Reconnecting Political Theory to Empirical Inquiry, or, A Return to the Cave?

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Introduction: A Shift Afoot?1

The organizers of the APSA Theme Panel for which this essay was first drafted were intrigued, I believe, by the possibility that something new is happening in political theory as practiced in political science departments. That “something” was thought to be a new emphasis on doing political theory in ways that engage work in empirical social science, including quantitative, historical, institutional, and policy analyses, more deeply and pervasively than was customary from roughly 1970 through 1995.2 Though I was not given any specific mandate in writing on this topic, I suspect the panel’s organizers hoped that I would document this shift, explain it, and, especially, defend it. I will do some of each, though perhaps not as much of any, particularly the last, as some may reasonably have anticipated. My thesis is that a variety of factors are indeed prodding scholars to try to do political theory in a more empirically engaged way now, and that this is a salutary development, but with three caveats. First, we must acknowledge that as yet this shift is much more a matter of rhetorical aspiration than achievement.3 Second, it is also true that engaging empirical work is not necessarily the same as engaging important, substantive political problems; and though many of us would like to see scholars do both, for me it is the latter that is most crucial.4 Finally, any celebration of this shift should not go so far as to cause us to lose sight of the many contributions more sweeping “grand theories” can provide, even though such theorizing is not likely to provide all that its practitioners have often hoped to achieve.

More specifically, I speculate that the causes of recent changes are the decline of the intellectual and, especially, the intensely conflictual political circumstances that prompted revivals of grand theory in the 1960s and early 1970s. These were times when to many it seemed necessary to abstract from current political
struggles to make any progress in normative discussions at all, and when it also seemed that there might be intellectually powerful new ways of doing so. The main reason it seems wise for political theory now to move away from the resulting “grand theory” phase is that in today’s less overtly conflictual but also less hopeful intellectual and political climate, efforts at grand theorizing hinder more than they help to address contemporary political problems.

Still, for at least three reasons, it seems important not to throw grand theorizing out with the holy water completely. First, the study of grand theories remains indispensable for keeping us aware both of the founding assumptions embedded in our prevailing institutions and practices and our empirical accounts of them, and of radically alternative ways of understanding and pursuing human lives—thus strengthening our capacities to ask probing empirical and normative questions about our politics. Second, precisely because grand theories represent a certain sort of response to particular intellectual and political contexts, their study can help us grasp the contextual problems that gave rise to them more fully, sometimes producing insights that help us better understand our own. Finally, it is possible that soon certain sorts of “grand theory” efforts will again become useful, perhaps indispensable means for helping us to envision new ways that we might conduct our shared lives in a century that promises further dramatic changes. We have learned since the original presentation of this paper that, though American domestic politics may appear less ideologically polarized than at some points in our past, the world hardly lacks deep divisions that can trigger violent conflicts around the globe. Those conflicts and efforts to combat them may eventually substantially alter the global political landscape. And under the impact of far-reaching cybernetic and genetic engineering innovations and other technological transformations, human political societies, political processes, and shared lives may well be extraordinarily different at the beginning of the twenty-second century than they are at the beginning of the twenty-first. Though it is wise for both scholars of “great works” and those striving to write new ones to be more empirically engaged and more concerned with important concrete problems than many recent ones have been, political theorists may nonetheless be well advised to seek to “imagine communities” in ways quite different than they have done heretofore.

Has Anything Changed?

In regard to documenting the claim that “something may be happening,” I hope it will be sufficient to provide some suggestive examples—for if there is any movement at all in the direction I describe, it is worth exploring, even if it is not a sea change. But first, we need to be aware of an important distinction. Our subject here is chiefly the work of people professionally designated “political theorists” in
political science departments, though their work cannot be discussed without reference to other sorts of “theorists” to whom they attend closely. These include scholars in the fields of philosophy (usually called “political philosophers”—think John Rawls and Richard Rorty); in sociology (usually called “social theorists”—think Pierre Bourdieu and Jürgen Habermas); in law (usually called “legal and political philosophers”—think Ronald Dworkin, Richard Posner, Roberto Unger); and in departments of historical, literary, and cultural studies (usually called “theorists” of some sort—linguistic, literary, cultural, postmodernist, psychoanalytical, feminist, queer—think Jacques Derrida, Stanley Fish, Jean François Lyotard, Jacques Lacan, Judith Butler. Think endlessly of Michel Foucault).

