

**PROBLEMS, METHODS, AND THEORIES
IN THE STUDY OF POLITICS, OR
WHAT'S WRONG WITH POLITICAL
SCIENCE AND WHAT TO DO ABOUT IT**

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Our mandate is to engage in navel-gazing about the condition of political theory. I confess that I find myself uncomfortable with this charge because I think political theorists have become altogether too narcissistic over the past half-century. Increasingly, they have come to see themselves as engaged in a specialized activity distinct from the rest of political science—either a bounded subdiscipline within it or an alternative to it. Political theorists are scarcely unusual in this regard; advancing specialization has been a hallmark of most academic disciplines in recent decades. When warranted, it facilitates the accumulation of knowledge in ways that would not otherwise be achieved. In many physical, biomedical, and informational sciences, the benefits are visible in expanding bodies of knowledge that were scarcely conceivable a generation ago. Specialization has also proceeded apace in the human sciences, seen in the proliferation of dedicated journals, professional organizations and suborganizations, and esoteric discourses notable for their high entry costs to the uninitiated. Here tangible advances in knowledge are less easily identified, however. In political science, even when the new subfields fly interdisciplinary banners (as with the new political economy in much American and comparative politics, the turn to social theory in international relations, or to approaches from moral philosophy in theorizing about justice), those who have not paid the entry costs would be hard-pressed to

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understand—let alone evaluate—the alleged contributions of the new specialized fields.

The specialization that has divided political philosophy from the rest of political science has been aided and abetted by the separation of normative from empirical political theory, with political philosophers declaring a monopoly over the former while abandoning the enterprise of “positive” political theory to other political scientists. This seems to me to have been bad for both ventures. It has produced normative theory that is no longer informed, in the ways that the great theorists of the tradition took it for granted that political theory should be informed, by the state of empirical knowledge of politics. A result is that normative theorists spend too much time commenting on one another, as if they were themselves the appropriate objects of study. This separation has also fed the tendency for empirical political theory to become banal and method driven—detached from the great questions of the day and focused instead on what seems methodologically most tractable. Both types of theory have evolved close to the point where they are of scant interest to anyone other than their practitioners. This might bump up citation indexes and bamboozle tenure committees in the desired ways, but it scarcely does much for the advancement of knowledge about what is or ought to be the case in politics.

My discomfort extends to commenting at length on this state of affairs, which replicates the disorder under discussion even more than Descartes's *cogito* established his existence. Rather, my plan here is to illustrate what I take to be one of the central challenges for political theorists: serving as roving ombudsmen for the truth and the right by stepping back from political science as practiced to see what is wrong with what is currently being done and say something about how it might be improved. **qqSKW18** Holding the discipline's feet to the fire might be an appropriate slogan. Let me hasten to add that I have no interest in declaring that this is the only important task for political theorists or indeed that it is the most important task, only that it is *an* indispensable task. If political theorists do not do it, then it seems to me to be unlikely that it will be done at all.

Donald Green and I have previously criticized contemporary political science for being too method driven and not sufficiently problem driven.¹ In various ways, many have responded that our critique fails to take full account of how inevitably theory laden empirical research is. Here I agree with many of these basic claims, but I argue that they do not weaken the contention that empirical research and explanatory theories should be problem driven. Rather, they suggest that one central task for political theorists should be to identify, criticize, and suggest plausible alternatives to the theoretical assumptions, interpretations of political conditions, and above all the specifi-

cations of problems that underlie prevailing empirical accounts and research programs and to do it in ways that can spark novel and promising problem-driven research agendas.

My procedure will be to develop and extend our arguments for problem-driven over method-driven approaches to the study of politics. Green and I made the case for starting with a problem in the world, next coming to grips with previous attempts that have been made to study it, and then defining the research task by reference to the value added. We argued that method-driven research leads to self-serving construction of problems, misuse of data in various ways, and related pathologies summed up in the old adage that if the only tool you have is a hammer, everything around you starts to look like a nail. Examples include collective action problems such as free riding that appear mysteriously to have been “solved” when perhaps it never occurred to anyone to free ride to begin with in many circumstances, or the concoction of elaborate explanations for why people “irrationally” vote, when perhaps it never occurred to most of them to think by reference to the individual costs and benefits of the voting act. The nub of our argument was that more attention to the problem and less to vindicating some pet approach **qqSKW19** would be less likely to send people on esoteric goose chases that contribute little to the advancement of knowledge.

What we dubbed “method-driven” work in fact conflated theory-driven and method-driven work. These can be quite different things, though in the literature they often morph into one another as when rational choice is said to be an “approach” rather than a theory. From the point of view elaborated here, the critical fact is that neither is problem driven, where this is understood to require specification of the problem under study in ways that are not mere artifacts of the theories and methods that are deployed to study it. Theory-drivenness and method-drivenness undermine problem-driven scholarship in different ways that are attended to below, necessitating different responses. This is not to say that problem selection is, or should be, uninfluenced by theories and methods, but I will contend that there are more ways than one of bringing theory to bear on the selection of problems and that some are better than others.

Some resisted our earlier argument on the grounds that refinement of theoretical models and methodological tools is a good gamble in the advancement of science as part of a division of labor. It is sometimes noted, for instance, that when John Nash came up with his equilibrium concept (an equilibrium from which no one has an incentive to defect), he could not think of an application, yet it has since become widely used in political science.² We registered skepticism at this approach in our book, partly because the ratio of suc-

cess to failure is so low, and partly because our instinct is that better models are likely to be developed in applied contexts, in closer proximity to the data.

I do not want to rehearse those arguments here. Rather, my goal is to take up some weaknesses in our previous discussion of the contrast between problem-drivenness and method- and theory-drivenness and explore their implications for the study of politics. Our original formulation was open to two related objections: that the distinction we were attempting to draw is a distinction without a difference and that there is no theory-independent way of characterizing problems. These are important objections, necessitating more elaborate responses than Green and I offered. My response to them in parts I and II leads to a discussion, in part III, of the reality that there are always multiple true descriptions of any given piece of social reality, where I argue against the reductionist impulse always to select one type of description as apt. This leaves us with the difficulty of selecting among potential competing descriptions of what is to be accounted for in politics, taken up in part IV. There I explore the notion that the capacity to generate successful predictions is the appropriate criterion. In some circumstances, this is the right answer, but it runs the risk of atrophying into a kind of method-drivenness that traps researchers into forlorn attempts to refine predictive instruments. Moreover, insisting on the capacity to generate predictions as the criterion for problem-selection risks predisposing the political science enterprise to the study of trivial, if tractable, problems. In light of prediction's limitations, I turn, in part V, to other ways in which the aptness of competing accounts can be assessed. There I argue that political theorists have an important role to play in scrutinizing accepted accounts of political reality: exhibiting their presuppositions, both empirical and normative, and posing alternatives. Just because observation is inescapably theory laden, this is bound to be an ongoing task. Political theorists have a particular responsibility to take it on when accepted depictions of political reality are both faulty and widely influential outside the academy.

