The Flight from Reality
in the Human Sciences

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Fear of Not Flying

IN MEDIEVAL ENGLAND there was a curious gap between the study and practice of law. From the thirteenth century to the seventeenth, the main language used for pleading in common law courts was Law French. It seems to have developed because Latin, the language of formal records, carried too much historical freight from Roman law for the peculiarities of English circumstances, whereas medieval English was insufficiently standardized for official use. Law French was a hybrid dialect, owing more to Picard and Angevin influences than to Norman French, in which French vocabulary was combined with the rules of English grammar. The common lawyers developed it for their pleadings in the courts, passing it down from generation to generation.[1]

This evolving vernacular of the common law courts had little, if any, impact on the academic study of law. Latin was the language of jurisprudence in Oxford and Cambridge, and, although Law French could reportedly be learned in ten days or fewer, the professors of jurisprudence appear not to have thought it worth their while. This might have been, as Fortescue said, because Latin was the language of all scientific instruction.[2] It might have been, as Blackstone claimed, because the civil law—taught and studied in Latin—was embraced in the universities and monasteries after the Norman Conquest but resisted in the

[1] See J. H. Baker, "The Three Languages of the Common Law," *McGill Law Journal*, Vol. 43 (January 1998), pp. 5–24.

[2] Sir John Fortescue, *De Laudibus Legum Anglie*, S. B. Chrimes, ed. (Cambridge, UK: Cambridge University Press, 1942 [c. 1460]), p. 114.

courts.[3] It might have been, as contemporary historians such as J. H. Baker maintain, because English Law was thought insufficiently cosmopolitan to merit serious study.[4] Whatever the reason, English jurisprudence developed in literal ignorance of the practice of English law.

A comparable disjunction afflicts the human sciences today. In discipline after discipline, the flight from reality has been so complete that the academics have all but lost sight of what they claim is their object of study. This goes for the quantitative and formally oriented social sciences that are principally geared toward causal explanation. Following economics, they have modeled themselves on physics—or at any rate on a stylized version of what is often said to go on in physics. But it also goes for many of the more interpretive endeavors that have been influenced by fashions in the humanities—particularly the linguistic turn in philosophy and developments in literary hermeneutics. Practitioners in these fields often see themselves as engaged in interpretation rather than explanation, thereby perpetuating a false dichotomy. Hence my use of the term *human sciences* here to encapsulate both endeavors. This book is my attempt to chronicle the extent of their flight from reality, and to combat it.

I should say at the outset that I do not believe the flight from reality has a single source or cause. It results, rather, from various developments that share elective affinities—developments that all too often are mutually reinforcing. Some of their sources are intellectual, having to do with the ebb and flow of academic fashion. Some of them are institutional, reflecting the structure of academic professions and the incentives for advancement in an era of exhausted paradigms and extensive specialization. This can be bolstered by a perverse sense of rigor, where the dread of being thought insufficiently scientific spawns a fear of not flying among young scholars. Some are political in the broadest sense, having to do with the relations between disengaged human sciences and the reproduction of the social and political order. The flight from reality is not without consequences *for* reality as we will see. At best it marginalizes the potential effects of political and social criticism, and sometimes it contributes to the maintenance of oppressive social relations—however unwittingly.

[3] William Blackstone, *Commentaries on the Laws of England* (Chicago: University of Chicago Press, 1979 [1765]), Vol. 1, pp. 16–32.

[4] In correspondence.

I begin making this case, with the help of Alexander Wendt, in the opening chapter. We expose the limitations of empiricist and interpretive methods of social research, showing how they bias the enterprise in method-driven ways, and we argue for a realist view in their stead. Rather than do this in the abstract, we pursue it by reference to a concrete phenomenon that has attracted a good deal of attention in the human sciences: the study of consent. Empiricism as we describe it here encompasses two different approaches to social inquiry—both bastard stepchildren of David Hume. The first we dub *logicism* to call attention to the fact that its proponents embrace the view, made famous by Carl Hempel, that good explanations are sound deductive arguments. For logicists, an hypothesis is scientific only if it is derived from a general theory. Such theories often rest on simplifying assumptions about reality, or even "as if" assumptions that are not valid empirically at all.

It is conventional to defend this practice on the grounds that these theories do a good predictive job in accounting for empirical reality. This might sound reasonable in principle, but in practice logicists often formulate their claims so generally that they turn out to be compatible with all possible empirical results—in effect rendering the empirical world epiphenomenal to the theory. We show how this vitiates the study of consent in practice, when theorists of rational consent have sought to explain away apparent anomalies by concocting redescriptions that render them compatible with their preferred theories. This was understandable with theorists of the early Enlightenment because, as I have argued elsewhere, they embraced the view that certainty is the hallmark of science—making the toleration of counterexamples unacceptable.[5] However, mature Enlightenment views of science assume knowledge generally to be corrigible, with the implication that scientists will not take hypotheses seriously if they cannot be falsified—and, indeed, if the conditions under which they will be rejected cannot be specified in advance. The irony, then, is that although contemporary logicists often like to think of themselves as the only genuinely rigorous practitioners of social science—"if you ain't got a theorem you ain't got shit!" as a partisan of this view once put it to me—in reality they are wedded a view of science that most practicing scientists have not taken seriously for centuries.

[5] Ian Shapiro, *The Moral Foundations of Politics* (New Haven: Yale University Press, 2003), chap. 1.

Hume's other bastard stepchildren discussed in chapter 1 are empiricists of a particular stamp—those who became known in the 1950s and 1960s as behavioralists. Often skeptical of the breathtaking theoretical ambition characteristic of logicists, these empiricists were partisans of Hume's insistence that knowledge is grounded in observation of events and that causal knowledge inheres in observing their constant conjunction. Whereas the logicist derives comfort from the certainty that seems to inhere in the deductive relations between premises and conclusions, the Humean empiricist looks to observation for reassurance. We show how, in the political science power literature, this biased research away from attending to factors that coerce people into apparently consensual behavior—whether by surreptitious manipulation of agendas, structuring people's perceptions of alternatives, or even shaping their preferences. The focus on observed behavior was conceived as a corrective to elite theories of politics put forward by Gaetano Mosca, Robert Michels, Vilfredo Pareto, and C. Wright Mills. They had been cavalier in their treatment of observable behavior, ignoring it or explaining away any tensions between it and what Michels described as the "iron law" of oligarchy. Ironically, the behavioralists ended up with a different kind of method-drivenness, one in which the realm of observed behavior was assumed to be the only pertinent realm in accounting for consent.

