"The New Non-Science of Politics: On Turns to History in Political Science"

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I. Introduction. The canon of major writings on politics includes a considerable number that claim to offer a new science of politics, or a new science of man that encompasses politics. Aristotle, Hobbes, Hume, Publius, Comte, Bentham, Hegel, Marx, Spencer, Burgess, Bentley, Truman, Easton, and Riker are amongst the many who have claimed, more or less directly, that they are founding or helping to found a true political science for the first time; and the recent writers lean heavily on the term "science."[1] Yet very recently, some of us assigned the title "political scientist" have been turning or returning to activities that many political scientists, among others, regard as unscientific--to the study of institutions, usually in historical perspective, and to historical patterns and processes more broadly. Some excellent scholars believe this turn is a disaster. It has been termed a "grab bag of diverse, often conflicting approaches" that does not offer anything like a scientific theory (Chubb and Moe, 1990, p. 565).[2]

In this essay I will argue that the turn or return to institutions and history is a reasonable response to two linked sets of problems. First, the dominant pluralist and functionalist approaches

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[1] Thus William Riker in 1962, advocating rational choice theory as the basis of political analysis: "These traditional methods--i.e., history writing, the description of institutions, and legal analysis....can produce only wisdom and neither science nor knowledge. And while wisdom is certainly useful in the affairs of men, such a result is a failure to live up to the promise in the name political *science*" (p. viii).

[2] In this context, John Chubb and Terry Moe are defending the theoretical superiority of economistic rational choice models. A Marxist scholar, Paul Cammack, has argued similarly against Theda Skocpol's historical and institutional sociology. It leads, Cammack says, to abandonment "of any kind of coherent theoretical framework at all, in favour of middle-level enquiries into a multiplicity of issues in a multiplicity of settings in the hope that something will turn up. Studies pitched in the middle of nowhere are not likely to lead anywhere" (Cammack, 1989, p. 287). Here I suggest instead that studies pitched, not nowhere, but in a number of theoretical places all trying to account for common experiences, are more likely to be fruitful than staking all on one grand theory in advance.

to political science had major limitations that were exposed by political events of the 1960s and 70s. Second, a more fundamental tension has characterized the enterprise of political inquiry since its inception: the tension between convictions that politics, and human agency in politics, really matter, and desires to explain politics and political decisions, almost inevitably in terms of exogenous, impersonal forces that may then seem far more important than "mere" politics. All studies of human conduct confront such conflicting pulls, between trying to explain human agency and trying to preserve its intrinsic significance; but the problem is particularly acute for political scientists, who generally believe political decisions are especially influential over virtually all spheres of human life, but who are particularly prone to analyze those decisions in terms of "non-political" forces, such as economic imperatives, psychological drives, or the largely pre-reflective social beliefs and customs that constitute a "culture." The unresolved, perhaps unresolvable tension between depicting political decisions as architectonic "first causes" and explaining their causes has quite understandably perennially prompted political analysts to seek better approaches. I will argue that reflection on how we might deal with this central tension suggests not only why a turn to history makes sense but also how political scientists can do so most fruitfully.

I nonetheless agree in part with the critics of recent historical/institutionalist efforts. At their best, those efforts offer hypotheses and examine evidence for them in accordance with the traditional logic of scientific method. If that is sufficient to merit the term science, then they are indeed scientific. But it is true that they do not now offer, or hold out much hope of offering, any grand overarching *theory* to guide a new science of politics; and many believe that inquiry without such a guiding theory is not yet science.[3] It is surely true that the enterprise of seeking such a theory is worth continuing and improving. The critics are quite wrong, however, to believe that the study of politics should be confined to such efforts. To do so would almost certainly involve assuming some ultimately deterministic explanation is correct, thus eclipsing the question of the character and significance of human political agency. To be intellectually honest, political science should adopt methods and a self-understanding acknowledging that these fundamental questions are still very much open ones, unsettled by all past and present efforts to found a true political science.

For if it is not altogether wrongheaded, it is surely premature to treat any particular version of such science, or even the quest for a unifying scientific theory *per se*, as the one legitimate claimant

[3] As noted below, I would opt for a less restrictive definition of what is required for analysis to be "science." As the title of this paper indicates, however, I think we can be too obsessed with labels.

to the throne of "true political science." There is no such rightful heir apparent; our potential princes are all still frogs if they are not frauds. The most we can hope for at present is to achieve some shared sense of how we can pursue different sorts of approaches to the study of politics in ways that help us to compare them, roughly but usefully--especially in regard to the enduring question of how we can understand human political agency. As I have argued previously, I think the institutional/historical turn in political science does suggest how we might achieve this (Smith, 1988a). It suggests not a single grand theory but an inclusive general approach, a new "non-science of politics" if you will, that can promote some meaningful accumulation of knowledge via comparison of different explanations for the common phenomena that most concern students of politics: the political decisions that seemingly must shape our lives if human beings are able consciously to shape them at all.

II. The Fundamental Tension.[4] Let me begin with the more fundamental source of the recent turn in political studies. Virtually everyone who writes about politics that I know about has or once had at some level the conviction that the decisions made by persons in power, particularly in positions of governmental power, matter a great deal. Such decisions alter the lives of most of the rest of us for good or ill, but significantly. Aristotle believed those who legislated for a polis had great influence on the character and endurance of its shared life, so that politics was "the most sovereign and most comprehensive master science" (Ethics, 1094a). Hobbes was moved to write, at least in part, by the incessant civil "disorders" of his era, for which he held its political leaders responsible, at least in part (1971, p. 728). Even Marx, who stressed the determining character of economic relations in much of his work, granted politics important autonomy in his historical writings and even more in his praxis. The various masters of American political science, from Madison to Merriam to Dahl, all analyzed politics with a view to enhancing, not denying, the efficacy of democratic statesmanship and citizenship (Seidelman and Harpham, 1985).

There the tension arises. How do we enhance the efficacy of political decision-making, and (to be sure) also enhance the prestige and influence of teachers of politics, and (perhaps more deeply) also enhance our ability to cope with the curiosities and fears about major political actions that we all feel at times, sometimes quite acutely? One obvious answer is to seek for reliable, perhaps even

[4] This tension is not identical, although it is akin, to what Duncan Kennedy at one time termed the "fundamental contradiction" of human existence, our longings at once to find freedom in social forms and to be free of social forms. (Kennedy has since disavowed this characterization) (Kennedy, 1979, pp. 211-12; 1984, p. 15).

predictive explanations of how political matters regularly work, and how they can be expected to go if decision-makers do Z instead of A. Natural scientists seem, after all, to have learned much by examining the physical world in these ways. In political analysis, such explanations usually trace conduct back to other factors, like economic interests; personal, group, or institutional power or status ambitions; the functional imperatives of social or political systems; religious, ethnic, nationalistic, republican, socialist, or other ideological commitments; psychic longings based on repressed trauma or eros; or simply human nature. These factors can best do the explanatory work we want from them, moreover, if they are themselves relatively fixed and enduring, not subject to frequent fluctuation or political alteration, instead regularly exerting their shaping influence on the political landscape. I trust it is fairly uncontroversial to assert that such explanations are either explicitly provided or suggested in some or all of the writings of all of the authors listed above, indeed in virtually all major works on politics.

Yet without more, such explanations are all at least implicitly reductionistic and deterministic. They suggest political decisions are relatively predictable products of constellations of forces, be they economic concerns, systemic needs, ambitions, ideologies, or erotic egos, that could not really be different than they are. Thus the decisions come to seem, in themselves, not all that interesting or momentous. They are not causes but effects, not "first things" but "epiphenomena", dogs wagged by their truly telling tails.