I. A DISTINCTION WITHOUT A DIFFERENCE?

The claim that the distinction between problem- and theory-driven research is a distinction without a difference turns on the observation that even the kind of work that Green and I characterized as theory driven in fact posits a problem to study. This can be seen by reflecting on some manifestly theory-driven accounts.

Consider, for instance, a paper sent to me for review by the *American Political Science Review* on the probability of successful negotiated transi-

tions to democracy in South Africa and elsewhere. It contended, *inter alia*, that as the relative size of the dispossessed majority grows, the probability of a settlement decreases for the following reason: members of the dispossessed majority, as individual utility maximizers, confront a choice between working and fomenting revolution. Each one realizes that, as their numbers grow, the individual returns from revolution will decline, assuming that the expropriated proceeds of revolution will be equally divided among the expropriators. Accordingly, as their relative numbers grow, they will be more likely to devote their energy to work than to fomenting revolution, and, because the wealthy minority realizes this, its members will be less inclined to negotiate a settlement as their numbers diminish since the threat of revolution is receding.

One only has to describe the model for its absurdity to be plain. Even if one thought dwindling minorities are likely to become increasingly recalcitrant, it is hard to imagine anyone coming up with this reasoning as part of the explanation. In any event, the model seems so obtuse with respect to what drives the dispossessed to revolt and fails so obviously to take manifestly relevant factors into account (such as the changing likelihood of revolutionary success as the relative numbers change), that it is impossible to take seriously. In all likelihood, it is a model that was designed for some other purpose, and this person is trying to adapt it to the study of democratic transitions. One can concede that even such manifestly theory-driven work posits problems yet nonetheless insist that such specification is contrived. It is an artifact of the theoretical model in question.

Or consider the neo-Malthusian theory put forward by Charles Murray to the effect that poor women have children in response to the perverse incentives created by Aid to Families with Dependent Children and related benefits.³ Critics such as Katz pointed out that on this theory, it is difficult to account for the steady increase in the numbers of children born into poverty since the early 1970s, when the real value of such benefits has been stagnant or declining.⁴ Murray's response (in support of which he cited no evidence) has only to be stated for its absurdity to be plain: "In the late 1970s, social scientists knew that the real value of the welfare benefit was declining, but the young woman in the street probably did not."⁵ This is clearly self-serving for the neo-Malthusian account, even if in a palpably implausible way. Again, the point to stress here is not that no problem is specified; Murray is interested in explaining why poor women have children. But the fact that he holds on to his construction of it as an attempt by poor women to maximize their income from the government even in the face of confounding evidence suggests that he is more interested in vindicating his theory than in understanding the problem.

Notice that this is not an objection to modeling. To see this, compare these examples to John Roemer's account of the relative dearth of redistributive policies advocated by either political party in a two-party democracy with substantial ex-ante inequality.⁶ He develops a model that shows that if voters' preferences are arrayed along more than one dimension—such as “values” as well as “distributive” dimensions—then the median voter will not necessarily vote for downward redistribution as he would if there were only a single distributive dimension. His model seems to me worth taking seriously (leaving aside for present purposes how well it might do in systematic empirical testing) because the characterization of the problem that motivates it is not forced as in the earlier examples. Trying to develop the kind of model he proposes to account for it seems therefore to be worthwhile.⁷

In light of these examples, we can say that the objection that theory-driven research in fact posits problems is telling in a trivial sense only. If the problems posited are idiosyncratic artifacts of the researcher's theoretical priors, then they will seem tendentious, if not downright misleading, to everyone except those who are wedded to her priors. Put differently, if a phenomenon is characterized as it is so as to vindicate a particular theory rather than to illuminate a problem that is specified independently of the theory, then it is unlikely that the specification will gain much purchase on what is actually going on in the world. Rather, it will appear to be what it is: a strained and unconvincing specification driven by the impulse to vindicate a particular theoretical outlook. It makes better sense to start with the problem, perhaps asking what the conditions are that make transitions to democracy more or less likely, or what influences the fertility rates of poor women. Next see what previous attempts to account for the phenomenon have turned up and only then look to the possibility of whether a different theory will do better. To be sure, one's perception of what problems should be studied might be influenced by prevailing theories, as when the theory of evolution leads generations of researchers to study different forms of speciation. But the theory should not blind the researcher to the independent existence of the phenomenon under study. When that happens, appropriate theoretical influence has atrophied into theory-drivenness.

II. ALL OBSERVATION IS THEORY LADEN?

It might be objected that the preceding discussion fails to come to grips with the reality that there is no theory-independent way to specify a problem. This claim is sometimes summed up with the epithet that “all observation is theory laden.” Even when problems are thought to be specified independ-

ently of the theories that are deployed to account for them, in fact they always make implicit reference to some theory. From this perspective, the objection would be reformulated as the claim that the contrast between problem-driven and theory-driven research assumes there is some pretheoretical way of demarcating the problem. But we have known since the later Wittgenstein, J. L. Austin, and Thomas Kuhn that there is not. After all, in the example just mentioned, Roemer's specification of problem is an artifact of the median voter theorem and a number of assumptions about voter preferences. The relative dearth of redistributive policies is in tension with that specification, and it is this tension that seems to call for an explanation. Such considerations buttress the insistence that there simply is no pretheoretical account of "the facts" to be given.

A possible response at this juncture would be to grant that all description is theory laden but retort that some descriptions are more theory laden than others. Going back to my initial examples of democratic transitions and welfare mothers, we might say that tendentious or contrived theory-laden descriptions fail on their own terms: one does not need to challenge the theory that leads to them to challenge them. Indeed, the only point of referring to the theory at all is to explain how anyone might come to believe them. The failure stems from the fact that, taken on its own terms, the depiction of the problem does not compute. We have no good reason to suppose that revolutionaries will become less militant as their relative numbers increase or that poor women have increasing numbers of babies to get decreasingly valuable welfare checks. Convincing as this might be as response to the examples given, it does not quite come to grips with what is at stake for social research in the claim that all description is theory laden.