If behavioralists bias research in favor of the phenomenal realm in one way, partisans of interpretation do it in another. By interpretivism I mean the research agenda that came into vogue in the human sciences during the 1970s, largely because of dissatisfaction with various failed reductionist enterprises. Prominent among these was Marxism, by then famous for its inability to account for the major political developments of the twentieth century. Instead of spawning revolutionary socialist proletariats, the advanced capitalist countries had experienced tenacious nationalism and working-class conservatism—not to mention the rise of Nazism in Germany and Fascism in Italy. Communists who did come to power either did so in peasant societies such as Russia and China contra Marx's prognostications, or they were forcibly imposed by the Soviets after World War II. In any case, by the 1950s it was obvious that, at best, communism as practiced rested on grotesque distortions of Marx's principles.

Attempts to rescue Marxism from an unhelpfully recalcitrant reality produced more noise than light over the succeeding decades. One motivation for those who found the interpretive turn attractive was to get away from the sectarian bickering over how to save Marx's materialism—even when it was conceded that this could operate only "in the last instance."[6] The interpretive turn involves treating articulated beliefs and ideas as elemental to human interaction. They are seen not as part of an epiphenomenal superstructure, to be understood, however circuitously, by reference to its links with the "underlying" material base. Rather, to use one of the buzz words of the day, they constitute reality—or at least human social reality—through language. Social reality is linguistic reality on this view. When human beings do things like create obligations or social contracts they do this through language, not by some other means that is then described by language. Understanding social reality means understanding the linguistic processes that give rise to it.

The interpretive turn thus went hand in glove with the ascent of ordinary language philosophy associated with the later Wittgenstein and J. L. Austin in the 1950s and with developments in literary hermeneutics in which understanding social processes was modeled on the interpretation of texts.[7] It was but a small step from this to the view that society should be conceived of *as* a text, whose meaning is best recovered by exploring the web of linguistic conventions within which social agents operate as collective authors. We are locked within a prison-house of language, as Frederick Jameson colorfully put it, the implication being that it is better to try to understand linguistic reality from the inside than to indulge vain fantasies of escape.[8] Different theorists had different views of how such understanding is best achieved, but they all agreed that the point of the exercise is to elucidate social meanings by exploring the linguistic conventions—the language games, as Wittgenstein had it—within which people inevitably operate. Social reality arises out of conventional linguistic usage, and the key to understanding it lies in recovering the conventions so as to see how people use them to act in the social world.

[6] Louis Althusser's phrase in "Ideology and Ideological State Apparatuses," in *Lenin and Philosophy, and Other Essays*, Ben Brewster, trans. (New York: Monthly Review, 2001), pp. 127–88. See also "Contradiction and Overdetermination," in *For Marx* (London: Verso, [1965] 1996), pp. 87–128. For a flavor of the polemics, see E. P. Thompson, "The Poverty of Theory or an Orrery of Errors," *The Poverty of Theory and Other Essays* (New York: Monthly Review Press, 1978), pp. 1–210.

[7] Ludwig Wittgenstein, *Philosophical Investigations* (Oxford: Blackwell, 1953) and J. L. Austin, *How to Do Things with Words* (Cambridge, MA: Harvard University Press, 1962).

[8] Frederick Jameson, *The Prison-House of Language: A Critical Account of Structuralism and Russian Formalism* (Princeton: Princeton University Press, 1974).

Elsewhere I have discussed the interpretative turn's impact on the historical study of political theory by examining the contextual theories of the Cambridge school—John Dunn, J.G.A. Pocock, and Quentin Skinner.[9] There is much to commend their approach to the study of the texts in the history of ideas. In particular, their insistence that contextual knowledge is essential to recover what an author meant to do in writing a text was an important corrective to prevalent methodologies that had assumed reading the text "over and over again" to be sufficient. Some of their contextual rereadings of particular authors are debatable and have been debated, but one would be hard pressed to dispute that important correctives to received interpretations have resulted from this scholarship. Skinner's rereadings of Hobbes have stood the test of time especially well—displacing a tired stereotype of him as "the monster of Malmesbury."[10] Dunn's relocation of Locke's political writings in the theological disputes that were his lifelong preoccupation have revolutionized Locke scholarship for a generation, and the careful contextual researches of Peter Laslett and Richard Ashcraft have established that the *Two Treatises of Government* were written the better part of a decade before the Glorious Revolution of 1688—rubbishing an older conventional wisdom that they were written to justify it.[11] Locke's contradictory views on slavery have received definitive illumination through the contextual analysis of James Farr.[12] Pocock's magisterial recovery of the civic humanist tradition has spawned a revival of interest in republican ideas, complicating, at least, our picture of liberalism's emergence and evolution.[13] This is to say nothing of the revisions of received interpretations of medieval and early modern natural law theory at the hands of Richard Tuck and James Tully, or the accounts of Adam Smith and David Ricardo's politics from Donald Winch, Shannon Stimson, and Murray Milgate.[14]

[9] Ian Shapiro, "Realism in the Study of the History of Ideas," *History of Political Thought*, Vol. 3, no. 3 (November, 1982), pp. 535–78.

[10] Quentin Skinner, "Thomas Hobbes and His Disciples in France and England," *Comparative Studies in Society and History*, (1965/1966) vol. 8, pp. 153–68, and "The Context of Hobbes's Theory of Political Obligation," in Maurice Carnston and R. S. Peters, ed., *Hobbes and Rousseau: A Collection of Critical Essays* (New York: Doubleday, 1972), pp. 109–42.

