the rigor and precision to which political scientists aspire. In public law, for example, we might have only slightly modified accounts of how enduring "structures" of interest groups place recurring demands on courts, of how dominant coalitions and realignments affect judicial behavior, along with qualitative probings of legal ideology and major decisions and quantitative studies of how judicial attributes correlate with results. But it is not clear how we would tell which accounts were more decisive or whether the others really mattered at all. In that case the "new institutionalism" would simply provide a convenient rationale for continued eclecticism, much as some say "political jurisprudence" turned out to be.

Suggestions

That outcome may be unavoidable. But the reasons for moving in this direction appear compelling, so perhaps we should try to see how these difficulties might be overcome. It is premature to advance settled answers, but some observations are possible. The fundamental methodological question is how students of politics, and in particular of public law, can give greater specificity to the precept that the "interplay of meaningful acts and structural contexts" should be central to their analyses. Four steps, relatively obvious but rarely fully implemented, suggest themselves.

Plainly, the first step would be for public-law scholars of various stripes explicitly to conceive of their "independent variables" as relatively enduring structures of the sort described here. Analysts oriented toward the study of interest groups might, for example, wish to study an area like labor law by exploring the impact of recurring *relations*—or patterns of conflict—between certain employer and employee groups. Students of legal ideology might instead take certain ongoing elements of "liberal legal" thought on property rights as their starting points for analyzing those same labor decisions. While each set of scholars would be building on existing approaches, significant modifications would be involved. Interest-group analysts would, for example, focus on discovering empirically ascertainable and important historical regularities in group interaction rather than simply identifying instances of patterns predicted by formal theory. Students of legal ideas would have to present more explicit models of the key mental or rhetorical structures they wished to explore than is often the case.

Secondly, analysts would generally be expected to provide some indication of the origins of the structures or institutions they examine, with particular sensitivity to how those structures may have arisen from past, controversial political choices. Obviously, a full explanation of a political institution's genesis would normally require a separate study. But if scholars provide at least some indication of the sources of the structures they deal with, both they and their readers will be less likely to view those structures, and the types of political life they shape, as patterns of behavior writ into the nature of things. They are thus more likely to be attentive to possibilities omitted in the prevailing order of political life, and to developments within it that may alter the institutions that have helped give rise to it over time.

Third, to execute this approach adequately, all analysts (and particularly those engaged in qualitative studies of patterns in thought and argumentation) should identify as fully as possible their dependent variables, the set of "meaningful acts," such as judicial decisions, they claim to help explain. It is true that qualitative analyses face some special burdens in this regard. If one is trying to ascertain the presence and saliency of not just certain catchphrases or broad normative dispositions but specific types of reasoning about, for example, property rights, then it is hard to codify those patterns into a data set suitable for statistical manipulation. A qualitative, interpretive narrative that shows the structures of thought and argument to be visible in texts is required. The extensiveness of such narratives alone can incline a writer to focus on a few major cases that seem representative or determinative of most judicial actions in a given area, instead of documenting how those structures are visible in all or most of the relevant cases (see, e.g., McCloskey 1960).

That sort of narrative can be conducted in compelling fashion, and so it may be a reasonable methodological choice. Yet if qualitative analysts define certain patterns of thought and postulate their likely influence with much rigor at all, it does not seem too much to ask them at least to indicate the relevant universe of cases and decisions where such patterns are held to be visible, and the portion of those cases in which they believe their claims are borne out. And if a scholar chooses to explore only a few leading cases in depth, it is reasonable to require some justification for granting those cases that leading status.

On the other hand, the approach described here would urge both quantitative and qualitative analysts to consider not only how far actual decisions conform to the results their structural contexts lead them to expect but also how those structures may have led decision makers like judges to perceive their situations and opportunities in ways that seem quite wrongheaded from other perspectives. In this regard, qualitative work on "legal consciousness" has generally been much more probing than quantitative studies (e.g., Klare 1978). Both qualitative and quantitative scholars can be criticized, moreover, for stressing only how their independent variables, here described as enduring structures of behavior or institutions, explain much of the variance of actual decisions. As I have argued, an adequate account must also acknowledge that some decisions appear to reflect judicial creativity, in whole or in part, in ways that may be comprehensible in light of broader factors but that are not reducible to them.