Consider theory-laden descriptions of institutions and practices that are problematic even though they do not fail on their own terms, such as Kathleen Bawn's claim that an ideology is a blueprint around which a group maintains a coalition⁸ or Russell Hardin's claim that constitutions exist to solve coordination problems.⁹ Here the difficulty is that, although it is arguable that ideologies and constitutions serve the designated purposes, they serve many other purposes as well. Moreover, it is far from clear that any serious investigation of how particular ideologies and constitutions came to be created or are subsequently sustained would reveal that the theorist's stipulated purpose has much to do with either. They are "just so" stories, debatably plausible conjectures about the creation and or operation of these phenomena.¹⁰

The difficulty here is not that Bawn's and Hardin's are functional explanations. Difficult as functional explanations are to test empirically, they may sometimes be true. Rather, the worry is that these descriptions might be of the following form: trees exist for dogs to pee on. Even when a sufficient account

is not manifestly at odds with the facts, there is no reason to suppose that it will ever get us closer to reality unless it is put up against other plausible conjectures in such a way that there can be decisive adjudication among them. Otherwise we have “Well, in that case what are lamp posts for?” Ideologies may be blueprints for maintaining coalitions, but they also give meaning and purpose to people’s lives, mobilize masses, reduce information costs, contribute to social solidarity, and facilitate the demonization of “out-groups”—to name some common candidates. Constitutions might help solve coordination problems, but they are often charters to protect minority rights, legitimating statements of collective purpose, instruments to distinguish the rules of the game from the conflicts of the day, compromise agreements to end or avoid civil wars, and so on. Nor are these characterizations necessarily competing: ideologies and constitutions might well perform several such functions simultaneously. Selecting any one over others implies a theoretical commitment. This is one thing people may have in mind when asserting that all observation is theory laden.

One can concede the point without abandoning the problem-driven/theory-driven distinction, however. The theory-driven scholar commits to a sufficient account of a phenomenon, developing a “just so” story that might seem convincing to partisans of her theoretical priors. Others will see no more reason to believe it than a host of other “just so” stories that might have been developed, vindicating different theoretical priors. By contrast, the problem-driven scholar asks, “Why are constitutions enacted?” or “Why do they survive?” and “Why do ideologies develop?” and “Why do people adhere to them?” She then looks to previous theories that have been put forward to account for these phenomena, tries to see how they are lacking, and whether some alternative might do better. She need not deny that embracing one account rather than another implies different theoretical commitments, and she may even hope that one theoretical outcome will prevail. But she recognizes that she should be more concerned to discover which explanation works best than to vindicate any priors she may happen to have. As with the distinction-without-a-difference objection, then, this version of the theory-ladenness objection turns out on inspection at best to be trivially true.

III. MULTIPLE TRUE DESCRIPTIONS AND APTNESS

There is a subtler sense in which observation is theory laden, untouched by the preceding discussion though implicit in it. The claim that all observation is theory laden scratches the surface of a larger set of issues having to do with the reality that all phenomena admit of multiple true descriptions. Consider

possible descriptions of a woman who says "I do" in a conventional marriage ceremony. She could be

- expressing authentic love,
- doing (failing to do) what her father has told her to do,
- playing her expected part in a social ritual,
- unconsciously reproducing the patriarchal family,
- landing the richest husband that she can, or
- maximizing the chances of reproducing her genes.

Each description is theory laden in the sense that it leads to the search for a different type of explanation. This can be seen if in each case we ask the question *why?* and see what type of explanation is called forth.

- *Why does she love him?* predisposes us to look for an explanation in terms of her personal biography.
- *Why does she obey (disobey) her father?* predisposes us to look for a psychological explanation.
- *Why does she play her part in the social ritual?* predisposes us to look for an anthropological explanation.
- *Why does she unconsciously reproduce patriarchy?* predisposes us to look for an explanation in terms of ideology and power relations.
- *Why does she do as well as she can in the marriage market?* predisposes us to look for an interest-based rational choice explanation.
- *Why does she maximize the odds of reproducing her genes?* predisposes us to look for a socio-biological explanation.

The claim that all description is theory laden illustrated here is a claim that there is no "raw" description of "the facts" or "the data." There are always multiple possible true descriptions of a given action or phenomenon, and the challenge is to decide which is most apt.

From this perspective, theory-driven work is part of a reductionist program. It dictates always opting for the description that calls for the explanation that flows from the preferred model or theory. So the narrative historian who believes every event to be unique will reach for personal biography, the psychological reductionist will turn to the psychological determinants of her choice, the anthropologist will see the constitutive role of the social ritual as the relevant description, the feminist will focus on the action as reproducing patriarchy, the rational choice theorist will reach for the explanation in terms of maximizing success in the marriage market, and for the socio-biologist it will be evolutionary selection at the level of gene reproduction.

Why do this? Why plump for any reductionist program that is invariably going to load the dice in favor of one type of description? I hesitate to say "level" here, since that prejudices the question I want to highlight: whether some descriptions are invariably more basic than others. Perhaps one is, but to presume this to be the case is to make the theory-driven move. Why do it?

The common answer rests, I think, on the belief that it is necessary for the program of social science. In many minds, this enterprise is concerned with the search for general explanations. How is one going to come up with general explanations if one cannot characterize the classes of phenomena one studies in similar terms? This view misunderstands the enterprise of science, provoking three responses: one skeptical, one ontological, and one occupational.

The skeptical response is that whether there are general explanations for classes of phenomena is a question for social-scientific inquiry, not one to be prejudged before conducting that inquiry. At stake here is a variant of the debate between deductivists and inductivists. The deductivist starts from the preferred theory or model and then opts for the type of description that will vindicate the general claims implied by the model, whereas the inductivist begins by trying to account for particular phenomena or classes of phenomena and then sees under what conditions, if any, such accounts might apply more generally. This enterprise might often be theory influenced for the reasons discussed in parts I and II, but it is less likely to be theory driven than the pure deductivist's one because the inductivist is not determined to arrive at any particular theoretical destination. The inductivist pursues general accounts, but she regards it as an open question whether they are out there waiting to be discovered.