[11] John Dunn, *The Political Thought of John Locke* (Cambridge: Cambridge University Press, 1969); Peter Laslett's "Introduction" to John Locke, *Two Treatises of Government*, Peter Laslett, ed. (Cambridge: Cambridge University Press, 1988), pp. 61, 123–26; and Richard Ashcraft, "Revolutionary Politics and Locke's 'Two Treatises of Government': Radicalism and Lockean Political Theory," *Political Theory*, Vol. 8, No. 4 (November 1980), pp. 429–86, and *Locke's Two Treatises of Government* (London: Allen & Unwin, 1987).

[12] James Farr, " 'So Vile and Miserable an Estate': The Problem of Slavery in Locke's Political Thought," *Political Theory*, vol. 14, no. 2 (May 1986), pp. 263–89.

[13] J.G.A. Pocock, *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition* (Princeton: Princeton University Press, 1974). For a review essay of the civic humanist scholarship spawned by Pocock, see my "J.G.A. Pocock's Republicanism and Political Theory: A Critique and Reinterpretation," *Critical Review*, Vol. 4, No. 3 (Summer 1990), pp. 433–71.

It is one thing to say that understanding what an author was trying to do depends critically on recovering the context in which he was writing; quite another to turn this into an a theory of politics and political change. It is this vastly more ambitious agenda, most self-consciously articulated by Quentin Skinner, with which I take issue. I agree with Skinner that any plausible account of political reality must take account of the role political ideas play in shaping it. But making this move inevitably puts large causal questions on the table about what ideologies are, how they shape and are shaped by political conflict and change, and how—if at all—they might be related to the ideas of political theorists.

Skinner ducks these questions by eschewing all causal analysis in favor of the "interpretation," but I argue that in effect this means he does his causal analysis behind his back, which he insists that we should not. He equates the meaning of a text with what an author intended to convey, and he gets at this by seeing how the author's ideas were received by his intended audience. But this overlooks gamuts of relevant possibilities once we are studying their ideas as ideologies. What people overlook might be more important, ideologically, than what they discern. People might be misled, whether for malevolent or accidental reasons. They might supply inadvertent legitimation for practices that they perceive dimly, if at all. How people's ideas are appropriated or misappropriated by subsequent generations might be more important than their intentions as communicated to contemporaries. By assuming that an "internal" reading, geared to recovering authorial intention, is synonymous with studying the history of ideas as the history of ideologies, Skinner affirms a new—rather whiggish—reductionism without ever acknowledging it.

[14] Richard Tuck, *Natural Rights Theories: Their Origin and Development* (Cambridge: Cambridge University Press, 1979); James Tully, *A Discourse Concerning Property: John Locke and his Adversaries* (Cambridge: Cambridge University Press, 1980); Donald Winch, *Adam Smith's Politics: An Essay in Historiographic Revision* (Cambridge: Cambridge University Press, 1978); Murray Milgate and Shannon Stimson, *Ricardian Politics* (Princeton: Princeton University Press, 1991).

In contrast, I argue for openness to "external" readings. These are geared to locating subjective accounts in larger causal processes without prejudging what those processes might consist in, without deciding in advance whether and how much they might shape or be shaped by political interests, agendas, and events, and without assuming anything a priori about how—if at all—they might be subsumable into a general theory of politics. These are all subjects for research that cannot be settled before it begins. The scientific outlook requires a commitment to discovering what is actually going on in a given situation without prejudging what that is. Opting for the recovery of what a particular political theorist meant to say involves one of many possible cuts at accounting for ideology's role in politics. It has to be justified by comparison with the going plausible alternatives, not smuggled in by the backdoor under the guise of eschewing the world of causation for that of interpretation. Partisans of interpretation often see themselves as fundamentally at odds with behavioral social scientists. So it is ironic that they end up embracing a reductive view that makes them cousins of the behavioralists. Both rule out looking behind the world of appearances. This biases the study of consent by taking some of the most significant possibilities off the table before research begins.

My call for attention to the "external" dimensions of political action as well as the "internal" ones is embedded a realist view of science. It owes much to the work of Rom Harré, Roy Bhaskar, Richard Miller, and others, and I harbor no ambition to offer a full-blown defense of it here. Rather, my goal is to underscore the commitments that are embodied in the realist outlook, explain their significance for the conduct of social inquiry, and show why they should be expected to lead to better results than the going alternatives. To be sure, I mean to portray the realist outlook in an attractive light in these pages, but I try to do this more by illustrating its felicitous consequences for social and political inquiry than by arguing for it from the ground up. Some prefatory remarks are nonetheless in order here to indicate what I take to be involved in the commitment to scientific realism, and to differentiate it from doctrines with which it is sometimes confused.[15]

I take the core commitment of scientific realism to consist in the twofold conviction that the world consists of causal mechanisms that exist independently of our study—or even awareness—of them, and that the methods of science hold out the best possibility of our grasping their true character. Adherents to this view are sometimes characterized as "transcendental" realists.[16] This cumbersome and loaded term perhaps obscures more than it illuminates. I take it to underscore the fact that the realist commitment is implicit in the conduct of science, not a product of it. Unless scientists assumed it to be valid, as they generally do, they would have no good reason to see their enterprise as superior to religion, superstition, tradition, and other pretenders to authority in accounting for reality. This is not to say that the realist commitment implies fidelity to any particular theories or hypotheses about reality's causal structure. Rather, embracing the commitment is necessary for thinking it worthwhile to develop theories and hypotheses, and to evaluate them by reference to the methods of science.

Wendt and I show how, in the study of consent, a realist commitment opens up research agendas to the study of causal questions that are ruled out of court by the behavioral and interpretivist schools. Yet it does this without dismissing behavior and subjective understanding as epiphenomenal, or affirming a reductionist view that is impervious to the demands of evidence so characteristic of logicist ventures. We discuss John Gaventa's *Power and Powerlessness* as exemplifying social science conducted in a realist spirit, both in its attempt to illuminate opaque causal mechanisms that produce consent—"quiescence" is Gaventa's term—in circumstances of domination, and in explaining the relations between those mechanisms and the realms of subjective perception and behavior. From a realist perspective it thus becomes plain that behavioral and interpretive methods exemplify the flight from reality, even though their proponents often resist the ambition to develop general theory.