Perhaps one day some such explanatory theory will be judged essentially true. But at present, for a host of reasons, reductionist-deterministic explanations do not satisfy. First, so far they mostly fail on their own terms. Most are difficult to express in well-specified, coherent theoretical models of measurable political behavior that can be tested in replicable experiments (Ricci, 1984, pp. 250-58). Aristotle's conception of a constant if complex human nature is, to say the least, hard to operationalize. Marxist accounts of class struggle find the modern kaleidoscopic array of relationships to the means of the production difficult to capture; group models of politics have trouble modelling the behavior of all those who belong to several groups, and even more in finding a place for those ascribed membership in "latent," unorganized groups. And even when they seem theoretically elegant, as in the best rational choice models, contemporary scientific analyses of politics fail to explain more than a narrow range of political behaviors: game theory cannot easily be extended to "games" involving more than two parties, for example (Tsebelis, 1990, p. 579) Neither have they displayed any great predictive capacity for major real-world political choices: the timing of the collapse of Soviet hegemony took most political scientists by surprise. Indeed, all past efforts at grand explanatory theories of politics and political history that make predictions at all, from Marx to recent American "modernization" theories, have gone wildly astray. No such theorists expected nationalism and religious fundamentalism to resurge in the contemporary world, to give but one instance of error.

The current proponents of historical/institutional approaches in political science have particularly, and appropriately, hammered on one descriptive failure of most prevailing brands of political science: they tend to take their independent variables, be they class, group interests and resources, ideology, etc., as fixed and exogenous to political choices (March and Olsen, 1984, pp. 735-38; 1989, p. 6; Wendt, 1987, p. 356; Smith, 1988a, pp. 94, 100). That treatment is convenient, perhaps even necessary, for elegant scientific explanations, but it just seems false. Such factors do shape political decisions, but political decisions also seem to shape them, often producing the most fundamental sorts of political transformations; and so all accounts that fail to capture those complex interactions are likely to fail in important ways to explain politics.

That point leads to the reasons the existing "sciences of politics" appear inadequate to persons outside the enterprise of scientific inquiry, who rely on perspectives borne of lived human experience. To most such persons political decisions, to tax and spend, launch wars or end them, redistribute crops or burn them, build roads, schools, hospitals, sometimes churches, or close them, all seem to matter very much, and they do not seem to be simply epiphenomenal to anything else. To persuade us otherwise, a heavy burden of proof must be met, quite appropriately. Nor do our own daily experiences of making choices seem reducible to single-factor or even multi-factor deterministic explanations. We feel that we, as mysteriously but meaningfully autonomous agents, have some independent causal role in shaping our choices. To tell us that we don't may be true, but it doesn't ring true to personal experience. Deep down, almost no one believes it. And in this court of appeal to personal experiences, which so often renders our governing judgments, it does not help the case for deterministic accounts that, logically, they give us no real basis to decide how we should act. They simply deny that we really can decide. Thus they can make the whole absorbing if often agonizing human condition of endlessly trying to shape our lives for the better seem a pointless farce. Since a conception of our condition as farcical threatens to foster nihilistic depression and moral irresponsibility, especially in politics, most people rightly refuse to grant it veracity very readily.

Yet if we insist firmly on the independent, autonomous importance of human choices and particularly political decisions, on their irreducibility to explanation by exogenous factors, on their role in reshaping all such factors, we are in danger of giving up the possibility of explaining political decisions in any rigorous way. The nature of this autonomous agency is and perhaps must be mysterious, requiring as it does that we see ourselves as socially and biologically constituted but somehow uncaused (and thus unexplained) causes. Hence political analyses stressing such agency seem quickly to be limited to recounting how a number of apparently important decisions came to be made and what followed, in the manner of a devoutly anti-theoretical narrative historian concerned to lay down events as they happened without any attempt to build more general explanations of politics out of those materials. The most such histories promise is that we may gain some prudence from immersion in past experience (though we may also be misled, like the architects of the Maginot Line). But they typically fail to capture our frequent perceptions of recurring patterns in political decisions that appear traceable to forces which transcend any particular sequence of events. And apart from possibly bestowing a rather ineffable prudence, political history told as one damn thing after another also fails to give us any very concrete, reliable sense of what we can do to understand and control our collective political lives better. So we may well feel compelled to shift back to explanatory theories that are, at least implicitly, reductionist and deterministic if they have clear content at all.

If this abstract account of the problem of deciding on how to conduct inquiry into politics is correct, then we should expect political analysts to oscillate between approaches closer to the character of deterministic, quantifiable theoretical models of causality in (some of) the natural sciences, and approaches that lean rather toward the tales of human agency found in narrative histories. At this point the reader will expect me to claim to perceive precisely this deep dynamic at work in the recent turn to history and institutions in political science, and I do not intend to disappoint. But I am not offering a single-factor causal theory; there have been other and more proximate causes at work, to which I now turn.

II. The Failures of Dominant Political Science Paradigms. A revealing aspect of the recent trends in political science has been intensified concern with the history of the profession itself. Histories, and debates over histories, of part or all of American political science and public administration turned from a trickle to a gushing stream in the 1980s.[5] These studies vary considerably, but one near-universal theme is the tale of how American political

[5] Cf. e.g. Crick, 1959; Somit and Tanenhaus, 1967, 1982; Kress, 1973; Karl, 1974; Blondel, 1981; Gunnell, 1983; Collini, Winch, and Burrow, 1983; Higgot, 1983; Ricci, 1984; Ward, 1984; Seidelman and Harpham, 1985, Seidelman, 1990; Natchez, 1985; Janos, 1986; Farr, 1988a, 1988b, 1990; Dryzek and Leonard, 1988, 1990.

science originated with aspirations to be both truly scientific and a servant of democracy, aspirations abetted by deep faith that these two enterprises went hand in hand. There were partial exceptions, of course: John Burgess' comparative, historical political science adjoined a sharply limited conception of democracy underpinned by Darwinian views of nationalistic and racial struggle (Burgess, 1890, pp. vi., 3-4; Somit and Tanenhaus, 1982, p. 28). Woodrow Wilson, a seminal figure of modern American political science, nonetheless denied that political studies could really amount to "science," precisely because he believed there was an ineradicable element of autonomous creativity in statesmanship that no science could capture. Wilson really differed only in emphasis from the outlook of many of his counterparts, however. Like them, he believed that scholars of politics could provide democratic citizens and statesmen with the knowledge about institutions, issues, and alternatives that was absolutely necessary if democracy was to be feasible (Wilson, 1911; Somit and Tanenhaus, 1982, p. 78; Seidelman and Harpham, 1985, p. 41).

Unfortunately, this faith in the joint destiny of science and democracy has proven hard to sustain. For in American political science, the more basic tension between asserting the importance of political agency and providing scientific explanations for it has especially been manifest in one way: as a tension between desires to affirm and assist meaningfully *democratic* self-governance and desires to develop truly scientific accounts of politics. The most influential scientific accounts have made democratic commitments appear naive by stressing the ignorance of voters and the apparently ineradicable power of economic, military, and professional elites, as well as structures and forces beyond conscious human control.

Recurringly, this tension has fostered an ironic arc to the career of leading political scientists, from Arthur Bentley and Charles Beard to Charles Merriam to Harold Lasswell to David Easton and Robert Dahl. Early in their careers, these scholars all criticized previous forms of political science for being naive about democracy, primitive and inadequate as science, and consequently incapable of contributing greatly to the conduct or reform of American democratic institutions. Each helped promote new efforts to create a true science of politics, explicitly or implicitly confident that it would help make more sophisticated forms of democracy possible. Subsequently, however, each became dissatisfied with the results and methods of their intellectual progeny, for those works often disparaged the feasibility of democracy, however construed, in professional jargon inaccessible to ordinary citizens. Thus, late in their careers, these leading figures all turned away from emphasizing the pursuit of a truly "scientific" political science, to stressing the vindication and advancement of a more truly democratic politics. Such later works frequently have been judged, in turn, naive and unscientific by younger proponents of yet another new science of politics.