The ontological response is that although science is in the second instance concerned with developing general knowledge claims, it must in the first instance be concerned with developing valid knowledge claims. It seems to be an endemic obsession of political scientists to believe that there must be general explanations of all political phenomena, indeed to subsume them into a single theoretical program. Theory-drivenness kicks in when the pursuit of generality comes at the expense of the pursuit of empirical validity. "Positive" theorists sometimes assert that it is an appropriate division of labor for them to pursue generality while others worry about validity. This leads to the various pathologies Green and I wrote about, but the one we did not mention that I emphasize here is that it invites tendentious characterizations of the phenomena under study because the selection of one description rather than another is driven by the impulse to vindicate a particular theoretical outlook.

The occupational response is that political scientists are pushed in the direction of theory-driven work as a result of their perceived need to differen-

tiate themselves from others, such as journalists, who also write about political phenomena for a living, but without the job security and prestige of the professoriate. This aspiration to do better than journalists is laudable, but it should be unpacked in a Lakatosian fashion. When tackling a problem, we should come to grips with the previous attempts to study it, by journalists as well as scholars in all disciplines who have studied it, and then try to come up with an account that explains what was known before—and then some. Too often the aspiration to do better than journalists is cashed out as manufacturing esoteric discourses with high entry costs for outsiders. All the better if they involve inside-the-cranium exercises that never require one to leave one's computer screen.

IV. PREDICTION AS A SORTING CRITERION?

A possible response to what has been said thus far is that prediction should be the arbiter. Perhaps my skepticism is misplaced, and some reductionist program is right. If so, it will lead to correct predictions, whereas those operating with explanations that focus on other types of description will fail. Theory driven or not, the predictive account should triumph as the one that shows that interest maximization, or gene preservation, or the oppression of women, or the domination of the father figure, and so on is “really going on.” On this instrumentalist view, we would say, with Friedman, “deploy whatever theory-laden description you like, but lay it on the line and see how it does in predicting outcomes.” If you can predict from your preferred cut, you win.¹¹

This instrumental response is adequate up to a point. Part of what is wrong with many theory-driven enterprises, after all, is that their predictions can never be decisively falsified. From Bentham through Marx, Freud, functionalism, and much modern rational choice theory, too often the difficulty is that the theory is articulated in such a capacious manner that some version of it is consistent with every conceivable outcome. In effect, the theory predicts everything, so that it can never be shown to be false. This is why people say that a theory that predicts everything explains nothing. If a theory can never be put to a decisive predictive test, there seems little reason to take it seriously.

Theories of everything to one side, venturing down this path raises the difficulty that prediction is a tough test that is seldom met in political science. This difficulty calls to mind the job applicant who said on an interview that he would begin a course on comparative political institutions with a summary of the field's well-tested empirical findings but then had nothing to say when asked what he would teach for the remaining twelve weeks of the semester.

Requiring the capacity to predict is in many cases a matter of requiring more than can be delivered, so that if political science is held to this standard, there would have to be a proliferation of exceedingly short courses. Does this reality suggest that we should give up on prediction as our sorting criterion?

Some, such as MacIntyre, have objected to prediction as inherently unattainable in the study of human affairs due to the existence of free will.¹² Such claims are not convincing, however. Whether or not human beings have free will is an empirical question. Even if we do, probabilistic generalizations might still be developed about the conditions under which we are more likely to behave in one way rather than another. To be sure, this assumes that people are likely to behave in similar ways in similar circumstances, which may or may not be true, but the possibility of its being true does not depend on denying the existence of free will. To say that someone will probably make choice *x* in circumstance *q* does not mean that they cannot choose not-*x* in that circumstance or, that, if do they choose not-*x*, it was not nonetheless more likely ex-ante that they would have chosen *x*. In any event, most successful science does not proceed by making point predictions. It predicts patterns of outcomes. There will always be outliers and error terms; the best theory minimizes them vis-à-vis the going alternatives.

A more general version of this objection is to insist that prediction is unlikely to be possible in politics because of the decisive role played by contingent events in most political outcomes. This, too, seems overstated unless one assumes in advance—with the narrative historian—that everything is contingent. A more epistemologically open approach is to assume that some things are contingent, others not, and try to develop predictive generalizations about the latter. For instance, Courtney Jung and I developed a theory of the conditions that make negotiated settlements to civil wars possible involving such factors as whether government reformers and opposition moderates can combine to marginalize reactionaries and revolutionary militants on their flanks. We also developed a theory of the conditions that are more and less likely to make reformers and moderates conclude that trying to do this is better for them than the going alternatives.¹³ Assuming we are right, contingent triggers will nonetheless be critical in whether such agreements are successfully concluded, as can be seen by reflecting on how things might have developed differently in South Africa and the Middle East had F. W. DeKlerk been assassinated in 1992 or Yitzhak Rabin had not been assassinated in 1995. The decisive role of contingent events rules out ex-ante prediction of success, but the theory might correctly predict failure—as when a moderate IRA leader such as Jerry Adams emerges but the other necessary pieces are not in place, or if Yassir Arafat is offered a deal by Ehud Barak at a time when he is too weak to outflank Hamas and Islamic Jihad. Successful prediction of

failure over a range of such cases would suggest that we have indeed taken the right descriptive cut at the problem.¹⁴

There are other types of circumstance in which capacity to predict will support one descriptive cut at a problem over others. For instance, Przeworski et al. have shown that although level of economic development does not predict the installation of democracy, there is a strong relationship between level of per capita income and the survival of democratic regimes. Democracies appear never to die in wealthy countries, whereas poor democracies are fragile, exceedingly so when per capita incomes fall below \$2,000 (in 1975 dollars). When per capita incomes fall below this threshold, democracies have a one in ten chance of collapsing within a year. Between per capita incomes of \$2,001 and \$5,000, this ratio falls to one in sixteen. Above \$6,055 annual per capita income, democracies, once established, appear to last indefinitely. Moreover, poor democracies are more likely to survive when governments succeed in generating development and avoiding economic crises.¹⁵ If Przeworski et al. are right, as it seems presently that they are, then level of economic development is more important than institutional arrangements, cultural beliefs, presence or absence of a certain religion, or other variables for predicting democratic stability. For this problem, the political economist's cut seems to be the right sorting criterion.¹⁶