Our subject is not, however, as broad as “people who theorize about politics,” not even within political science. In truth virtually all political scientists—empirical, formal, philosophical, job-seeking and therefore up for anything—do such theorizing. Those designated “political theorists” in political science are distinctive chiefly because most have had training focused on the history of major works in political philosophy, though many may give greatest intellectual allegiance to any of a variety of schools of contemporary “political theory.” And today, “political theory” in political science also includes those designated as “formal” theorists. Most of those scholars have been trained in neoclassical and sometimes Marxian economic theory far more than in the history of political philosophy, and as a group they have relatively little intellectual interaction with philosophically oriented “political theorists.” But since they are counted as “political theorists” in many political science departments, even when theorizing comparativists, IR scholars, and various sorts of “Americanists” are not, they count as “political theorists” here. My concern in this essay is not to define what should count as “political theory,” but rather to explore what is happening in the work of people who are deemed “political theorists” in the profession of contemporary political science.

Given that disciplinary focus, what more appropriate place to begin than with the essays on the state of political theory that have appeared in the two extant APSA volumes on *Political Science: The State of the Discipline*? In the first of these volumes, published in 1983, John Gunnell laid out an account of the evolving historical place of “political theory” in the discipline that he would elaborate in subsequent influential works. We will return to some of his specifics, but pertinent for now is his conclusion. He observed some emergent aspirations to make political science “political theory” less “academic” and said that the “extent to which practical concerns . . . will have an impact on the . . . direction of political theory . . . is a definite question for the 1980s.” Gunnell was concerned not so much with whether political theorists engaged empirical social science as with whether they addressed major substantive political problems. But he professed “skepticism” about whether political theory was really about to move in such a more practical and engaged direction (Gunnell 1983, 38).
His skepticism arose in part because he regarded as “artificially limiting” (Gunnell 1983, 35) a trend that William Riker celebrated in the same volume. Riker thought that “main line” of development in political theory was its increasing identification with formal rational choice theory, with the enterprise of using deductive models to “identify the conditions for an equilibrium of preferences” on political issues (Riker 1983, 47). Gunnell implied that such political theory was ill equipped to shed light on many important political issues.

Yet as much as Gunnell, Riker could be read as urging those he viewed as “main line” political theorists to achieve a form of more practical engagement with politics. Riker called for the collection of data on the types of strategies that political actors deploy to achieve the sorts of equilibria they desire, and he provided examples that, he believed, supported theoretical generalizations about the sources of political change (Riker 1983, 55–64). Neither Gunnell’s skeptical essay or Riker’s evangelistic one, however, really devoted much attention to whether and how the forms of “political theory” they considered could engage empirical analysis more extensively. Nor did either essay strongly express any sense of urgency about doing so.

In subsequent works, Gunnell has gone on to articulate his mounting if rather despairing urgency about what he terms the “alienation” of political theory from engagement with substantive political phenomena and problems, its “imperious isolation” from “real institutions, interests, and political processes” (Gunnell 1986, 187–88; Gunnell 1993, 272–78). His concerns have begun, moreover, to be more widely sounded. True, in the second State of the Discipline volume in 1993, the tri-authored essay on “Formal Rational Choice Theory” presented such theory as a “method of theory construction” that was not concerned with any “particular set of institutions nor on a substantively defined set of political problems” and that had no particular “content” (Lalman, Oppenheimer, and Swistak 1993, 77–78). The essay’s three authors identified formal theory’s chief products as different types of theorems and formally modeled concepts, not as any theoretically guided empirical findings, though they briefly asserted that such contributions existed (Lalman, Oppenheimer, and Swistak 1993, 98).