This cyclical pattern has been significantly modified by a secular linear one. Whereas Beard and to a lesser degree Merriam came to denounce excessive "scientism" in politics, even the possibility of a political science altogether, the later figures have not issued such explicit denunciations, and the self-understanding of political scientists as members of a disinterested profession pursuing scientific knowledge for its own sake has become more entrenched (a phenomenon to which I will return). Furthermore, from the time of Lasswell onwards, the concerns to aid democratic politics of political scientists like David Truman, V. O. Key, Jr., David Easton and Robert Dahl in the 1950s increasingly came to center on the provisions of tools and insights to managerial "democratic" elites. To be sure, from the late 60s on Easton, Dahl and Charles Lindblom, amongst others, reinstantiated the older pattern of moving from an early emphasis on scientific analyses (of systems, group politics, and incrementalist decision-making respectively) to explicitly normative efforts to further democracy by criticizing the power of corporations and experts and promoting a better informed democratic citizenry. Generally, however, such figures have neither been quite so dismissive of political *science* as Beard came to be nor so optimistic about the real possibilities for democratic improvements as Merriam seems to have remained.[6]

William Riker's writings are representative of how the cyclical pattern of moving from an emphasis on science to democracy has been tempered by increased commitment overall to the study of politics as a scientific pursuit and the inevitability of elite predominance even in democratic politics. Since turning to social choice theory, Riker has never abandoned his confidence that it is the right candidate to guide a truly scientific politics. He has, however, tried to show that although traditional democratic ideals were unscientific and naive (particularly blind to cycling problems), an admittedly more elite-dominated version of democratic theory can be developed and defended (1962, 1982). He has also recently written a more popular work edifying citizens about the "heresthetic art" of democratic statesmanship, an art "free men use to control

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[6] The preceding three paragraphs are chiefly distilled from the overviews provided by Crick, 1959; Purcell, 1973; Somit and Tanenhaus, 1982; Ricci, 1984; Seidelman and Harpham, 1985; and Farr, 1988. Easton's noted critique of the profession, which still defends its scientific character, is his Presidential address (Easton, 1969). The later works of Dahl and Lindblom, pursuing more elaborately aspirations to inform citizens and combat obstacles to democracy that have been visible throughout their careers, include Dahl, 1985a, 1985b, 1989, and Lindblom, 1977, 1990.

their surroundings," which includes creative elements that the science of rational choice can explicate but not generate or predict (1986, pp. ix-x). Thus Riker has to some degree retraced the intellectual path of Beard and Merriam, but now with science and an elite theory of democracy more firmly in command. Many other contemporary political scientists are much more skeptical about both science, particularly in the form of rational choice theory, and democratic elitism, as we shall see. But Riker probably typifies the modern profession's predominant trajectory on this issues prior to the recent turns to history and institutions, and perhaps still today.

Here, I will not try to review the specific objections each successive "new scientist of politics" raised against his predecessors or the exact elements of the new version of science each offered. To keep things manageable, let me instead pick up the story with the approaches to politics that became dominant after World War II. These are the forms that recent scholars have tried to transcend.

Most historians of the discipline agree that the post-war era offered multiple incentives for political scientists to identify themselves exclusively as professional pursuers of objective scientific knowledge. The relative consensus on political ends and the desirability of liberal democratic political institutions during the cold war, leading to claims of "the end of ideology," made technical questions of means seem more salient than debates over purposes or first principles. Enhanced faith in science and technology stemming from their military contributions reinforced the already enormous prestige of "scientism." Government and foundations offered massive new support for scientific higher education and research. Thus the discipline was able to grow as never before by presenting itself more as a purely academic profession and more as a science (Purcell, 1973, pp. 237-240; Somit and Tanenhaus, 1982, pp. 145-47, 167-172, 183-94; Ricci, 1984, pp. 112-13, 126-27; Seidelman and Harpham, 1985, pp. 151-59).

The now-familiar label for the new science of politics of the 1950s was "behavioralism," but behavioralism meant many things. It usually indicated predilections for focusing on observable actions of political persons, rather than formal institutions or those persons' public rhetoric and self-understandings ("study what they do, not what they say"). It also favored questions of description and causal explanation rather than quests to define moral standards and normative ideals. But most commonly, behavioralism simply renewed calls for a scientific methodology of hypothesis formulation and empirical testing. The behavioralist "mood" was thus compatible with a range of outlooks on the primary phenomena of politics, and with no clear general outlook at all, only narrower hypotheses about specific aspects of political conduct.[7]

Many believed that if political science were truly to be a science, such scientific methods were not enough. The discipline needed an "operationalizable" guiding *theory* of what the primary factors in politics were and how they worked, from which more particular hypotheses could be derived. It needed, in short, what Kuhn would call a dominant "paradigm." It is generally agreed as well that two (connectible) would-be paradigms came to the fore as foundations for the new "behavioral" science of politics, the pluralist group theory of Truman and Dahl, harkening back to Bentley (and John Dewey), and the structural-functionalism of Easton and Gabriel Almond, derived from the sociology of Talcott Parsons and the anthropology of the 1920s.[8] These perspectives each stood in opposition, above all, to frameworks derived from Marx: in the bipolar cold war world, Marxian analysis overhung the discourse of American political scientists even though there were few highly prominent proponents (and perhaps no great grasp) of it in the discipline (particularly in McCarthy's 1950s).

The response of the new American political scientists was largely to show that contemporary politics did not meet Marxian descriptions. True, groups and systems were often driven by economic interests, as Madison well knew; but they had a range of other interests as well, so that relationships to the means of production were not all-determining. Moreover, modern American politics, at least, were too porous and multiply populated for any particular economic group or interest to prevail all the time. Generally, because of their contemporary focus, behavioral political scientists responded less directly to Marxian accounts of history. But when they sketched accounts of historic development, as in Dahl's review of New Haven's history or V.O. Key's theory of realigning elections, they tended to write as if group struggle was a timeless model for politics, with a gradual broadening of the groups involved combined with a tendency for religious and racial cleavages to give

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[7] Purcell, 1973, p. 240; Somit and Tanenhaus, 1982, pp. 173-83; Ricci, 1984, pp. 134-44; Seidelman and Harpham, 1985, p. 151-53. These works review the leading attempts to define "behavioralism."

[8] There were important differences between focusing on "groups" and "systems," but the two modes of analysis could be linked, as Almond did, by treating "interest articulation" as a systemic function performed by various sorts of interest groups (Almond and Coleman, 1960, p. 33; Almond and Powell, 1966, pp. 73-127). Much other influential work, such as the numerous analyses of voting behavior, could be read as identifying existing political groups and mapping their interests and electoral behavior, even when pluralist theory was not explicitly invoked.

way to "rational," compromisable socioeconomic interests over time. Almond's elaborations of structural-functional systems analysis into a model of economic and political development also served as an alternative theory of history, in which systemic adaptation and acquisition of some type of "modern," Western-style political and economic institutions replaced class struggle and the ultimate triumph of the proletariat. In so arguing, both pluralists and systems analysts seemed to regard their models of politics as applicable to virtually all times and places, with truncated historical or third world versions destined to flower into modern forms.[9]

There were always many critics of the new behavioral science of politics, advocates of a wide range of alternative modes of analysis and political perspectives, from surviving exponents of the older institutional-historical approaches to conservative advocates of classical natural right to leftist scholars influenced by the Frankfurt School. Scholars agree that the tumultuous politics of the late 1960s and 70s strengthened the voices and the credibility of these varied critics, particularly those decrying not only behavioral methods but the character and institutions of American political life. The race riots, the politics of poverty, the ferocious protests over Vietnam met by sometimes violent repression, the mushrooming of the "counter-culture," followed in the 70s by Watergate and the relative decline of the United States' global economic and political status after OPEC's embargo and the Vietnam withdrawal, all challenged prevailing political science portraits of American and world politics in numerous ways.