These examples suggest that prediction can sometimes help, but we should nonetheless be wary of making it the criterion for problem selection in political science. For one thing, this can divert us away from the study of important political phenomena where knowledge can advance even though prediction turns out not to be possible. For instance, generations of scholars have theorized about the conditions that give rise to democracy (as distinct from the conditions that make it more or less likely to survive once instituted, just discussed). Alexis de Tocqueville alleged it to be the product of egalitarian mores.¹⁷ For Seymour Martin Lipsett, it was a byproduct of modernization.¹⁸ Barrington Moore identified the emergence of a bourgeoisie as critical, while Rueschemeyer, Stephens, and Stephens held the presence of an organized working class to be decisive.¹⁹ We now know that there is no single path to democracy and therefore no generalization to be had about which conditions give rise to democratic transitions. Democracy can result from decades of gradual evolution (Britain and the United States), imitation (India), cascades (much of Eastern Europe in 1989), collapses (Russia after 1991), imposition from above (Spain and Brazil), revolutions (Portugal and Argentina), negotiated settlements (Bolivia, Nicaragua, and South Africa), or external imposition (Japan and West Germany).²⁰

In retrospect, this is not surprising. Once someone invents a toaster, there is no good reason to suppose that others must go through the same invention

processes. Perhaps some will, but some may copy it, some may buy it, some may receive it as a gift, and so on. Perhaps there is no cut at this problem that yields a serviceable generalization and, as a result, no possibility of successful prediction. Political scientists tend to think they must have general theories of everything, as we have seen, but looking for a general theory of what gives rise to democracy may be like looking for a general theory of holes.²¹ Yet we would surely be making an error if our inability to predict in this area inclined us not to study it. It would prevent our discovering a great deal about democracy that is important to know, not least that there is no general theory of what gives rise to it to be had. Such knowledge would also be important for evaluating claims by defenders of authoritarianism who contend that democracy cannot be instituted in their countries because they have not gone through the requisite path-dependent evolution.

Reflecting on this example raises the possibility I want to consider next: making a fetish of prediction can undermine problem-driven research via wag-the-dog scenarios in which we elect to study phenomena because they seem to admit the possibility of prediction rather than because we have independent reasons for thinking it worthwhile to study them. This is what I mean by method-drivenness, as distinct from theory-drivenness. It gains impetus from a number of sources, perhaps the most important being the lack of uncontroversial data concerning many political phenomena. Predictions about whether or not constitutional courts protect civil rights run into disagreements over which rights are to count and how to measure their protection. Predictions about the incidence of war run into objections about how to measure and count the relevant conflicts. In principle, it sounds right to say "let's test the model against the data." In reality, there are few uncontroversial data sets in political science.

A related difficulty is that it is usually impossible to disentangle the complex interacting causal processes that operate in the actual world. We will always find political economists on both sides of the question whether cutting taxes leads to increases or decreases in government revenue, and predictive tests will not settle their disagreements. Isolating the effects of tax cuts from the other changing factors that influence government revenues is just too difficult to do in ways that are likely to convince a skeptic. Likewise, political economists have been arguing at least since Bentham's time over whether trickle-down policies benefit the poor more than do government transfers, and it seems unlikely that the key variables will ever be isolated in ways that can settle this question decisively.

An understandable response to this is to suggest that we should tackle questions where good data is readily available. But taking this tack courts the danger of self-defeating method-drivenness, because there is no reason to

suppose that the phenomena about which uncontroversial data are available are those about which valid generalizations are possible. My point here is not one about curve fitting—running regression after regression on the same data set until one finds the mix of explanatory variables that passes most closely through all the points to be explained. Leaving the well-known difficulties with this kind of data mining to one side, my worry is that working with uncontroversial data because of the ease of getting it can lead to endless quests for a holy grail that may be nowhere to be found.

The difficulty here is related to my earlier discussion of contingency, to wit, that many phenomena political scientists try to generalize about may exhibit secular changes that will always defy their explanatory theories. For instance, trying to predict election outcomes from various mixes of macro political and economic variables has been a growth industry in political science for more than a generation. But perhaps the factors that caused people to vote as they did in the 1950s differ from those forty or fifty years later. After all, this is not an activity with much of a track record of success in political science. We saw this dramatically in the 2000 election in which all of the standard models predicted a decisive Gore victory.²² Despite endless post hoc tinkering with the models after elections in which they fare poorly, this is not an enterprise that appears to be advancing. They will never get it right if my conjecture about secular change is correct.

It might be replied that if that is really so, either they will come up with historically nuanced models that do a better job or universities and funding agencies will pull the plug on them. But this ignores an occupational factor that might be dubbed the Morton Thiokol phenomenon. When the *Challenger* blew up in 1986, there was much blame to go around, but it became clear that Morton Thiokol, manufacturer of the faulty O-ring seals, shouldered a huge part of the responsibility. A naïve observer might have thought that this would mean the end of their contract with NASA, but, of course, this was not so. The combination of high entry costs to others, the dependence of the space program on Morton Thiokol, and their access to those who control resources mean that they continue to make O-ring seals for the space shuttle. Likewise with those who work on general models of election forecasting. Established scholars with an investment in the activity have the protections of tenure and legitimacy, as well as privileged access to those who control research resources. Moreover, high methodological entry costs are likely to self-select new generations of researchers who are predisposed to believe that the grail is there to be found. Even if their space shuttles will never fly, it is far from clear that they will ever have the incentive to stop building them.

To this it might be objected that it is not as if others are building successful shuttles in this area. Perhaps so, but this observation misses my point here:

that the main impetus for the exercise appears to be the ready availability of data, which sustains a coterie of scholars who are likely to continue to try to generalize on the basis of it until the end of time. Unless one provides an account, that, like the others on offer, purports to retrodict past elections and predict the next one, one cannot aspire to be a player in this game at which everyone is failing. If there is no such account to be found, however, then perhaps some other game should be played. For instance, we might learn more about why people vote in the ways that they do by asking them. Proceeding instead with the macro models risks becoming a matter of endlessly refining the predictive instrument as an end in itself—even in the face of continual failure and the absence of an argument about why we should expect it to be successful. Discovering where generalization is possible is a taxing empirical task. Perhaps it should proceed on the basis of trial and error, perhaps on the basis of theoretical argument, perhaps some combination. What should *not* drive it, however, is the ready availability of data and technique.

A more promising response to the difficulties of bad data and of disentangling complex causal process in the “open systems” of the actual world is to do experimental work where parameters can be controlled and key variables more easily isolated.²³ There is some history of this in political science and political psychology, but the main problem has been that of external validity. Even when subjects are randomly selected and control groups are included in the experiments (which often is not done), it is far from clear that results produced under lab conditions will replicate themselves outside the lab.