Finally, public-law studies of the interrelations between legal choices and the "background" institutions that shape them should at least raise questions about how those choices have in turn affected such institutions, intentionally and unintentionally. Again, no manageable analysis can be expected to explore these ramifications in depth; but they must at least be suggested if we are to approach the broader political significance of the decisions analyzed. We must not only try to explain *Dred Scott;* we must also consider what developments *Dred Scott* helps to explain.

These points describe the basic alterations in existing forms of research that would make them expressive of a common focus on the dialectical interplay of meaningful decisions and structural constraints. This approach might succeed in making judicial choices explicable in terms of relatively constant structural factors, avoiding the errors of assuming that all can be explained in terms of individual or group calculations. At the same time, it would preserve the possibility that the actions themselves may not prove epiphenomenal to any combination of background factors, and that they may have unexpected significance for later events. Those features would save the approach from the pitfalls of both historical determinism and the undue reification of "structures."

Once analyses were thus recast, moreover, I think public-law scholars would find themselves in a position to seek further kinds of interconnections, both between different modes of descriptive work and between empirical and normative concerns. The quest for such interconnections is vital to the larger descriptive enterprise of the subfield, for clearly no scholar can hope to deal with more than a fraction of the possible structures influencing the legal actions he or she explores. But if many analysts first cast their work as explorations of the interactions between diverse specified structures and sets of decisions, they could relate them more readily to parallel studies in the field. Indeed, an individual scholar might wish to explore how far two disparate structures—such as, perhaps, the Jeffersonian political coalition dominant in the executive and legislative branches, compared with persisting laissez-faire economic doctrines—influenced judicial reasoning on the commerce clause in the first quarter of the nineteenth century. The analyst might generate different predictive hypotheses based on these different structures and then determine which ones were best borne out, while attending in conclusion to the consequences of the decisions for existing political coalitions and economic ideologies. Students in the field could then gain some sense of the comparative explanatory power of these different structural contexts, as well as the ways actual decisions transformed them.

Of course, there will often be significant interactions in the impact of different background structures. In the case of quantifiable factors, contemporary techniques of interactive computing in multiple regression analyses are increasingly able to model some of these relationships as well as the relative impact of different variables. Here, however, is where the differences between quantitative and qualitative analyses become most troubling. If we have, for example, a model of Supreme Court decision making based on the place of the litigants in prevailing economic or political power structures that proves to correlate reasonably well with the actual pattern of decisions in voting-rights cases, how can we connect it precisely to a less quantifiable account of prevailing conceptions of representation and their problems that claims to capture the logical tensions and patterns of argument in those cases? No perfect fusion of these modes of analysis can be expected, but some points of contact are possible.

The two types of models might to some degree be properly viewed as complementary, shedding light on different aspects of behavior, one on the causal determinants of the vote, the other on the patterns of discourse used to rationalize the vote. But if legal discourse truly displays some "relative autonomy," then it will at times play more than this justificatory role. By identifying cases where conceptual analysis would suggest different voting behavior than the litigant model, we could ascertain whether this "relative autonomy" is indeed manifested. And by seeing which model accounts for the most decisions in cases where their predictions conflict, we could still get at least a rough ordinal sense of the relative explanatory power of the two analyses. If the conceptual account appears to have some independent validity, moreover, we might be able to get some indication of how it interacts with other factors by seeing if its results correlate better with some quantitative models than others.

It is true that because of the differences between qualitative and quantitative analyses, these comparisons and correlations will probably remain rough at best. We will be able to say, for example, that more cases are explained by a "realignment" explanation than by a particular conceptual model or that the latter correlates more clearly with a judicial "party origins" analysis than one stressing "socioeconomic origins." But precise cardinal estimations of just how much variance each model explains, by itself or in interaction with others, are not likely to be convincing. Even relatively crude estimates, however, might enable us to address some important questions that are not specifically on many current research agendas.