Insofar as they really existed, pluralist group politics did not seem so benign, since groups like blacks and the poor might be harshly excluded; nor did such politics seem so transparently likely to endure. The "state" seemed more real and significant than pluralists allowed, for it appeared at once capable of initiating redistribution and of stifling popular protest movements, and it could also falter internationally with dangerous consequences at home and abroad. Notions that western states might altruistically lead others toward liberal democratic "modernization" seemed naive. Politics, moreover, did not appear so fundamentally non-ideological, nor moral questions so obsolete. Thus in a conflict-ridden, often topsy-turvy era, political science could persuasively be accused of offering only static, complacent, ethnocentric models that did not

[9]The above two paragraphs rely chiefly on Key, 1955, 1958; Almond and Coleman, 1960; Almond and Powell, 1966, esp. pp. 34-451, 299-332; Dahl, 1961, esp. pp. 34-36, 59; Purcell, 1973, pp. 242, 260-66; Somit and Tanenhaus, 1982, pp. 188-89; Ricci, 1984, 124-25, 136, 149, 155-57, 169-71, 214-17, 282-83; Skocpol, 1984, pp. 2-3, 1985, pp. 4-5; Seidelman and Harpham, 1985, pp. 178-83, 193; McDonald, 1989, pp. 19-22.

simply fail to produce any behavioral "laws," or to predict, explain or provide effective social guidance concerning the startling events occurring. To an embarrassing extent, the political science literature failed even to discuss these topics.[10]

The support that events gave to critical perspectives in political science during these years ushered the discipline into what some have called the "post-behavioral" era, but that label can mislead. For one thing, much of political science continues much as it did before. Furthermore, the various protests swept together by the label have little in common beyond some enemies. Hence even though many of the critics emphasized the importance of history, as we shall see, they cannot plausibly be lumped together as part of a "turn to history." "Post-behavioralism" is best understood as describing a time period, not an intellectual school or approach (Somit and Tanenhaus, 1982, pp. 230-33; Ricci, 1984, pp. 188-90; Seidelman and Harpham, 1985, pp. 192-200).

In fact, in the 1970s, the leading professional response to the perceived failings of behavioral political science probably was increased interest in the leading new candidate to provide a true science of politics, rational (or social, or public) choice theory, in both of its main variants, models of preference maximization and aggregation and game theory. Since the mid-70s, virtually all the nation's leading departments have competed vigorously to recruit the leading scholars in rational choice. Their work has, to be sure, evolved in ways that are partially responsive to criticisms of earlier forms of behavioralism. Most notably, Riker and others have abandoned their earlier disparagement of institutional analysis to champion a rational choice form of "new institutionalism" or "neo-institutionalism." Pioneered by Kenneth Shepsle, these analyses involve efforts to model how institutional rules and structures affect the expression and aggregation of preferences, empowering some actors and at times resolving cycling problems. Riker and others have also shown how rational choice theory can illuminate the behavior of political actors, legislative bodies, and electoral systems in different historical periods (Riker, 1980, 1986; Cox, 1987; Shepsle and Weingast, 1987).

From the standpoint of non-rational choice proponents of a turn to history and to institutions, however, such modelling does not really succeed in capturing the most important things that behavioralism missed. It still assumes the substance of political actors' preferences are fixed by forces impervious to political transformation, and it evaluates alternative institutions only in

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[10] Easton, 1969; Purcell, 1973, p. 267; Somit and Tanenhaus, 1982, pp. 213-17; Ricci, 1984, pp. 175-78; Skocpol, 1984, pp. 3-4; 1985, p. 6-7; Seidelman and Harpham, 1985, pp. 188-98.

terms of their effects on actors' efforts to maximize such preferences. If there were any common denominators to the anti-behavioral protests, however, they probably were the complaints that in describing "mature" political reality as group competition within "modern" systems, political scientists had dismissed the possibility of meaningful alternatives to such systems and such a politics. Many critics insisted that insofar as the behavioralists' (and rational choice) versions of "reality" were true, they should instead be seen as recent products of major transformations in quite different past political worlds, and as potentially subject to major, possibly radical transformation in the future (or even the present). Such transformations might not only involve the reconstitution of basic institutions, but also of the basic values and indeed the very identities of political actors.

For some, these broader-ranging concerns called for a renewal of speculative political theory; but for many, they gave reasons for more genuine turns to history. As Sheldon Wolin argued at the height of the assault on behavioralism, the history of political regimes and of political theory was a superb storehouse of alternative political visions that might suggest diagnoses and new possibilities for the present. Furthermore, history might reveal the transforming forces that helped craft the politics of the present, forces that must be understood if the character and prospects of that politics were to be fully comprehended (Easton, 1969, p. 1058; Wolin, 1969, p. 1077; Ricci, 1984, pp. 278, 311-12).

Despite these protests, there was no overwhelming rush to the study of historical processes or the history of political ideas in the 1970s or 80s. The recent attention to the history of the discipline itself may be a sign that scholars are still searching to identify how and why political science has fallen short of its practitioners' aspirations. Nonetheless, some "turns to history" were made that have steadily grown more influential. In history of ideas, the investigations of Bernard Bailyn, Gordon Wood, and especially the sometime political scientist J.G.A. Pocock have seemed to a wide variety of scholars to have actually uncovered an alternative political vision that might be of some use in the present. Their accounts of "civic humanism" or "republicanism" have suggested that Americans are heirs to a neglected non-liberal legacy of more communitarian, virtuous, perhaps even participatory politics. Some political scientists have subsequently joined the historians' quest to shed light on modern politics by discovering how and how far republicanism became liberalism in America; others are engaging in speculation about how the desirable features of early republicanism can be recaptured today (Bailyn, 1967; Wood, 1972; Pocock, 1975; Lienesch, 1980; Hanson, 1985; Sandel, 1984; Sunstein, 1990).

Especially since the mid-1970s, political theorists and historians of political ideas have also offered the discipline another sort of "turn to history" with mounting influence: the "hermeneutical turn," inspired largely by "postmodernist" European critical theory. Those labels refer to an imposingly large and varied body of work by scholars like Hans-Georg Gadamer, Jürgen Habermas, Michel Foucalt, Paul Ricoeur and Jacques Derrida, expounded and extended in American political science by theorists including Charles Taylor, William Connolly and Michael Shapiro and reinforced by the qualified advocacy of leading historians of political ideas, notably Quentin Skinner (Skinner, 1969; Taylor, 1971; Connolly, 1974; Derrida, 1976; Foucalt, 1977; Habermas, 1979; Ricoeur, 1981; Shapiro, 1981; Gadamer, 1982).