To deal with these problems, Donald Green and Alan Gerber have revived the practice of field experiments, in which subjects can be randomized, experimental controls can be introduced, and questions about external validity disappear.²⁴ Prediction can operate once more, and when it is successful there are good reasons for supposing that the researcher has taken the right cut at the problem. In some ways this is an exciting development. It yields decisive answers to questions such as which forms of mobilizing voters are most effective in increasing turnout, or what the best ways are for partisans to get their grassroots supporters to the polls without also mobilizing their opponents.

Granting that this is an enterprise that leads to increments in knowledge, I want nonetheless to suggest that it carries risks of falling into a kind of method-drivenness that threatens to diminish the field-experiment research program unless they are confronted. The potential difficulties arise from the fact that field experiments are limited to studying comparatively small questions in well-defined settings, where it is possible to intervene in ways that allow for experimental controls. Usually this means designing or piggybacking on interventions in the world such as get-out-the-vote efforts or

attempts at partisan mobilization. Green and Gerber have shown that such efforts can be adapted to incorporate field experiments.

To be sure, the relative smallness of questions is to some extent in the eye of the beholder. But consider a list of phenomena that political scientists have sought to study, and those drawn to political science often want to understand, that are not likely to lend themselves to field experiments:

- the effects of regime type on the economy, and vice versa;
- the determinants of peace, war, and revolution;
- the causes and consequences of the trend toward creating independent central banks;
- the causes and consequences of the growth in transnational political and economic institutions;
- the relative merits of alternative policies for achieving racial integration, such as mandatory bussing, magnet schools, and voluntary desegregation plans;
- the importance of constitutional courts in protecting civil liberties, property rights, and limiting the power of legislatures;
- the effects of other institutional arrangements, such as parliamentarism versus presidentialism, unicameralism versus bicameralism, federalism versus centralism on such things as the distribution of income and wealth, the effectiveness of macroeconomic policies, and the types of social policies that are enacted; and
- the dynamics of political negotiations to institute democracy.

I could go on, but you get the point.

This is not to denigrate field experiments. One of the worst features of methodological disagreement in political science is the propensity of protagonists to compare the inadequacies of one method with the adequacies of a second and then declare the first to be wanting.²⁵ Since all methods have limitations and none should be expected to be serviceable for all purposes, this is little more than a shell game. If a method can do some things well that are worth doing, that is a sufficient justification for investing some research resources in it. With methods, as with people, if you focus only on their limitations you will always be disappointed.

Field experiments lend themselves to the study of behavioral variation in settings where the institutional context is relatively fixed and where the stakes are comparatively low so that the kinds of interventions required do not violate accepted ethical criteria for experimentation on human subjects. They do not obviously lend themselves to the study of life-or-death and other high-stakes politics, war and civil war, institutional variation, the macro-political economy, or the determinants of regime stability and change. This still leaves a great deal to study that is worth studying, and creative use of the method might render it deployable in a wider array of areas than I have noted

here. But it must be conceded that it also leaves out a great deal that draws people to political science, so that if susceptibility to study via field experiment becomes the criterion for problem selection, then it risks degenerating into method-drivenness.

This is an important caution. I would conjecture that part of the disaffection with 1960s behaviorism in the study of American politics that spawned the model mania of the 1990s was that the behaviorists became so mindlessly preoccupied with demonstrating propositions of the order "Catholics in Detroit vote Democrat."²⁶ As a result, the mainstream of political science that they came to define seemed to others to be both utterly devoid of theoretical ambition as well as detached from consequential questions of politics, frankly boring. To paraphrase Kant, theoretical ambition without empirical research may well be vacuous, but empirical research without theoretical ambition will be blind.

V. UNDERVALUING CRITICAL REAPPRAISAL OF WHAT IS TO BE EXPLAINED

The emphasis on prediction can lead to method-drivenness in another way: it can lead us to undervalue critical reappraisals of accepted descriptions of reality. To see why this is so, one must realize that much commentary on politics, both lay and professional, takes depictions of political reality for granted that closer critical scrutiny would reveal as problematic. Particularly, though not only, when prediction is not going to supply the sorting device to get us the right cut, political theorists have an important role to play in exhibiting what is at stake in taking one cut rather than another and in proposing alternatives. Consider some examples.

For more than a generation in debates about American exceptionalism, the United States was contrasted with Europe as a world of relative social and legal equality deriving from the lack of a feudal past. This began with de Tocqueville, but it has been endlessly repeated and became conventional wisdom, if not a mantra, when restated by Louis Hartz in *The Liberal Tradition in America*. But as Rogers Smith showed decisively in *Civic Ideals*, it is highly misleading as a descriptive matter.²⁷ Throughout American history, the law has recognized explicit hierarchies based on race and gender whose effects are still very much with us. Smith's book advances no well-specified predictive model, let alone tests one, but it displaces a highly influential orthodoxy that has long been taken for granted in debates about pluralism and cross-cutting cleavages, the absence of socialism in America, arguments about the so-called end of ideology, and the ideological neutrality of the lib-

eral tradition.²⁸ Important causal questions are to be asked and answered about these matters, but my point here is that what was thought to stand in need of explanation was so misspecified that the right causal questions were not even on the table.

Likewise with the debate about the determinants of industrial policy in capitalist democracies. In the 1970s, it occurred to students of this subject to focus less on politicians' voting records and campaign statements and look at who actually writes the legislation. This led to the discovery that significant chunks of it were actually written by organized business and organized labor with government (usually in the form of the relevant minister or ministry) in a mediating role. The reality was more of a "liberal corporatist" one, and less of a pluralist one, than most commentators who had not focused on this had realized.²⁹ The questions that then motivated the next generation of research became as follows: Under what conditions do we get liberal corporatism, and what are its effects on industrial relations and industrial policy? As with the Tocqueville-Hartz orthodoxy, the causal questions had to be reframed because of the ascendancy of a different depiction of the reality.³⁰

In one respect, the Tocqueville-Hartz and pluralist accounts debunked by Smith and the liberal corporatists are more like those of democratic transitions and the fertility rates of welfare mothers discussed in part I than the multiple descriptions problem discussed in part III. The difficulty is not how to choose one rather than another true description but rather that the Tocqueville-Hartz and pluralist descriptions fail on their own terms. By focusing so myopically on the absence of feudalism and the activities of politicians, their proponents ignored other sources of social hierarchy and decision making that are undeniably relevant once they have been called to attention. The main difference is that the democratic transition and welfare mother examples are not as widely accepted as the Tocqueville-Hartz and pluralist orthodoxies were before the debunkers came along. This should serve as a salutary reminder that orthodox views can be highly misleading and that an important ongoing task for political theorists is to subject them to critical scrutiny. This involves exhibiting their presuppositions, assessing their plausibility, and proposing alternatives when they are found wanting. This is particularly important when the defective account is widely accepted outside the academy. If political science has a role to play in social betterment, it must surely include debunking myths and misunderstandings that shape political practice.³¹

Notice that descriptions are theory laden not only in calling for a particular empirical story but often also in implying a normative theory that may or may not be evident unless this is made explicit. Compare the following two descriptions:

- The Westphalian system is based on the norm of national sovereignty.
- The Westphalian system is based on the norm of global apartheid.