Perhaps as a reaction against the behavioral and interpretive hostility to general theory, logicist enterprises have won a new lease on life in recent decades. The main vehicle has been the import of microeconomic models into the noneconomic human sciences—notably to political science, sociology, and law—under the banner of rational choice theory. Donald Green and I explore this development as it relates to

[15] Philosophers and social theorists who consider themselves scientific realists differ on various particulars, and not every one of them shares my view on all particulars. Aspects of these differences are taken up in chapter 1, although for the most part they are not germane to the present volume. They are explored more fully in my *Political Criticism* (Berkeley, Los Angeles: University of California Press, 1990), chapter 8.

[16] The term is Roy Bhaskar's, as elucidated in *A Realist Theory of Science* (Sussex: Harvester, 1978) and *The Possibility of Naturalism* (Sussex: Harvester, 1979).

political science in chapter 2, by responding to critics of our book *Pathologies of Rational Choice Theory*.[17] Although logicist ventures are wanting from a realist perspective, we show here that they also collapse under their own weight—largely because of their quixotic theoretical ambition. Taken on their own terms, rational choice theories have, for the most part, degenerated into elaborate exercises geared toward saving universalist theory from discordant encounters with reality. Belying the fanfare about theoretical rigor that often accompanies their claims, we show how rational choice theorists play fast and loose with the definition of rationality in developing hypotheses, in specifying their empirical implications, and in testing them against the evidence.

The litany of failures that we identify includes elaborating sufficient accounts for political phenomena without showing how or why they should be preferred to the going alternatives; "explaining" stylized facts that turn out on close inspection not to bear much relationship to any political reality; post-hoc fiddling with theories in ways that amount to little more than thinly disguised curve-fitting; specifying theories so vaguely that they turn out to be compatible with all empirical outcomes; scouring the political landscape for confirming illustrations of the preferred theory while ignoring the rest of the data; and projecting evidence from the theory by coming up with tendentious descriptions of the political world. Even when rational choice theorists back away from pure universalist claims, they do so in ad-hoc and in unconvincing ways that reinforce their reluctance to entertain the possibility that their theory is incorrect. It is as if someone, on observing one day that red apples no longer fell toward the ground when dropped, asserted that the theory of gravity is fine; we must just accept that it does not apply to red apples.

As this example might suggest, when all else fails the universalist impulse leads rational choice theorists to take refuge in the philosophy of science. Accusing their critics of being "naïve falsificationists," they appeal to the arguments of Thomas Kuhn and Imre Lakatos in vindication of their procedures. Those who appeal to Kuhn seem innocent of his notorious inability to distinguish developing research paradigms, when knowledge is advancing, from decaying ones, when it is not. Given the failure of rational choice theorists to identify unambiguous advances in empirical knowledge, this is a serious worry. Indeed, as

[17] Donald P. Green and Ian Shapiro, *Pathologies of Rational Choice Theory: A Critique of Applications in Political Science* (New Haven: Yale University Press, 1994).

Kuhn was careful to insist, the human sciences have yet to reach the stage where there is a dominant paradigm within which normal science can proceed by puzzle-solving. Hence his description of them as "preparadigmatic." Those rational choice theorists who acknowledge that this is the true state of affairs sometimes see it as their mission to provide a remedy—as my discussion of David Laitin's agenda for political science in chapter 6 reveals. They would be well advised to take note of Kuhn's insistence that "I claim no therapy to assist the transformation of a proto-science to a science, nor do I suppose that anything of the sort is to be had."[18]

Rational choice theorists who appeal instead to Lakatos make heavy weather of the claim that you can't beat something with nothing; that theories are not falsified by being tested against "the facts," but rather when a better theory comes along—one that explains what was known before and then some. This is concededly a good account of what often happens in science, yet the difficulty we identify is that in political science rational choice theorists operate is if it applies to their critics but not to themselves. Their characteristic proclivity is to ignore previous scholarship on the topics they study, to create trivial null hypotheses when any are considered at all, and to translate existing knowledge into their preferred terminology rather than add to it. Rational choice theorists operate more like brief-writers for their universal theories, not Lakatosian scientists who try to add to the inherited stock of knowledge by developing theories that perform better empirically than those that have been tried before. If rational choice theorists took their Lakatosian protestations seriously, they would abandon the logicist impulses that flow from Hempel's theory of science and engage seriously in comparative empirical evaluation of their arguments against the most plausible going alternatives. We note that the few of them who have done this have, indeed, contributed to the study of politics. For most, however, the impulse to flee from problem-driven theory to method-driven theorem proves irresistible.

In chapters 3 and 4, I turn from the explanatory to the normative dimensions of the flight from reality. One way in which commentators have sought to link positive and normative theory is to try to derive principles for action and policy from their accounts of how the world

[18] Thomas S. Kuhn, "Reflections on My Critics," in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970), pp. 244–45.

works. Another is to try to intervene in the world to get it to conform to one's normative ideal better than presently it does. Richard Posner, a United States appellate court judge since his appointment to the Seventh Circuit by Ronald Reagan in 1981, engages in both. I provide an assessment of his efforts in chapter 3.

Getting from explanation to prescription means getting from is to ought. Posner's attempt to do this is elusive and ultimately unsuccessful, but it reveals one of the ways in which the flight from reality can have an impact on reality. Posner posits a functionalist account of the common law's evolution, according to which it operates over time to maximize economic efficiency. His is an invisible-hand account in that this process is alleged to occur beyond the ken of common-law judges, who typically do not perceive—let alone understand—the ways in which their decisions contribute to this result. Indeed, as if to underscore this point, Posner upbraids some judges who have sought to apply his theory of wealth-maximization in the course of adjudication. Yet Posner nonetheless delivers a series of nostrums about how judges should behave and proposes various reforms to the administration of courts in the United States based on appeals to his efficiency arguments.