Two themes visible in much of this literature are particularly pertinent here. First is an emphasis on the interpretive character of all human knowledge: far from ever discovering objective laws about politics or indeed any other facet of our world, postmodernist thinkers stress how all accounts are only controversial, partial interpretations stitched together largely from other such interpretations, and often unified only by the interpreter's quest to impose on phenomena a sense and meaning the interpeter values. Second, however, this apparent focus on "the interpreter" is undercut by an insistence that "the interpreter" and his senses of meaning are to a significant degree constituted by pre-existing languages, interpretations and traditions that have been both spawned in and formative of a wide range of social contexts. Thus to more radical postmodernists, all "interpeters" (that is to say, all human beings) may be understood as no more than particular congeries of interpretations emergent out of the complex pageant of preceding human history. In that unfolding drama, moreover, some perspectives have been forced into the wings, often brutally.

Hermeneutically-minded scholars do not concur on what all this implies for understanding human beings. Some stress how appreciation of interpretive contexts helps us grasp the intentionality of historical human agents more fully; others suggest the constitutive role of these contexts makes any such focus on "the subject" obsolete. Still, most agree that understanding humanity involves, at least in part, grasping more fully the traditions, contexts and languages that have constituted particular persons or groups. That task is pursued largely by taking the discourse and actions of such persons as "texts" and unfolding (or "deconstructing") their constitutive elements to approach, not a full grasp of the human world in which they emerged, but, in Gadamer's term, the "effective-historical" world they can project for us. Some postmodernist scholars rest with such deconstructive readings; many others enrich their interpretations via similar engagement with other "texts" comprising and comprised by the historical contexts of the scholars' primary interests. These sorts of historical inquiries may help scholars see, for example, the ways in which one language (of, say, professional psychology) has delegitimated others (such as forms of political, cultural and sexual radicalism). For all such postmodernists, the quest for a positive, causal social science gives way to, or is at most a transitional moment in, interpretive inquiries into historically-constituted "texts" that encompass human political action. Although still much more avant-garde than mainstream and usually more centered on theoretical expositions than concrete explorations, hermeneutical works are proliferating in the discipline and are increasingly concerned with substantive interpretations of, for example, American political culture (e.g. Hanson, 1985; Norton, 1986; cf. Ricci, 1984, pp. 275-88).

Until very recently, however, republican revisionism and hermeneutics were of interest almost exclusively to political theorists, not to empirically-minded political scientists. They were more impressed by perhaps the most influential assault on pluralist complacency, Theodore Lowi's End of Liberalism; and that work did include the contention that the pluralist politics of the 50s and 60s were far from timeless. Pluralism was instead the genuinely modern product of historical forces, particularly the rise of a (deplorable) new public philosophy of "interest group liberalism" embodied in a (corrupt) Second Republic of the United States. On the whole, Lowi's work remained quite contemporary, with only the most broadly sketched historical arguments, so it only raised the questions of how American politics had been changed over time and what forces might change them in the future. But its success helped renew the receptivity of the discipline to studies that explored how institutions (like the Presidency) had altered through history, in ways partly traceable to different historical conceptions of their purposes. Many of those studies have been more historical than Lowi's own work (Lowi, 1969, 1979, 1985; Ceaser, 1979; Tulis, 1987).[11]

For empirical political scientists, a "turn to history" probably appeared even more clearly in works that identified themselves in the first instance as turning to institutions, specifically to the importance of "the state." Calls to "bring the state back in," which quickly attracted numerous supporters and critics in the late 1970s and early 80s, were traceable to the protests of a few years before against the failure of prevailing behavioral accounts to comprehend what was then going on in the world. For example, Theda Skocpol's influential argument that state structures conditioned the character and fate of the world's major modern revolutions was sparked by a concern to understand the obstacles to revolutionary change facing movements in America and elsewhere during her student days.

[11] Much of my own work falls under this rubric (Smith, 1985; 1988b).

Those obstacles (in, e.g., South Africa) emphatically seemed to include the role of states. She and others arguing for the importance of state structures and institutions generally have, however, typically come to that perspective through extensive immersion in history; and their arguments about institutions usually have to be made via history, via discussion of fairly long periods of time (Skocpol, 1979, pp. xii-xiii, 1985; Skowronek, 1982).[12]

The reasons why the turn to institutions necessitates turning to history can be seen by considering the most ambitious effort so far to "theorize" a "new institutionalism" not limited to rational choice modelling, James G. March and Johan P. Olsen's Rediscovering Institutions (1989), an expansion of their earlier, exceptionally influential American Political Science Review article (March and Olsen, 1984). Acknowledging that the "new institutionalism" does not offer any general guiding theory as rational choice does, March and Olsen indicate that "new institutionalist" studies are driven by a number of linked intellectual dissatisfactions with the models of politics that have prevailed in social science in the post-war era, including both behavioralism and rational choice. First, those models did indeed treat politics as "epiphenomena," affected by, but not significantly affecting, elements like "class, geography, climate, ethnicity, language, culture, economic conditions, demography, technology, ideology, and religion." Second, extending that point, the older models treated the "preferences and powers" of political actors as "exogenous...depending on their positions in the social and economic system," and not alterable by political deliberations, decisions, institutions or processes. Third, but also an aspect of the same reductionism, behavioralist and rational choice theories have treated symbols, ideologies, political visions, all essentially as political "devices" wielded on behalf of political actors' socioeconomic self-interests, not as matters of intrinsic value and importance. And fourth, this persistent dismissal of the causal importance of political life has generally been made credible by assuming that economic and social systems are functionally "efficient." Less productive and stable modes are assumed to be weeded out over time one way or the other; so if we know what constitutes a more efficient economic or social system (e.g., liberal market systems), we can expect that socioeconomic (and thus political) systems will evolve toward such institutions in the long run, however politicians may choose in the short run (March and Olsen, 1989, pp. 3-4, 7-8, 48-52).

In place of these views, March and Olsen suggest that political actions and political institutions shape their environments even as

[12] Terrence McDonald has analyzed the influential urban politics literature by scholars including Amy Bridges, Ira Katznelson and Martin Shefter that similarly brings "the state" back in, in the form of the urban "machine" (McDonald, 1989).

they are shaped by them; that persons' resources and very preferences are often constituted by past political actions and institutions, which shape the meanings persons find in their lives; that these efforts to craft and preserve meaning are, as hermeneuticists often contend, as much moving forces in political behavior as economic or social and political status imperatives; and finally, that as a result, the actors and concerns that shape history are so multiple and complex that historical processes cannot be captured by models of evolutionary efficiency. Socioeconomically inefficient structures and behavior do occur and endure, for other sorts of reasons.

Those, at least, are propositions that "new institutionalists" want to defend. But to defend them, they must turn to history. They cannot otherwise show how past actions and institutions constitute the powers and preferences of agents in contemporary politics, nor can they discern any actual patterns of historical evolution that may exist even though sequential stages of efficient evolution do not. Thus while some "new institutionalist" studies focus primarily on the present, the agenda of the institutionalist turn means that on the whole, if political scientists accept these arguments, historical inquiries must proliferate.

We have now reached the multi-faceted recent turn to history in political science by way of its proximate causes. Let me indicate here why I believe these causes can legitimately be understood in terms of the deeper tension I described, as dissatisfactions with a political "science" that failed to capture how politics seems so much to matter. The "post-behavioral" critics gained force, against powerful professional incentives to be as disinterestedly scientific as possible, from widespread beliefs that contemporary political events were so important as to render a political science negligent of them indefensible. The quests for political alternatives these events triggered had as implicit premises beliefs that political actors can make meaningful choices of the sorts of political worlds they work for, and that their choices have some real chance to reshape the world. Although the growth of rational choice represented the continued appeal of a science of politics more than an affirmation of agency, it did at least portray actors as rational calculators and instrumental chosers, even if their ends appeared to be beyond their choosing. The renewed attentions to the history of political ideas, public philosophies and political culture, to administrative institutions and to "the state," all expressed similar senses that our beliefs about politics and our central governing institutions do make a difference. All of March and Olsen's arguments, moreover, clearly rest on the belief that politics matters, that socioeconomic reductionisms are inadequate, because politics reshapes other aspects of our social world, including our powers, preferences, and very senses of meaning, and hence affects all historical development.