Both are arguably accurate descriptions, but, depending which of the two we adopt, we will be prompted to ask exceedingly different questions about justification as well as causation. Consider another instance:

- When substantial majorities in both parties support legislation, we have bipartisan agreement.
- When substantial majorities in both parties support legislation, we have collusion in restraint of democracy.

The first draws on a view of democracy in which deliberation and agreement are assumed to be unproblematic, even desirable goals in a democracy. The second, antitrust-framed formulation calls to mind Mill's emphasis on the centrality of argument and contestation and the Schumpeterian impulse to think of well-functioning democracy as requiring competition for power.³²

Both *global apartheid* and *collusion in restraint of democracy* here are instances of problematizing redescrptions. Just as Smith's depiction of American public law called the Tocqueville-Hartz consensus into question, and the liberal corporatist description of industrial legislation called then-conventional assumptions about pluralist decision making into question, so do these. But they do it not so much by questioning the veracity of the accepted descriptions as by throwing their undernoticed benign normative assumptions into sharp relief. Describing the Westphalian system as based on a norm of global apartheid, or political agreement among the major players in a democracy as collusion in restraint of democracy, shifts the focus to underattended features of reality, placing different empirical and justificatory questions on the table.

But are they the right questions?

To answer this by saying that one needs a theory of politics would be to turn once more to theory-drivenness. I want to suggest a more complex answer, one that sustains problematizing redescription as a problem-driven enterprise. It is a two-step venture that starts when one shows that the accepted way of characterizing a piece of political reality fails to capture an important feature of what stands in need of explanation or justification. One then offers a recharacterization that speaks to the inadequacies in the prior account.

When convincingly done, prior adherents to the old view will be unable to ignore it and remain credible. This is vital because it will, of course, be true that the problematizing redescription is itself usually a theory-influenced, if

not a theory-laden, endeavor. But if the problematizing redescription assumes a theory that seems convincing only to partisans of her priors, or is validated only by reference to evidence that is projected from her alternative theory, then it will be judged tendentious to the rest of the scholarly community for the reasons I set out in parts I and II of this essay. It is important, therefore, to devote considerable effort to making the case that will persuade a skeptic of the superiority of the proffered redescription over the accepted one. One of the significant failings of many of the rational choice theories Green and I discussed is that their proponents fail to do this. They offered problematizing redescriptions that were sometimes arrestingly radical, but their failure to take the second step made them unconvincing to all except those who agreed with them in advance.

VI. CONCLUDING COMMENTS

The recent emphases in political science on modeling for its own sake and on decisive predictive tests both give short shrift to the value of problematizing redescription in the study of politics. It is intrinsically worthwhile to unmask an accepted depiction as inadequate and to make a convincing case for an alternative as more apt. Just because observation is inescapably theory laden for the reasons explored in this essay, political theorists have an ongoing role to play in exhibiting what is at stake in accepted depictions of reality and reinterpreting what is known so as to put new problems onto the research agenda. This is important for scientific reasons when accepted descriptions are both faulty and influential in the conduct of social science. It is important for political reasons when the faulty understandings shape politics outside the academy.

If the problems thus placed on the agenda are difficult to study by means of theories and methods that are currently in vogue, an additional task arises that is no less important: to keep them there and challenge the ingenuity of scholars who are sufficiently open-minded to devise creative ways of grappling with them. It is important for political theorists to throw their weight against the powerful forces that entice scholars to embroider fashionable theories and massage methods in which they are professionally invested while failing to illuminate the world of politics. They should remind each generation of scholars that unless problems are identified in ways that are both theoretically illuminating and convincingly intelligible to outsiders, nothing that they say about them is likely to persuade anyone who stands in need of persuasion. Perhaps they will enjoy professional success of a sort but at the price of trivializing their discipline and what one hopes is their vocation.

NOTES

1. See Donald Green and Ian Shapiro, *Pathologies of Rational Choice Theory: A Critique of Applications in Political Science* (New Haven, CT: Yale University Press, 1994); idem, "Pathologies Revisited: Reflections on Our Critics," in *The Rational Choice Controversy: Economic Models of Politics Reconsidered*, ed. Jeffrey Friedman (New Haven, CT: Yale University Press, 1996), 235-76.
2. John Nash, "The Bargaining Problem," *Econometrica* 18 (1950): 155-62. For explication, see John Harsanyi, "Advances in Understanding Rational Behavior," in *Rational Choice*, ed. Jon Elster (New York: New York University Press, 1986), 92-94.
3. See Charles Murray, *Losing Ground—American Social Policy, 1950-1980* (New York: Basic Books, 1984).
4. Michael B. Katz, *The Undeserving Poor: From the War on Poverty to the War on Welfare* (New York: Pantheon, 1989), 151-56.
5. Charles Murray, "Does Welfare Bring More Babies?" *The Public Interest*, spring 1994, 25.
6. John Roemer, "Does Democracy Engender Justice?" in *Democracy's Value*, ed. Ian Shapiro and Casiano Hacker-Cordón (Cambridge, UK: Cambridge University Press, 1999), 56-68.
7. It should be noted, however, that the median voter theorem is eminently debatable empirically. For discussion, see Green and Shapiro, *Pathologies*, 146-78.
8. Kathleen Bawn, "Constructing 'Us': 'Ideology, Coalition Politics and False Consciousness,'" *American Journal of Political Science* 43, no. 2 (1999): 303-34.
9. Russell Hardin, *Liberalism, Constitutionalism, and Democracy* (Oxford, UK: Oxford University Press, 2000), 35, 86-88, 106, 114, 144, 285.
10. I leave aside, for present purposes, how convincing the debatable conjectures are. Consider the great difficulties Republican candidates face in forging winning coalitions in American politics that keep both social and libertarian conservatives on board. If one were to set out to define a blueprint to put together a winning coalition, trying to fashion it out of these conflicting elements scarcely seems like a logical place to start. Likewise with constitutions viewed as coordinating devices, the many veto points in the American constitutional system could just as arguably be said to be obstacles to coordination. See George Tsebelis, *Veto Players: How Political Institutions Work* (Princeton, NJ: Princeton University Press, 2002). This might not seem problematic if one takes the view, as Hardin does, that the central purpose of the U.S. Constitution is to facilitate commerce. On such a view, institutional sclerosis might arguably be an advantage, limiting government's capacity to interfere with the economy. The difficulty with going that route is that we then have a theory for all seasons: constitutions lacking multiple veto points facilitate political coordination, while those containing them facilitate coordination in realms that might otherwise be interfered with by politicians. Certainly nothing in Hardin's argument accounts for why some constitutions facilitate more coordination of a particular kind than do others.
11. Milton Friedman, "The Methodology of Positive Economics," in *Essays in Positive Economics* (Oxford, UK: Oxford University Press, 1953).
12. Alasdair MacIntyre, *After Virtue* (Notre Dame, IN: University of Notre Dame Press, 1984), 88-108.
13. See Courtney Jung and Ian Shapiro, "South Africa's Negotiated Transition: Democracy, Opposition, and the New Constitutional Order," *Politics and Society* 23 (1995): 269-308.
14. What is necessary in the context of one problem may, of course, be contingent in another. When we postulate that it was necessary that Arafat be strong enough to marginalize the radicals