Attention to Posner's invisible-hand account reveals good reasons for skepticism, but even if we bracket them it is far from clear that we should regard it as benign. Consider Posner's efficiency-based account of why the criminal law disproportionately punishes harmful acts committed by poor people.[19] Criminal sanctions become necessary, on his account, when the threat of compensation through the torts system fails to deter potential wrongdoers. Because poor people typically lack the resources to compensate victims that would be sufficient to deter potential perpetrators, the threat of incarceration is needed instead. This "efficiency" based account of a system that discriminates against the poor entails nothing about its moral attractiveness, unless supplemented by an argument showing efficiency to be more desirable than equitable treatment. This becomes obvious when we recall that scholars in the critical legal studies movement have argued for the same evolutionary thesis as Posner's, but to make a rather different normative point.[20] Claims about neutrality to the contrary notwithstanding, they argue, the law operates to sustain capitalist market relations. Oliver

[19] Richard Posner, "An Economic Theory of the Criminal Law," *Columbia Law Review*, Vol. 85, No. 6 (October 1985), pp. 1193–231.

[20] Roberto Unger, *The Critical Legal Studies Movement* (Cambridge, MA: Harvard University Press, 1986).

Wendell Holmes might have been right that the Fourteenth Amendment "does not enact Mr. Herbert Spencer's *Social Statics*," but they think that it creeps into the common law nonetheless.

The functionalist case does not get Posner from *is* to *ought* any more than it gets Unger and his colleagues from *is* to *ought not.* Nor does it exhaust the possibilities for pressing explanatory theory into the service of normative argument. Another strategy Posner deploys is to point to gaps between purposes he alleges to be immanent in the law and the reality on the ground, thereby supplying impetus to the suggestion that reality stands in need of reform. This is strategy is at least as old as Jeremy Bentham's market-failure theory of the need for government because of the possibility of free riding, tragedy-of-the-commons problems, and other by-products of selfish individual behavior.[21] A different variant was pursued by the legal realists, who pointed to yawning gaps between the professed ideals of American law and the brutal realities of criminal prosecution during the 1930s and after. This eventually spawned a wide range of reforms to criminal procedure and defendants' rights by the Warren Court, designed to bring reality into better conformity with professed constitutional ideals.

Posner's modus operandi is to identify inefficiencies in the law and the administration of the courts, thereby generating impetus for reforms oriented to bringing them into better conformity with his efficiency ideal. Passing over the difficulty, already noted, that it is far from clear that his efficiency ideal is in fact immanent in American law, I show how his appeals to efficiency all involve reifying contestable economic theories as "the" economic theory or the law. This enables Posner to mask a particular ideological agenda in the garb of abstract theory. For instance, close inspection reveals that Posner's unhappiness with the increase in litigation in federal courts in recent decades does not depend on his—or any—general theory of the optimal level of litigation or judicial services. Rather, it reflects a conservative antipathy for government.

These proclivities extend to Posner's activities on the bench. Through an analysis of his labor law and antitrust opinions in his first five years as a federal appellate court judge, I show that he imports his version of the *Social Statics* into judicial opinions in predictable ways, all the while portraying them as uncontroversial economic theory—if not unassailable common sense. The truth, as I show, is that different economic

[21] See Shapiro, *The Moral Foundations of Politics*, chapter 2.

theories than the ones Posner invokes would produce different results in these cases; yet he never supplies us with principled reasons for preferring the ones he advocates. The logicist's variant of the flight from reality involves misrepresenting the world by confusing it with dubious models that flow from a pet general theory. Posner's academic writing exemplifies this pathology at every turn. More troubling is that his position on the bench gives him the power to try to reshape reality in accordance with his models—to the arbitrary detriment or benefit of litigants who happen to have their cases appealed to his court.

Dramatically arresting as Posner's reification of efficiency might be, I argue in chapter 4 that he is scarcely alone in his general approach. Political theorists often fail to appreciate that any claim about how politics is to be organized is bound to be a relational claim involving agents, actions, legitimacy, and ends. If they did, they would resist many of the standard contending views in the controversies that preoccupy them. I demonstrate this by reference to contemporary debates concerning the nature of right, law, autonomy, utility, freedom, virtue, and justice. Rather than confront the complexities implicit in the relational logics of these and other political ideals, all too often political theorists appeal to *gross concepts*—my term for ideas that feed into and promote misleading dichotomies. By appealing to gross concepts, political theorists reduce what are actually relational claims to claims about one or another of the terms in a relational argument. I explain how this systematically obscures the phenomena they purport to analyze, diverting attention from first-order questions about the world to second-order conceptual debates that can never be settled because they rest on category mistakes. Gerald MacCallum Jr. pointed this out a long time ago in connection with the arguments about liberty. He showed that debates between "negative" and "positive" libertarians are not really about kinds of freedom, as protagonists since Isaiah Berlin have supposed, but rather about *who* is free, *from what* constraints or *because of what* enabling conditions, *to perform which* actions.

Despite a good deal of ritualistic genuflecting to MacCallum in the literature, my analysis reveals that his reasoning has penetrated little. This is partly because taking it seriously would entail coming to grips with contentious empirical debates about human psychology and the causal structure of the social world—which theorists are loath to do. In arguments about freedom, as in numerous other controversies that exhibit a similar structure, gross concepts are instead presented as dichotomous alternatives. Protagonists on both sides defend their views

mainly by pointing to the demerits of the supposed alternative—about which they are invariably right. The result is to perpetuate debates that lurch among gross concepts without getting anywhere.

The activity thrives nonetheless, partly because it is rewarded by professional incentives in the academy but partly, I argue, because many people find gross concepts appealing. They are comforting and simplifying devices that function, as ideologies, to legitimate things that people want to believe. Recognizing the alluring power of gross concepts, I defend an account of the political theorist's vocation that revolves around resisting it. Our job is to reel in gross concepts, not to traffic in them. Rather than try to find the right gross concept to champion, we do better to operate as principled social critics whose goal is embellish political argument with political reality. We should be roving ombudsmen for the truth rather than partisans of any particular message. What this means for the conduct of political theorists within the political science discipline is explored in the final two chapters.