The turn to history and to institutions in political science, then, does seem to manifest the basic tension between developing scientific explanations, which behavioralism ardently tried to do even if it failed, and vindicating our sense that politics matters, which behavioralism disputed, at least implicitly. Moreover, since many critics on the right and the left also identified themselves with the vindication of a more genuinely democratic politics than the American status quo of the late 60s and 70s, the critical assaults also manifest the more particular tension in American political science between commitments to science and democracy. The "science/democracy" formulation seems inadequate, however, since so much contemporary scholarship suggests that even if politics matters, meaningful democracy may not be attainable.

As that point implies, thus far the scholarship making these turns cannot boast of any great progress in resolving either of these profound tensions. In regard to scholars' more specifically democratic aspirations, rational choice theorists, again, have explicitly concluded that only a quite elitist version of "democracy" is possible. Thus Riker's more popular work edifies about democratic statesmanship, not citizenship. Many advocates of republicanism have instead hoped for a more truly democratic, participatory polity; yet their work suggests that if classical republicanism was ever present, it is irretrievably lost in the privatistic, commercial, inegalitarian modern world. Often postmodernist critical scholarship promises to democratize human affairs, by debunking the truth claims of dominant views and showing the insights in marginalized ones; but frequently the final lesson is that prevailing forms of political interpretation and culture are unalterably hegemonic. Lowi has repeatedly called for a more truly democratic "juridical democracy," and writers like Skocpol and Skowronek have clearly wished to probe ways stronger but more fully democratic states can be achieved. But their writings tend to imply that the dominance of self-seeking and unequal interest groups over American institutions cannot be broken, and that Americans are not likely to achieve a much stronger state unless it is much less democratic.

Furthermore, insofar as these works are explanatory, they tend implicitly to minimize human agency just as much as previous explanations did. Rational choice models limit us to questions of means, not ends. Ideologies, public philosophies, prevailing interpretations and languages either seem still puppets of socioeconomic forces, or they are themselves prisons of consciousness that control and limit political actors rather than constituting them as effective, autonomous agents. Structures, including "state structures" that come close to being reified as autonomous entities, severely constrain if they do not altogether displace "voluntarist" features of political life.[13] Sensitive to these problems, "new institutionalist" writers like March and Olsen and Skocpol have increasingly called for approaches that give due weight to both sides of the "dialectic of meaningful actions and structural determinants," but how one does that remains very problematic. The various types of "turns to history" I have canvassed offer quite different answers.

Rational choice analysts of historical developments not only reduce the realm of "meaningful actions" simply to *strategic* actions on behalf of politically unalterable preferences. They also take a narrow view of those preferences. Although in principle we might value anything, many motives are hard to define or measure precisely. Much of this scholarship avoids those problems in practice by assuming that we are motivated chiefly by wealth and power or status, apparently as a result of relatively unchangeable human biological and psychological structures. Taking such structures as givesn, rational choice theorists then illuminate only the rational calculations they dictate, without undermining their dictatorial status by exploring whether agents have some power to reshape those structures consciously over time.

All the other perspectives I have noted represent more genuine "turns to history" in that they do see the character and preferences of political agents as developed and changed through historical processes; but they conceive of those processes in significantly different ways. At the other end of the current intellectual spectrum from rational choice, hermeneutical scholars tend to portray agents as constituted primarily by historically evolving languages and traditions that offer standards of intrinsic meaningfulness and therefore give the conduct of agents a dimension not captured in models of instrumental rationality on behalf of economic or status interests. Scholars of "republicanism" and the rise of modern "interest group liberalism" also stress the role of changing public ideologies in shaping agency, action, and outcomes, at least implicitly. But they generally have greater faith that they can portray the interactions between traditions of public philosophy and "external" socioeconomic developments than do pure hermeneuticists, who see languages only as self-referential systems of signs. The "new institutionalists," in turn, are willing to encompass languages and ideologies as kinds of structures or institutions, but they usually place more emphasis on the determining role of political organizations as traditionally understood, especially state bureaucracies. Hence these approaches do suggest some ways meaningful actions can reshape structural contexts, but

[13] Skocpol's initial methodological manifesto explicitly called for "nonvoluntarist structural" perspectives, although she has subsequently stressed "the interplay of meaningful actions and structural contexts" (1979, p. 14; 1984, p.1).

they suggest different ways, and few analysts have compared these varied perspectives systematically. Such comparisons are not helped by the fact that none of these approaches is very clear about how we tell when or whether languages, ideologies, or structures are determining the constitution and actions of agents, how far they are instead subject to conscious alteration by such agents, or just what is meant by their "dialectical interaction."

These differences and ambiguities are reasons I think the critics are right to assert that the turn to history has produced no clear and compelling overarching theory of structure and agency that can guide empirical analysis (or normative arguments). That is why some believe recent developments have only exacerbated what they see as the increasingly worrisome state of the discipline. Gabriel Almond, among others, has characterized political science as a profession marked by increasing balkanization, with devotees of true social "science," especially rational choice, ever more greatly polarized from the assorted "historical approaches" reviewed here, as well as more traditional behavioralism. The historical approaches, moreover, are feared to sacrifice scientific rigor in favor of fuzzy, indeterminate, belle-lettristic "explanations," without providing anything that can better advance the case for human political agency, particularly via democratic politics. Hence we are now hearing the gloomy indictments of the "new institutionalist" turn to history cited at the outset (Skocpol, 1984, p. 4; Almond, 1988; Smith, 1988a, pp. 92-101; March and Olsen, 1989, pp. 16, 46).

III. A Possible Future for History. It seems to me that the "new institutionalism" has much more promise than its critics allow, although I hold out little hope for the emergence at last of a definitive new science or grand theory of politics. The leading contender at present, rational choice theory, obtains elegance and simplicity by advancing a narrow, unpersuasive view of political agency, and its implausibility is not compensated by any great predictive power. But neither do I think political science will flourish best if it continues as it on the whole does now: often we complacently answer doubts by invoking the desirability of a pluralistic discipline and suggesting that over time, the competition of ideas will automatically winnow out better from poorer accounts of politics, or else virtually all approaches will be seen to capture truths at different but compatible "levels," such as the scientific "level" of physical causality and the hermeneutical "level" of symbolic meaningfulness.

That complacency cannot be justified. A sort of competition surely occurs, but it is usually not one in which persons attentive to all forms of political analysis choose the types that prove most powerful The discipline is already so fragmented that its different schools of thought rarely compete directly, offering rival accounts of the same phenomena to the same audience. Instead, adherents of different outlooks write and judge largely amongst themselves. Their relative professional status is greatly influenced by the extent to which they find prestigious like-minded audiences in other corners of the intellectual world outside the discipline, like economics, philsophy, or literary theory. There is no guarantee that the most intellectually powerful forms of political science will find equally influential external audiences; nor does this form of competition encourage cross-fertilization among different approaches. Occasional suggestions that we embrace almost all approaches as illuminations of truth at different "levels," moreover, risk being fatuous. The various approaches are advancing opposed views of the character and driving forces of human agency. If they can nonetheless be shown to be somehow compatible, it cannot be done by fiat.