In contrast, Arlene Saxonhouse’s discussion of the study of “Great Books” in political science insisted, against more purely historical approaches, that the exploration of classic texts could assist reflection on enduring questions, especially normative questions, that are very much problems in contemporary politics. She also perceived a revival of such study and reflection occurring (Saxonhouse 1993, 8–9, 21; cf. S. B. Smith 2000). But though Saxonhouse championed this sort of increased engagement by “political theorists” with political problems that are current as well as perennial, she, too, did not explicitly contend for any more extensive grappling with empirical social science.
Susan Carroll and Linda Zerilli’s companion essay on “Feminist Challenges to Political Science,” however, ended precisely by exploring the implications of feminist theoretical debates on the category of “women” for various kinds of empirical research, as well as for ongoing political problems of unjust inequality (Carroll and Zerilli 1993, 72). And even more emphatically, William Galston’s account of “Political Theory in the 1980s” concluded by urging theorists both to “try harder to take real political controversies as their point of departure” and to “become more empirically aware” in doing so (Galston 1993, 40–41).

Since that volume appeared, calls for theorizing that is both driven by real-world political problems and empirically informed have continued to proliferate. In 1994 Donald Green and Ian Shapiro published Pathologies of Rational Choice Theory, arguing that the preoccupation of formal theorists with crafting “universal theories of politics” instead of illuminating concrete “political phenomena” had generated a body of literature with few empirical applications and no significant empirical findings. They called for “theorists to get closer to the data so as to theorize in empirically pertinent ways” (Green and Shapiro 1994, ix–x, 203). The following year, the journal Political Theory published a polemical essay by Jeffrey Isaac that advanced a similar critique; but his target was the whole range of other sorts of political theorists. Isaac contended that “too much political theory” was “intent on producing metatheoretical insights at the expense of sustained engagement with empirical reality.” He maintained that political theorists must begin “to acknowledge this world as a source of intellectual and practical problems, to engage in it in all of its empirical and historical messiness, to demonstrate that our categories help to illuminate this political reality and even, dare I say it, to improve it.” And Isaac admonished that this did not mean invoking “real political problems” as merely “a pretext for scholarly investigations of other things; they should be what drives our inquiries” (Isaac 1995, 645–46, 682). His essay prompted a number of responses by leading political theorists that included some counterpolemics; but there were also some significant concurrences.

Also in 1995, but independent of that symposium, a prominent political theorist specializing in conceptual history, Terence Ball, published Reappraising Political Theory, and his reappraisal echoed the concerns of Gunnell and Isaac on many points. Reiterating and extending arguments he had been advancing for some years, Ball expressed worry about “political theory’s increasing isolation from its own subject-matter . . . namely, politics,” and its “increasing preoccupation” with “methodological and/or metatheoretical disputes” (Ball 1995, 53–54). He also argued that if theorists were “to speak knowledgeably about and intervene intelligently in the crises of our time, we will need at least some of the sensibilities of those among our fellow political enquirers who are conventionally classified as ‘empirical’ political scientists” (Ball 1995, 58). Thus he urged “a rapprochement between political ‘theory’ and political ‘science’” (Ball 1995, 60).
Around the same time that these critical calls for greater and more substantive empirical engagement were appearing from different varieties of political theorists, I participated in a conference sponsored by the American Academy of Arts and Sciences exploring disciplinary changes and trends in four disciplines. One common pattern that emerged was that advocacy of some sort of increased attention both to empirical reality and to concrete problems seemed to be heightening in every discipline. Philosopher Alexander Nehamas called for philosophy to become a more “engaged and consequential enterprise of the sort envisioned by the American pragmatists,” addressing “practical” problems in terms a “larger audience” could grasp (Nehamas 1998, 240–41). Economist David Kreps suggested that with “the increasing availability of data” and “computing power,” a “revolution” was beginning in economics which would see “a decrease in the importance of deduction” and “substantial increases in the empirical testing of economic theories” (Kreps 1998, 91, 95). I argued that “more empirically rigorous, historically sensitive” theorizing and testing should and would become “more prominent across the subfields” of political science (Smith 1998, 291). Even in English, Catherine Gallagher perceived a trend toward “carrying out research programs” rather than “debating theories,” with the research absorbing concerns about ethnicity, gender, culture, and politics into “new kinds of ‘practical criticism’” of a wide range of texts (Gallagher 1998, 168–69).

In the same general spirit, in 1997 the Foundations of Political Theory Section of the American Political Science Association established a new book prize, the “David Easton Award,” that was consciously intended to encourage greater engagement between more empirical and more philosophical work on politics. This pattern within political science and across multiple disciplines suggested to me then, and suggests now, that there are forces working to promote more problem-driven and empirically informed inquiries in American intellectual life generally. If so, those same forces are likely to continue to push many of political science’s different kinds of political theory in such directions.