This question is taken up in chapter 5 in the context of a critique of the impulse toward, and rewards for, reductionist explanations in political science. My point of departure is the observation—often leveled at my argument with Green in *Pathologies*—that all observation is theory-laden. Because there is no theoretically unsullied account of "the facts," so the argument goes, we were naïve in supposing that any particular explanation can be evaluated simply by how well it does empirically. Agreeing that Green and I attended insufficiently to this question, I make the case here that it does not stand in the way of a view of the social-scientific enterprise geared to getting at the truth, that this is unlikely to be achieved by any of the going reductionist ventures in political science, and that the endemic availability of alternative descriptions of political reality creates an important ongoing responsibility for political theorists in the division of labor within political science.

The assertion that all observation is theory-laden turns out on close inspection to merit the retort that some types of theory-ladenness are nonetheless more plausible than others. Once the headlights are turned up on particular cases, it becomes plain that there is a world of difference between theory-driven description, in which tendentious accounts are projected onto the problems in order to vindicate pet theories, and situations where there are persuasive reasons, while taking account of previous attempts to study a problem and their limitations, for preferring an alternative construction of it. This is to say nothing of method-driven work, where the construction of the problem is contaminated by the methods available to the researcher. A study in which John Huber and Charles Shipan try to measure Congress's desire to confer discretion on administrative agencies by the number of words in the relevant statutes is easier to comprehend once one is reminded of the "word count" feature of modern word-processing programs.[22] No matter that it would seem to suggest that the countless reams of the Internal Revenue Code are there to limit the discretion of IRS agents. Try telling that to your accountant.

Moreover, whereas some disagreements about explanation are really disagreements about competing descriptions in drag, many are not. When a particular description *is* recognized as apt, there is then a truth of the matter as far as explanation is concerned—even if no one yet knows what it is. That is the realist presumption on which the conduct of all science, on my account, rests. The mistake is to suppose that explanations are scientific only when they flow from a single general theory, which then leads scholars to shoehorn the construction of every problem into terms that are compatible with it. Antireductionist priors, that build toward feasible generality rather than take it for granted prior to empirical research, may well involve descriptions that are theory-influenced. But they will not be theory-driven.

This is not to deny that significant challenges are posed for political science by the theory-ladenness literature. Different ways of characterizing social phenomena predispose researchers to reach for different explanatory arguments, raising questions about how appropriately to choose among them. The conventional answer is prediction. If we can better predict the ways in which judges will decide cases by looking at which outcome maximizes wealth rather than at their jurisprudential commitments, then we should opt for the efficiency-based characterization. But prediction is not all that it is cracked up to be. It can lead us to the misconception that playing basketball makes people taller. More consequentially, I show that because prediction is so difficult in the human sciences, excessive preoccupation with it can drive researchers to focus on trivial but tractable questions—three points to the right of the decimal. Alternatively, it can lead them endlessly to refine predictive instruments that are never going to work. The result? Immensely complicated clocks that neither tick nor tell the time.

[22] See John D. Huber and Charles Shipan, *Deliberate Discretion: Institutional Foundations of Bureaucratic Autonomy* (Cambridge: Cambridge University Press, 2002).

Political theorists have important roles to play in political science just because there is no algorithm that dictates the correct descriptive cut at the social world. Among our central tasks is to identify, criticize, and suggest plausible alternatives to the theoretical assumptions, interpretations of political conditions, and above all specification 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. And, especially when esoteric forms of redescription are involved, they must elucidate the links to more familiar understandings of politics.

If the study of politics is inherently contentious, how should it be taught to the next generation of students? This is the subject of my final chapter, a critique of David Laitin's recent proposal to standardize political science teaching to undergraduates. His goal is to do for political science what Samuelson did for economics by creating an orthodox curriculum, and he proposes a model syllabus for its introductory course. I make the case that Laitin's proposal is wrongheaded in ways that both reflect and encourage the flight from reality.

Laitin's proposal reflects the flight from reality by conflating the discipline with its object of study, calling to mind the economist's reputed answer to a student query that economics is "what economists do." Undergraduates typically become political science majors because they are interested in politics, not the political science discipline. It should be troubling if these students are presented, as Laitin believes that they should be, with an account of political science that is sanitized to obscure its controversial character. The impulse to do this has more to do with professional incentives to get grants and public recognition for the discipline, as Laitin comes close to conceding, than with any plausible account of how to teach students to think systematically about the fundamentals of politics. Even when scholars believe they have the One True Theory or the One Best Method—indeed, particularly in that case—they do students a disservice not to tell them about the others.

Laitin's proposal is liable to encourage the impulse to flee reality by detaching scholars from a particular kind of discipline that undergraduate teaching offers. His reasoning depends on the illusion that an initial political science course can get students up to speed in the same way that essentials are covered in introductions to physics, chemistry, and math. These disciplines differ from the social sciences in that there is little, if any, disagreement among their practitioners on what the appropriate descriptive cut on the world actually is. Against Laitin, I argue that the economists' decision to standardize their discipline as he recommends and would emulate has had the disastrous consequence of divorcing introductory teaching from controversies at research frontiers of their discipline. This famously alienates undergraduates from what they are required to study in introductory economics courses. It also reinforces the powerful disincentives for active research scholars to teach the introductory courses, turning them over instead to adjunct faculty and flunkies.

This is a loss for the students, but also for the researchers. Bright undergraduates often have noses for important problems and a refreshing desire to cut to the chase about what difference the theories they are being asked to understand make in the actual world. They are not hostage to academic literatures that may be little more than rotten boroughs in the ways that graduate students so often are. If scholars in political science were to free themselves from the discipline that comes with having to teach undergraduates as completely as economists have done in recent decades, they would have yet one more reason to succumb to their—already overdeveloped—fear of not flying.