This survey does suggest, however, that the multiple flavors of political science do have certain things in common. Although those things are quite general, I believe the discipline can build upon them. As I have endeavored to show, virtually all political scientists have always shared the basic concerns I described at the outset. They generally begin with beliefs that politics matters greatly, but they differ over the character of human political agency, and most if not all offer theories of such agency that explicitly or implicitly minimize the independent significance of political decisions. I therefore think political science should put these issues center stage, a position they intrinsically merit. Throughout the discipline, political scientists should view their descriptive, explanatory inquiries as means to help us decide whether political agency really is governed by specifiable deterministic causes or not, and whether political decisions really matter or not, instead of assuming affirmative or negative answers to those questions.

Since investigators do have to construct theories that supply hypothetical answers to those questions in order to pursue productive research, this admonition is ultimately most aimed at how they *appraise* the results of particular inquiries; but it does have implications for how political scientists should *design* their research. They need to strive consciously to take advantage of the fact that by and large they do share a common subject matter, phenomena that virtually all recognize as politics. That commonality gives the discipline the potential to address these central issues in a variety of ways that nonetheless cohere enough to produce some cumulative knowledge. To exploit that potential, political scientists should think of the field not as committed to any one theory but as committed to systematic comparisons of alternative theories and accounts of their common subject matter, to see what can be learned about political agency. And the way to do that is for political science to take as its central units of analysis not classes or groups or systems or instrumentally rational choices *per se*, but rather, as Skocpol and the new institutionalists suggest, the more general "structure-agent" problem itself: the interaction of (possibly) influential structural contexts and the (possibly) meaningful actions of political agents.

Political science will most progress, I believe, if researchers consciously design their work to be describable by that quite general formulation. Particular studies, to be sure, may well focus on the role that classes, game theorizable strategic interactions, or other elements play in political life. But political science as a discipline should demand of all such investigators first, that they facilitate comparisons of their substantively and methodologically distinct inquiries by studying phenomena they think to be politically important and know to be so viewed by proponents of other schools of thought, who may therefore also study those phenomena. Second, researchers should seek as much as possible to examine a range of political phenomena, and to sample that range if necessary, without biassing their results by choosing phenomena or samples that best suit their hypotheses or theories. Third, scholars should specify clearly the structural contexts, agents and decisions they are examining and their hypotheses concerning their relative roles. And fourth, in evaluating their results, they should indicate explicitly how successful they have been in finding results consistent or inconsistent with the existence of deterministic causal mechanisms, and give some attention to how the political conduct they have examined may have altered the contexts of future political action.

If scholars complied with these precepts, then the discipline as a whole might be better able to accumulate some knowledge of how well models emphasizing different causal contexts account for the same political decisions and actions; of how well different structures seem to account for ranges of political conduct; of where political scientists have found behavior that simply does not seem well captured by any deterministic account; and of how those apparently autonomous actions, in particular, have affected the political world. Political scientists then might be able to pursue even more complex questions concerning when different structural contexts are most salient, how they interact, when and how more genuinely autonomous political conduct seems to occur, and what broader historical patterns, either cyclical or developmental, can be discerned and explained in light of this knowledge. At some point, a grand theory of politics, including an account of historical development, might re-emerge; or, if significantly autonomous human agency seems to be present, political scientists might understand more surely why such a theory cannot be formulated. In the interim, political scientists could view their profession not as troubled, confused, or conflicted because it lacks a paradigm, nor as simply offering an eclectic pluralism that may or may not work toward greater unity of its own accord. Rather, the profession would be self-consciously united by common concerns with truly important issues and by comparable but not identical methods that permit it to make some progress toward better cumulative understanding of those issues.

Political science was once unified, at least to some degree, by shared familiarity with the traditional canon of great works in political philosophy, and some readers may wonder how such texts would fit into the sort of discipline I am describing. I believe they would be central to it, in at least two ways. First, great works of political theory arguably craft or at least articulate memorably structures of political ideas that may themselves be causal factors in political developments, a possibility that can be explored through a combination of careful textual readings and empirical historical inquiries. Second, by presenting powerful alternative views of politics, the canon of great works can serve as an invaluable guide for significant theories and hypotheses about how politics operates and what alternate arrangements might be feasible, thus helping empirical investigators avoid both reinventing wheels and wallowing in trivia. As I have argued elsewhere, these hypotheses could well include notions about the existence and impact of any ultimate moral reality that may exist, as well as more mundane factors.[14]

Few will be persuaded of all this, however, without more effort to address the obvious ambiguities and difficulties in the basic formulation I have offered, "the interaction of (possibly) influential structural contexts and (possibly) meaningful actions of political agents." Questions immediately arise concerning the terms it employs. What counts as "political"? What count as "actions"? What sorts of things are eligible to be considered "agents," what "structural contexts"? The answers will rightly be contested, which in itself argues for inclusive definitions. Let me nonetheless tender some suggestions.

"Politics" should center on the apparent exercise of power by human beings over each other, recognizing that power has many faces and that deterministic theories may ultimately indicate that none of us exercises power meaningfully at all. Moreover, the field should give considerable attention to the struggles over the possession

[14] The discipline should also continue to encompass many forms of normative inquiry and argument. All such efforts inevitably take some stance on the character and significance of political agency, but I would not burden normative theorists with the duty of providing empirical support for their assumptions in every work. For debate over whether the sorts of descriptive work I am advocating can actually assist normative inquiry, as I think, see Smith, 1988a, pp. 105-06; 1989, pp. 74-87; Barber, 1989, pp. 56-73.

and use of governmental power that are at the center of common sense notions of "politics," at least until we have very strong evidence that common sense is wrong and those struggles are not particularly significant. Doing so seems intrinsically wise, and it would also help generate different analyses of the same phenomena, promoting comparison of various types of work. I would, however, certainly also include the exercise of power in non-governmental contexts, such as families and corporations, as aspects of "politics."

Next, although many things might be considered "actions," it is probably most productive to focus on concrete political decisions, choices, and practices, such as electoral votes, enactments of statutes, series of judicial rulings, the commencement of military engagements, revolutionary insurrections, and other exercises of violence, etc. Again, such conduct falls well within the core of most definitions of political action and therefore research directed toward well-specified sets of such decisions will facilitate comparisons of different approaches. A constant danger, admittedly, is that forms of "politics" or "political action" that seem important only from minority points of view will be neglected, even though they may ultimately seem most decisive. But there are few perspectives that do not view as significant at least some decisions that most other perspectives would also single out, and it does not seem too much to ask researchers to be sure to address those decisions in their work.

"Agents" should also be defined broadly to include possible actors like classes, state agencies, nations, etc. But in principle, every political account should ultimately be able to explain how the conduct of actors that are in some way aggregates of individual human beings, such as states, is related to, though not necessarily reducible to, the decisions and actions of those human beings. Every account should at least aspire to help us see the operations of the factors it stresses from the standpoint of our worlds of lived experiences, which are always worlds of people.

"Structural contexts" ought to be conceived as including virtually everything that might be reasonably postulated as influencing human political conduct: genetic structures, structures of psychological drives, enduring governmental institutions, economic relations and technologies, distributions of military power, arrays of organized pressure groups, languages, ideological persuasions or traditions, mass cultural attitudes, etc. Again, refusing to purport to have a privileged general theory of politics rules out assuming the correctness of existing claims for some of these factors; it does not rule out their exploration. Thus most practitioners of existing political science approaches would simply be urged to present their preferred independent variables as hypothetically influential structures shaping specified dependent variables, certain sets of political decisions by identifiable political actors. Then researchers would be asked to present their empirical evidence that these decisions were indeed consonant with the hypothesized influences of the structures they emphasize.