Again, it would be foolish to claim that “political theory” has undergone any substantial “revolution” of this sort as yet. Still, it can be said that these various critics have not stopped at criticism. Most have made credible efforts to produce substantive works that engage extensively with concrete contemporary problems and pertinent empirical data, while still elaborating distinctive “political theoretical” perspectives. Among the political theorists advocating heightened empirical awareness, William Galston and Ian Shapiro seem to me particularly exemplary in these regards (Galston 1991; Galston and Baehler 1995; Shapiro 1999); but they are certainly not alone. Admittedly, I do not think it is yet possible to provide an extensive list of stellar works by political theorists that display impressive mastery of pertinent empirical studies of important political problems. But it is also true that efforts to write new “grand theories” of the sort that
figures like John Rawls (1971), Robert Nozick (1974), Jürgen Habermas (1979, 1984), Bruce Ackerman (1980), Roberto Unger (1987), and others produced in the 1970s and 1980s have become much less common in recent years. And far from hearing pleas to revive such efforts, instead it appears that the calls for more “practical” theorizing are continuing to mount.

The philosopher Thomas McCarthy, for example, presented a paper at the Central Division Meeting of the American Philosophical Association in April 2000 in which he argued that philosophic attention to “ideal theory” has for too long insulated “political theorizing . . . from the messiness of political reality.” He faulted subsequent “forays into nonideal theory” for what he saw as “their loose, post hoc connection to empirical work.” And he urged joining “the constructive and reconstructive aims of normative theory to the interpretive, analytical, and explanatory aims of history and other empirically based human studies,” as well as to “the practical aims of social and cultural criticism” (McCarthy 2000, 24, 26–27). Similarly, in a recent review essay, Cambridge political theorist Marc Stears perceives the emergence of “a widespread acceptance that there are greater potential gains to be made by combining the insights of political theory with observations as to the contingencies of practical politics.” He contends that “political theorists are turning back to the world of real politics” (Stears 2001, 216). And perhaps most strikingly, consider the following temporarily bowdlerized quotation:

The subject of (x) has enormous impact on everyone’s life, and yet the discipline lacks the status of a real science, follows rather than leads ideological trends, and sometimes indulges in fanciful theoretical representations of reality. Many branches . . . are not anchored in empirical knowledge, probably because the subject originated as part of moral philosophy and is still regarded as having to do more with thinking than with observation. This attitude is compatible with the field’s dependence on easily accessible statistical data, which, though essential, are also inadequate. Often it is not clear what they measure, and without this knowledge they can be used to support almost any contention. . . . Empirical knowledge means systematic experience with the object of study, and this can be had only by taking responsibility for data collection. Unfortunately, the gathering of . . . data is considered to be hardly within the scope of academic inquiry . . . great progress in science technology occurred only when thinking people came in contact with . . . practical realities . . . and when pragmatic attitudes took precedence over religious and ideological ones.

One might expect this quotation to be an account of political science coming from an advocate of ethnographic political research like James C. Scott. It is in fact a characterization of economics by Truman F. Bewley, the Alfred Cowles Pro-
fessor of Economics at Yale (Bewley 1999, 468). Long a leading game theorist, Bewley concluded some time ago that he could not answer the question that then concerned him—why firms lay workers off in bad times rather than lower their wages—by any modeling exercise or by using survey data alone. He needed to conduct more in-depth interviews with employers; and so he did. As the quotation above suggests, that experience has led him to go still further than David Kreps in insisting that economics must become a less abstract, more empirically informed enterprise. It must be informed not just by statistics but by “systematic experience with the object of study,” and it must draw on psychological as well as abstractly rational accounts of human cognition and behavior.

Others in the economics profession must agree. In April 2001, the American Economic Association’s John Bates Clark medal, given every other year to the “best economist in the United States under age forty,” went for the first time to a behavioral economist, Matthew Rabin of Berkeley, whose work combines psychology with economic theory. In recent years similar behavioral economists have been added to the Harvard, MIT, and Chicago economics departments, among others. If leading economists are moving in this direction, it seems likely that at least some leading formal theorists in political science will eventually follow. If they do so, expectations that all kinds of political theory should be empirically grounded and practically relevant will probably strengthen as well.