CHAPTER ONE

The Difference That Realism Makes: Social Science and the Politics of Consent

Ian Shapiro and Alexander Wendt

All forms of social inquiry rest on beliefs about what counts as an explanation of social phenomena. Should explanations of social life be deduced from observable facts? Should they be grounded on peoples' self-understandings? Should they be based on whatever enables us to intervene with effect in the world? Most of the time, social scientists go about their research without worrying about these issues, which primarily interest philosophers of social science. This may be unavoidable and even desirable given the intellectual division of labor, but it becomes problematic when bad philosophical assumptions contaminate the conduct of social science.

Since the 1950s, philosophical debates about social explanation have been dominated by two mutually antagonistic schools of thought: logical empiricism and interpretivism. The former, which was dominant from the heyday of the Vienna Circle until the early 1960s, rested on the view that observation is theory-neutral and that the task of science is to come up with law-like generalizations about these observations. Interpretivism coalesced around a series of critiques of this view during the 1960s. Whether inspired by Thomas Kuhn's work on conventionalism in the history of science or by the linguistic turn in philosophy and literary theory, interpretivists share the belief that "all observation is theory-laden" and that science therefore cannot be the objective inquiry that logical empiricists took it to be.

In recent decades, the terms of this debate have been challenged by a group of self-styled scientific realists. Realists contend that theory-ladenness is a matter of degree and that the fact that observation is

CHAPTER FIVE

Problems, Methods, and Theories in the Study of Politics: Or, What's Wrong With Political Science and What to Do about It

OUR MANDATE is to engage in navel-gazing about the condition of political theory.[1] 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 occur. In many physical, biomedical, and informational sciences, the benefits are visible in expanding bodies of knowledge that were scarcely conceivable a generation ago. Specialization also has proceeded apace in the human sciences, seen in the proliferation of dedicated journals, professional organizations and sub-organizations, 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 understand—let alone evaluate—the alleged contributions of the new specialized fields.

[1] Originally written for the thirtieth anniversary issue of *Political Theory* in which all contributors were asked to address the question: "What Is Political Theory?" Vol. 30, No. 4 (August 2002), pp. 596–619.

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, 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. 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 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, not sufficiently problem-driven.[2] 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

[2] See Donald Green and Ian Shapiro, *Pathologies of Rational Choice Theory: A Critique of Applications in Political Science* (New Haven: Yale University Press, 1994) and chapter 2 in this volume.

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 specifications 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 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.[3] We registered skepticism at this approach in our book, partly because the ratio of success 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 leads to a discussion of the fact 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 the second half of the chapter. 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 political scientists to study trivial, if tractable, problems. In light of prediction's limitations, I turn to a discussion of 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

[3] John F. Nash Jr., "The Bargaining Problem," *Econometrica*, Vol. 18, No. 2 (April 1950), pp. 155–62. For explication, see John Harsanyi, "Advances in Understanding Rational Behavior," in Jon Elster, ed., *Rational Choice* (New York: New York University Press, 1986), pp. 92–94.

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.

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 transitions 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 completely 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 specifies problems, yet nonetheless insist that the 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 (AFDC) and related benefits.[4] 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.[5] Murray's response (in support of which he cited no evidence) is hard to read with a straight face: "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."[6] This is manifestly 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 two-party democracies with substantial ex-ante inequality.[7] He develops a model which 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. Roemer's 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 in order to account for the dearth of redistributive platforms seems therefore to be worthwhile.[8]

[4] See Charles Murray, *Losing Ground: American Social Policy, 1950–1980* (New York: Basic Books, 1984).

[5] Michael B. Katz, *The Undeserving Poor: From the War on Poverty to the War on Welfare* (New York: Pantheon, 1989), pp. 151–56.

[6] Charles Murray, "Does Welfare Bring More Babies?" *The Public Interest* (Spring 1994), p. 25.

[7] John Roemer, "Does Democracy Engender Justice?" in Ian Shapiro and Casiano Hacker-Cordón, ed., *Democracy's Value* (Cambridge, UK: Cambridge University Press, 1999), pp. 56–68.

[8] It should be noted, however, that the median voter theorem is eminently debatable empirically. For discussion, see Green and Shapiro, *Pathologies*, pp. 146–78.

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 has been independently specified, then it is unlikely to gain much purchase on what is actually going on. Rather, it will be a strained and unconvincing specification driven by the impulse to save the pet theory. 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.

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 independently 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 pre-theoretical way of demarcating the problem. But we have known at least 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 political platforms 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 pre-theoretical 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: there is no 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 woman have increasing numbers of babies in order 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 coalition[9] or Russell Hardin's claim that constitutions exist to solve coordination problems.[10] 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.[11]

[9] Kathleen Bawn, "Constructing 'Us': Ideology, Coalition Politics and False Consciousness," *American Journal of Political Science*, Vol. 43, No. 2 (April 1999), pp. 303–34.

[10] Russell Hardin, *Liberalism, Constitutionalism, and Democracy* (Oxford and New York: Oxford University Press, 2000), pp. 35, 86–88, 106, 114, 144, 285.

[11] 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

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 form: Trees exist in order 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 lampposts 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?" or "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

to be obstacles to coordination. See George Tsebelis, *Veto Players: How Political Institutions Work* (Princeton: 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.

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.

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
- 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 sociobiological 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 prejudges the question I want to highlight: whether some descriptions are invariably more basic that 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 worry 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 groups 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 groups 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 earlier, but 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 looks for 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. 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 differentiate 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 grappled with 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.

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 if it is 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.[12]

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 potentially disconfirming 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 because of the existence of free will.[13] 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*

[12] Milton Friedman, "The Methodology of Positive Economics," in *Essays in Positive Economics* (Oxford, UK: Oxford University Press, 1953).

[13] Alasdair MacIntyre, *After Virtue* (Notre Dame, IN: University of Notre Dame Press, 1984), pp. 88–108.