All this so far can rightly be seen as no more than an endorsement of the traditional scientific logic of hypothesis specification followed by honest efforts at empirical falsification. Nonetheless, these suggestions are neither uncontroversial nor trivial. Some may feel uncomfortable describing their independent variables as "structural contexts," but I cannot see how doing so works important substantive distortions on any existing body of theory. Indeed political scientists of virtually all stripes use such language at least occasionally. But more than semantic difficulties will arise. Rational choice theorists, for example, often concentrate on modelling the interaction of structures and decisions. They tend to assume certain structures of preferences and "rules of the game" and then to work out mathematical expressions of strategic calculations or aggregation processes. Some then look simply for empircal instances of such behavior in the world. At times their substantive concern is to show how actors can deal with difficulties, such as cycling problems, generated most clearly by the rational choice theorist's assumed premises. On the approach recommended here, they would be more pressed to model decisions that other schools of thought on politics view as worth explaining and to consider how well their theories accounted for fair samples of the full range of relevant phenomena in the world. These pressures might also lead them to begin more often with empirically visible institutional rules and structures of preferences (although many now do so). Traditional behavioralists more consistently explore partisan and governmental conduct that most agree is central to politics, but both they and rational choice scholars tend to ignore quests to obtain and preserve meaning (beyond possession of wealth and power) as important parts of political behavior. They would be encouraged to consider hypotheses about such quests as legitimate members of the discipline's family of potential explanations (as, again, many are increasingly doing).

The dismissive attitudes of some traditional empiricists are, however, partly explained by the way students of meaningful ideologies, particularly the hermeneutically-minded, often themselves dismiss any need to describe very precisely the structures of consciousness they are alleging to be influential, or to hypothesize specifically about how those structures shape identifiable actors and decisions. At times some writers suggest that one can only judge a hermeneutical account by intuitive decisions about whether the proffered interpretations resonate with one's own sense of meaning. But whether or not this is so, scholars can still specify the ideological or linguistic structures they perceive as constituting the consciousness of political actors or the meaning of political conduct, and they can indicate the political actions, or at least the range of possible political actions, that should follow if their interpretations have force. If their accounts really capture or explain something about intersubjectively recognized political decisions that others miss, they should be able tell us quite concretely what they believe that something is. Finally, scholars for whom the turn to history means chiefly "bringing the state back in" would on this approach not be able to center research on that enterprise alone.

Furthermore, the approach I am advocating has another stage that is compatible with but not necessarily included in the traditional logic of scientific method. As my colleague Alexander Wendt has written, the interplay of structural contexts and the actions of political agents may take several basic forms. We can conceive of structures as being determinative of agents' behavior; agents as instead determining the constitution and operation of structures; or of structures and agents as somehow "co-determining" or "mutually constitutive" (Wendt, 1987, pp. 337-39). We can, moreover, flesh out the nature of those determinative or mutually determinative influences in innumerable ways.

We might, for example, think of some structure, say a system of economic production, as determining all the basic political choices of agents like coal miners. We might instead think their relationship to the economic system determines a particular range of coal miners' political decisions, such as their plainly economic ones, but not other sorts of political decisions, such as support for government aid to religions. Or we might think of economic relations as constituting actors like coal miners with certain interests and capacities that make a specifiable range of political behaviors probable for them, without determining any of their concrete political choices. (Or, of course, we may deny that a structural context, such as the system of production, has anything much to do with those agents' character and choices). We may also think in any of these cases that the decisions and practices of the political actors have some greater or lesser effect on the structure with which we began: coal miners might lead a mounting series of general strikes that finally overthrows existing economic relations. But the more we see the original structure as determining the decisions of the political actors in question, the less we are likely to see those decisions as matters of interest in their own right.

Although the discipline should not be confined to any particular conception of the "true" structure-agent relationship, it should be committed to exploring both aspects of it, not only the effects of structures on agents but also those of agents on structures. Thus, beyond urging that political scientists specify their projects in these terms of structural factors, political agents, and hypotheses about their interaction, I am suggesting that we regularly do some further work.[15] Part of the time, in separate projects if necessary, researchers should focus on the second half of the interaction, on how political decisions, particularly those that appear relatively autonomous, have affected the actors' environments, particularly those structures that political scientists consider most important. By regularly looking for actions for which deterministic explanations seem to run out and at the consequences of those actions, political scientists can gain an improved empirical grasp of what significantly autonomous political agency looks like and how important it is in historical political developments and transformations. That knowledge is crucial if we are to judge better whether and how far political decisions really do matter and understand better the political agents making such decisions.

Once we include in the profession's agenda investigations not only of how various structural contexts shape political agents and important political decisions, but also how far those agents reconstitute their contexts, it should be clear why the sort of "new institutionalist" approach I am advocating must be historical. Although individual studies might still simply model the operations of particular structures in contemporary politics, the concerns of the discipline as a whole would include appraisal of whether and how the constitution of both structures and agents have been and can be transformed over time via their mutual interaction. Work that explores the possible existence of an ongoing, transformative dialectic between structures has to involve historical analysis, consideration of at least one and often more instances of a "structures to decisions to structures" chain of events. Moreover, such historical analysts are probably well advised to attend not only to "normal politics" within various regimes but also to the "extra-ordinary" politics of revolutionary and founding periods, for these are the times when we are most likely to find political actors altering not only their strategies but their very senses of political identity and purpose, even as they creates institutions and policies that may dramatically reorder the political lives of their communities. And again, political scientists might work over time toward more general theories of the role of various structural contexts and political agents in shaping historical political development and triggering such transformations; although if meaningfully autonomous human agency does in fact play a significant role in human history, we should not expect any overarching theory on the order of classical Marxism or structural-functionalist modernization theory to be plausible.[16]

[15] For a similar suggestion, see Wendt, 1987, p. 364-65.

[16] For useful reflections on how "new institutionalist" historical accounts are more likely to portray "multilayered" histories mapping temporaral alignments of independently developing "orders" in different facets of political life, see Skowronek, 1990.

If the various turns to history in the contemporary discipline do lead political scientists to forms of work approximating those I am describing here, a not wholly unrealistic expectation since most existing modes will not have to undergo any dramatic revisions, I believe the turns will have significantly strengthened the discipline. Political scientists will be able to preserve their existing accomplishments while also addressing the calls to consider how political institutions and actors might be transformed over time that have properly come to greater prominence in the "postbehavioral" era. At this point, however, some will wonder how such a political science differs from history, and if it does not, whether it should be termed a social science at all.

These are large topics, and as I am seeking to conclude, I do not wish to take them up in depth here. Let me briefly observe that, although such a political science would not in my view differ much from what many good historians do in practice, it would be more dedicated to explicit use of traditional scientific methods of theory and hypothesis formulation and fair testing, and it would focus more consistently on discovery of the causal influences of basic structures or lack thereof, than most traditional narrative histories. Despite my title, I firmly believe the methods I describe merit the name "science." But for those who insist science involves not only such methods but also the assumption of deterministic behavior or the singleminded quest to elaborate one comprehensive theory of the phenomena under study, I acknowledge that the political science I propose would fall short of their demands, at least for the foreseeable future and probably always. Although some would call the resulting "non-science" a violation of the promise the discipline's name implies, I do not think such names alone should determine what tasks we set ourselves. I suggest that, whether or not we call it a science, there is much to be said for an intellectual discipline that eschews premature reliance on obviously partial theories in order to encourage a diverse array of comparable inquiries into matters that virtually all regard as both vital and unsettled. I believe, in fact, that it is an appropriate expression of the best scientific spirit to conceive of our enterprise in terms that leave open questions open, constraining research only by what is required for effective communication and collective analysis of our results. It is also an appropriate expression of the human spirit to refuse to rule out in advance the possibility that what many of us hope and believe about politics, that through it people can make a difference in their lives, may actually turn out to be true.

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