In particular, it might be possible over time to build up a fairly rich body of theory concerning *when* certain sorts of structural contexts are likely to be more or less relevant, drawing implications for broader patterns of institutional evolution. For example, economically dominated decision making of the sort that preoccupies Judge Posner's "law and economics" school might prove to capture regularities of choice well in certain contexts (Posner 1977). But the processes leading to the prevalence of those economic concerns—and, indeed, efficacious economic decisions themselves—may eventually alter the situation in certain fairly predictable ways. Narrowly economic reasoning might prove to be most visible when utilitarian patterns of thought are prominent and economic crises evident, as in, perhaps, the 1890s, the 1930s, or the 1970s. Once decision makers respond to those pressing concerns, however, less directly economic constituencies, claims, and structures of thought may come to the fore. If so, then wealth-maximizing models of decision making would properly find their place within a broader theory that explains when such patterns of reasoning, as opposed to other enduring patterns of behavior, are likely to be most salient. As March and Olsen note (1984, 742), it is such broader theory that the "new institutionalism" thus far lacks.

And significant theoretical progress along these lines may, of course, prove unattainable. But in any case, if these directions are pursued, we would not regard scholars who try to see how far ideological structures shape judicial decisions and scholars who analyze the impact of party realignments or appointive processes as engaged in sharply opposed enterprises. That sense of a common endeavor might make qualitative scholars more aware of the need to connect their claims with measured patterns in actual decision making, and it might also promote quantitative studies more sensitive to the complex conceptual structures and characteristics of political beliefs.

Furthermore, one even more significant consequence might result. The agenda of empirical investigations might become better tailored to enrich the normative debates in the field, and those debates might be more attentive to the problems of the empirical generalizations they rely upon tacitly. To see how, we must identify further the recent philosophic developments that suggest the relevance of "new institutionalist" descriptions of the legal system.

The New Institutionalism and Normative Debates

After a period in which rather abstract and hypothetically oriented moral and political theory was in vogue, many philosophers have recently been urging the adoption of more concrete, particularistic, and historically infused modes of moral and political argument. Writers who describe themselves as subscribing to quite different substantive outlooks, including neo-Aristotelianism, Enlightenment liberalism, and democratic socialism, have agreed with Alasdaire MacIntyre (1981, 119) that "morality is always to some degree tied to the socially local and particular." Therefore, as Richard Rorty (1986, 13) argues, following John Dewey, moral philosophy is properly concerned with the "precious values embedded in social traditions," and with the "conflict of inherited institutions with incompatible contemporary tendencies." Consequently, we are now frequently urged to begin not with states of nature, past, future, or hypothetical, but "with the values we hold and the political world we inhabit," examining the record of the diverse inherited values in our past and present social existences in order to deliberate on what prospective courses seem more or less promising (Herzog 1985, 225).

This recent turn (or return) in moral and political philosophy, which may be called a move away from "ideal theory" to more empirically oriented "pragmatist" approaches, seems to me largely sound. It does raise legitimate fears that we will be too wedded to past values or else inclined to doubt our values if we see them as merely our own. Pragmatism must indeed prove unsatisfactory if we use it to conceal from ourselves the necessity to criticize the current array of normative dispositions and to make reasoned choices among them. Those choices may have to be defended in turn by appeals to what seem to us to be more lasting aspects of our condition, in the manner of natural-law theorists.

Yet it simply seems true that our moral outlooks are largely the products of past traditions, whether we are conscious of that fact or not. So we are more likely to be liberated from the inadequacies of our inherited principles if we recognize their historical roots and attend carefully to the role—great or small—they have played politically, attempting to judge their characteristic tendencies, strengths, and weaknesses in the crucible of social life. Furthermore, recognition that our values have been carried to us via particular traditions does not preclude faith in their transcendental validity, much less their propriety for us. And if it makes us somewhat more dubious about making universalistic claims, generally that may be to the good.