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 social life consists of one damn thing after another. 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 have developed an account of the conditions that make negotiated settlements to civil wars possible. It depends on such factors as whether government reformers and opposition moderates can combine to marginalize reactionaries and revolutionary militants on their flanks. We have also developed an account 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.[14] 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 Gerry Adams emerged in the mid-1990s but the other necessary pieces were not in place, or when Yasir Arafat was offered a deal by Ehud Barak at a time when he was 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.[15]

[14] See Courtney Jung and Ian Shapiro, "South Africa's Negotiated Transition: Democracy, Opposition, and the New Constitutional Order," *Politics and Society*, Vol. 23, No. 2 (September 1995), pp. 269–308.

[15] 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 was to make an agreement with Barak in 2000, we do not mean to deny that his relative strength in this regard was dependent on many contingent factors. For further discussion,

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.[16] 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.[17]

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 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.[18] For Seymour Martin

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?" *Politics and Society*, Vol. 33, no. 2 (July 2005). 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 because of 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.

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

[17] For Przeworski et al.'s discussion of other explanatory variables, see *ibid.*, pp. 122–37.

[18] Alexis de Tocqueville, *Democracy in America*, J. P. Mayer, ed., and George Lawrence, trans. (New York: Harper Perennial, 1966 [1832]).

Lipset, it was a byproduct of modernization.[19] Barrington Moore identified the emergence of a bourgeoisie as critical, whereas Rueschemeyer, Stephens, and Stephens held the presence of an organized working class to be decisive.[20] 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).[21]

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.[22] 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.

[19] Seymour Martin Lipset, "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *American Political Science Review*, Vol. 53, No. 1 (March 1959), pp. 69–105.

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

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

[22] 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.

Reflecting on this example raises the possibility I want to consider next: that 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 datasets 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 where all of the standard models predicted a decisive Gore victory.[23] 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 meant that they continued 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 waiting out 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 observations misses my point here: that the main impetus for the exercise appears to be the

[23] See the various postmortem papers in the March 2001 issue of *PS: Political Science and Politics*, Vol. 34, No. 1, pp. 9–58.

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 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 being 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 perhaps 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 undertake experimental work where parameters can be controlled and key variables more easily isolated.[24] 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 difficulties Donald Green and Alan Gerber have revived the practice of field-experiments, where subjects can be randomized, experimental controls can be introduced, and questions about external validity disappear.[25] 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. 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.

[24] For discussion of the difficulties with prediction in open systems, see Roy Bhaskar, *A Realist Theory of Science* (Sussex: Harvester Wheatsheaf, 1975), pp. 63–142, and *The Possibility of Naturalism* (Sussex: Harvester Wheatsheaf, 1979), pp. 158–69.

[25] Alan Gerber and Donald Green, "Do Phone Calls Increase Voter Turnout? A Field Experiment," *Public Opinion Quarterly*, Vol. 65, No. 1 (Spring 2001), and "Reclaiming the Experimental

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 institutional choices 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
- 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

Tradition in Political Science," in Ira Katznelson and Helen Milner, eds., *Political Science: The State of the Discipline*, 3rd ed. (Washington, DC: American Political Science Association, 2002).

for protagonists to compare the inadequacies of one method with the adequacies of a second, and then declare the first to be wanting.[26] 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. 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 that "Catholics in Detroit vote Democrat."[27] As a result, the mainstream of political science that they came to define seemed to others to be both utterly devoid of theoretical ambition and detached from consequential questions of politics; frankly, boring. To paraphrase Kant, theoretical ambition without empirical research may well be empty, but empirical research without theoretical ambition will be blind.

[26] For discussion of an analogous phenomenon that plagues normative debates in political theory, see chapter 4 in this volume.

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

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.[28] 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 liberal tradition.[29] 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 mis-specified that the right causal questions were not even on the table.

[28] See Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt Brace, 1955); and Rogers Smith, *Civic Ideals: Changing Conceptions of Citizenship in U.S. Law* (New Haven: Yale University Press, 1995).

[29] 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 Foner, "Why Is There No Socialism in the United States?" *History Workshop Journal*, Vol. 17 (1984), pp. 57–80.

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.[30] The questions that then motivated the next generation of research became: 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.[31]

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 earlier than the multiple descriptions problem. 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. Doing this involves exhibiting their presuppositions, assessing their

[30] Philippe Schmitter, "Still the Century of Corporatism?" *Review of Politics*, Vol. 36, No. 1 (1974), pp. 85–121; and Leo Panitch, "The Development of Corporatism in Liberal Democracies," *Comparative Political Studies*, Vol. 10, No. 1 (1977), pp. 61–90.

[31] It turns out that joint legislation-writing is a small part of the story. What often matters more is ongoing tri-partite consultation about public policy and mutual adjustment of macroeconomic policy and "private" but quasi-public policy (e.g., wage increases, or multi-employer 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 take 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.

plausibility, and proposing alternatives when they are found wanting. This activity is particularly important when the defective account is widely accepted outside the academy. If political science has a constructive role to play outside the academy, it must surely include debunking myths and misunderstandings that shape political practice.[32]

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.[33]

Both *global apartheid* and *collusion in restraint of democracy* here are instances of problematizing redescriptions. 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. Redescribing the Westphalian system as based on a norm of global Apartheid, or political

[32] 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: Yale University Press, 1990), pp. 118–32.

[33] For discussion of differences between these models, see my *The State of Democratic Theory* (Princeton: Princeton University Press, 2003), pp. 59–62, 108–9, 112–49.

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 plausible. 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 at the start of this chapter. 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 that Green and I discussed is that their proponents failed to do this. They offered problematizing redescriptions that were sometimes arrestingly radical, but their reluctance or inability to take the second step made them unconvincing to all except those who agreed with them in advance. This is in notable contrast to Gaventa's redescription of apparently consensual behavior among his Appalachian mineworkers as quiescence discussed in chapter 1. Much of the credibility of Gaventa's account can be traced to his efforts plausibly to link his redescription to the commonsense understandings of the participants, as well as to previous characterizations of such behavior in the power literature.

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 chapter, 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.