on his flank if he were to make an agreement with Barak, we do not mean to deny that his relative strength in this regard was dependent on many contingent factors. For further discussion, see Courtney Jung, Ellen Lust-Okar, and Ian Shapiro, "Problems and Prospects for Democratic Transitions: South Africa as a Model for the Middle East and Northern Ireland?" (mimeo, Yale University, 2002). In some ultimate—if uninteresting—sense, everything social scientists study is contingent on factors such as that the possibility of life on Earth not be destroyed due to a collision with a giant meteor. To be intelligible, the search for lawlike generalizations must be couched in "if . . . then" statements that make reference, however implicitly, to the problem under study.

15. Adam Przeworski, Michael Alvarez, Jose Cheibub, and Fernando Limongi, *Democracy and Development: Political Institutions and Well-Being in the World, 1950-1990* (Cambridge, UK: Cambridge University Press, 2000), 106-17.

16. For Przeworski et al.'s discussion of other explanatory variables, see *ibid.*, 122-37.

17. Alexis de Tocqueville, *Democracy in America*, ed. J. P. Mayer, trans. George Lawrence (New York: Harper Perennial, 1966).

18. Seymour Martin Lipset, "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *American Political Science Review* 53 (1959): 69-105.

19. Barrington Moore, *The Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston: Beacon, 1966), 413-32; Dietrich Rueschmeyer, Evelyne Huber Stephens, and John D. Stephens, *Capitalist Development and Democracy* (Oxford, UK: Polity, 1992).

20. See Adam Przeworski, *Democracy and the Market* (Cambridge, UK: Cambridge University Press, 1991), ix-xii, 1-9, 51-99; Przeworski et al., *Democracy and Development*, 78-106; Samuel P. Huntington, *The Third Wave: Democratization in the Late Twentieth Century* (Norman: University of Oklahoma Press, 1991), 3-18; Ian Shapiro, *Democracy's Place* (Ithaca, NY: Cornell University Press, 1996), 79-108.

21. Perhaps one could develop such a theory but only of an exceedingly general kind such as "holes are created when something takes material content out of something else." This would not be of much help in understanding or predicting anything worth knowing about holes.

22. See the various postmortem papers in the March 2001 issue of *PS: Political Science and Politics*, vol. 34, no. 1, pp. 9-58.

23. For discussion of the difficulties with prediction in open systems, see Roy Bhaskar, *A Realist Theory of Science* (Sussex, UK: Harvester Wheatsheaf, 1975), 63-142; *idem*, *The Possibility of Naturalism* (Sussex, UK: Harvester Wheatsheaf, 1979), 158-69.

24. Alan Gerber and Donald Green, "Do Phone Calls Increase Voter Turnout? A Field Experiment," *Public Opinion Quarterly* 65 (2001): 75-85; *idem*, "Reclaiming the Experimental Tradition in Political Science" in *Political Science: The State of the Discipline*, 3d ed., ed. Ira Katznelson and Helen Milner (forthcoming).

25. For discussion of an analogous phenomenon that plagues normative debates in political theory, see Ian Shapiro, "Gross Concepts in Political Argument," *Political Theory* 17, no. 1 (1989): 51-76.

26. Charles Taylor, "Neutrality in Political Science," in *Philosophical Papers II: Philosophy and the Human Sciences*, ed. Charles Taylor (Cambridge, UK: Cambridge University Press, 1985), 90.

27. See Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt Brace, 1955); Rogers Smith, *Civic Ideals: Changing Conceptions of Citizenship in US Law* (New Haven, CT: Yale University Press, 1995).

28. Indeed, Smith's argument turns out to be the tip of an iceberg in debunking misleading orthodoxy about American exceptionalism. Eric Foner has shown that its assumptions about

Europe are no less questionable than its assumptions about the United States. See Eric Foner, "Why Is There No Socialism in the United States?" *History Workshop Journal* 17 (1984): 57-80.

29. Philippe Schmitter, "Still the Century of Corporatism?" *Review of Politics* 36, no. 1 (1974): 85-121; Leo Panitch, "The Development of Corporatism in Liberal Democracies," *Comparative Political Studies*, April 1977, 61-90.

30. It turns out that joint legislation writing is a small part of the story. What often matters more is ongoing tripartite consultation about public policy and mutual adjustment of legislation/macroeconomic policy and "private" but quasi-public policy (e.g., wage increases, or multiemployer pension and health plans). There is also the formalized inclusion of private interest representatives in the administration and implementation process where de facto legislation and common law-like adjudication takes place. The extent of their influence in the political process varies from country to country and even industry to industry, but the overall picture is a far cry from the standard pluralist account.

31. For a discussion of the dangers of convergent thinking, see Charles E. Lindblom, *Inquiry and Change: The Troubled Attempt to Understand and Shape Society* (New Haven, CT: Yale University Press, 1990), 118-32.

32. For a discussion of differences between these models, see Ian Shapiro, "The State of Democratic Theory," in *Political Science: The State of the Discipline*, 3d ed., ed. Ira Katznelson and Helen Milner (forthcoming).

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