It is fair to say, however, that so far the call to begin with the empirical and historical realities of our moral traditions remains more a program than an achievement. While Rorty, MacIntyre, Walzer and others have offered useful historical analyses of certain grand philosophic traditions, few of these writers have paid any detailed attention to the other varieties of political and moral discourse that have played such a prominent part in shaping our current societies and selves. Clearly, if the "new institutionalism" is understood in the manner proposed here, as encompassing among other things descriptions of the influence of enduring structures of legal ideas, it is well suited to help fill this gap. Research that identifies actual patterns in legal and political discourse and their consequences, testing their significance versus that of other structural contexts, should enable public-law scholars to argue more powerfully about the values U.S. law has really embodied historically, about the ways those values have shaped, and been shaped by, political conflicts, and about the results they have furthered or forestalled.

Those studies would be of great relevance for many contemporary normative debates, such as, for example, whether more "republican" or "liberal" conceptions of civic life have been dominant in U.S. public acts in particular eras, and whether "republican" conceptions have most assisted democratic reforms or reactionary localism (see, e.g., Bellah et al. 1985). Often, I fear, theorists would have to acknowledge that the historic traditions they favor, whether "republican" or "liberal" or both, have often been relatively ineffective in furthering the ends they admire, and have even proven conducive to quite unattractive results. If so, many current normative arguments would be greatly altered by such increased historical awareness. And since I believe debates over the values that should guide the U.S. legal system in the future must always remain a prominent part of the public-law agenda, this contribution of "new institutionalist" studies of historical ideological structures is a vital one indeed.

At the same time, analysts engaged in more quantitative empirical research might benefit from an increased awareness of how their studies are relevant to ongoing normative controversies. When a quantitative scholar can see quite easily how prevalent moral disputes rest in part on empirical claims about the historical operations of the legal system, he or she is more likely to choose to research topics that speak sharply to those disputes. For example, much of the current discussion of "law and economics" takes place on an abstract normative level. Scholars ask, Is it desirable to think of the legal system's goals in this way? But there are attendant empirical questions. For instance, Is it adequately realistic, or historically plausible? To convince us that it is, Posner, among others, has advanced strong claims that much of nineteenth-century common law is explicable in terms of economic reasoning (Posner 1977, 13–14, 18; Posner 1981, 5, 106, 114). Those claims are susceptible to much fuller empirical and historical investigation than they have heretofore received from either the advocates or critics of this viewpoint. (Even when adherents of this school do not claim that economic reasoning was consciously employed, they generally assert that their models capture the "deep structure" of common-law decision making, a claim that is empirical on its face and that is in any case dependent on arguments about what actually was economically efficient in the historical periods in question.) The results of such investigations are likely to affect the credibility of such economic reasoning as a normative model for contemporary jurisprudence. More generally, a public-law subfield that explicitly took part of its empirical agenda from normative debates and that produced descriptions relevant to current normative claims would go far toward advancing the unity of descriptive and prescriptive concerns that some participants in the 1984 symposium found lacking.

All this may be too optimistic. At present we can be more confident of the critiques of current efforts than of the potential of the suggestions offered here. But I would insist that at a minimum, these recent trends mean first, that empirical investigators should agree that the impact of structures of ideas forms a part of their enterprise; second, that qualitative analysts of the history of legal ideas and normative advocates should recognize that their endeavors will seem too abstract if they do not study carefully the role that the values they appeal to have played in the actual judicial decisions that constitute so much of our legal traditions; and, I would hope, third, that both quantitative and qualitative public-law scholars might consider exploring further how we might realize this suggested recasting of their work in terms of a common focus on the interplay of specified structures and decisions. If the methods and conceptions of the "new institutionalism" can help promote such shared awareness among the diverse scholars at work in the field, these recent developments will indeed prove to have both importance and promise for students of public law, as well as for political scientists in general.

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