Okay, So Why the Shift?

I hope this admittedly impressionistic evidence suffices to persuade readers that we are at least hearing more calls for political theorists of all stripes to connect more closely with empirical studies and concrete political problems; because ready or not, I now move on to the “why” question. What are the forces or factors moving political theory and other intellectual endeavors in this direction? My answers are at best merely informed guesses, but I hope they will be of use in reflecting on what really is at stake in this discussion, decisions about how political theorists in political science might pursue their work most productively today.

It seems plausible to presume that, in general, our intellectual endeavors are shaped to some degree both by the internal logic of the tasks they undertake and by their broader environments. I believe this also to be true of the endeavors of political theorists in political science, and I think that, at least for these scholars, the broader environment of “real-world” politics is ultimately most decisive. This latter claim may seem to put me at odds with the perceptive scholars just quoted who claim that political theory has been at risk of becoming too unconnected with such politics. So let me add quickly that the ways political theorists respond to their political environments, though understandable,
are not necessarily most intellectually fruitful or politically useful; and in any
case, no response can remain fully appropriate forever in a changing political
world.

To grasp at least one aspect of the internal logic that has shaped the intellec-
tual evolution of both political theory and political science over time, we must
recognize that modern academic analysts of politics face two imperatives that
are in profound tension. We seek to achieve genuinely scientific knowledge
about politics, that is to say, descriptions, explanations, and arguments that are
as precisely stated, logically rigorous, well tested, and empirically accurate as
possible. We also seek to provide substantive, useful, normatively compelling
answers to political questions that we, along with most other people, regard as
extremely important in human life.¹⁰

These aspirations are in tension because in truth it is probably impossible to
achieve very rigorous empirical results in support of clear, logical theoretical
propositions addressed to any but a small number of relatively minor political
matters. We can and should do such studies; but when we move on to larger
questions, as we also should do, our best science is inevitably “softer” science.
We have to stitch together roughly and prudentially the findings of various per-
tinent but distinctive specialized studies, filling the gaps with cruder sorts of
data and assumptions, and linking them all together with even more disputable
normative evaluations. On “big” questions, political science’s most important
contributions have never really resembled pristinely tested experimental find-
ings evaluating precisely stated theoretical claims derived from secure, uncon-
troversial premises. They have rather been well-informed, probabilistic but
debatable arguments about what is most likely to be both true and desirable con-
cerning the great political problems humanity faces. And that is all they are ever
likely to be.

These circumstances do not arise because political scientists are not smart
enough, industrious enough, or genuinely scientific enough to do their work
properly—though to be sure, leaving the psychological health of ourselves and
our families aside, it is desirable always to try to be better in all these regards.
The reasons are deeper; and they can be ameliorated but not overcome. Unlike
many types of natural scientists, academic political analysts cannot hope to un-
dertake the most reliable kind of empirical inquiries, properly controlled field
experiments, except on a relatively narrow range of nontrivial but still less than
central political questions. We cannot, for example, randomly distribute popula-
tions and then subject them to different sorts of legislative institutions to isolate
the consequences of those institutions. We must instead turn to quasi-exper-
imental manipulation of quantitative data about various populations with differ-
ten legislative systems—but on most matters these are rife with empirical
inaccuracies and debatable categorizations, and the manipulations are always

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imperfect approximations of actual experiments. Given those limitations, we must also rely on induction from ethnographic observations, case studies, history, and lived experience to produce what are, at best, plausible speculative generalizations. It is likely, moreover, that basic features of human agency, including people’s tendencies to learn and to be otherwise affected by discourses concerning them, mean that any reasonably concrete behavioral regularities we identify will be at best probabilistically “true,” and “true” in some times and places but not others.

Hence empirical knowledge on a whole host of matters pertinent to important political issues can be obtained only in rough and imprecise forms, with a few things known more reliably than others, but with good reason to remain skeptical about many descriptive, much less explanatory, claims. All arguments that build on such knowledge must therefore be regarded as even more rough and imprecise, no stronger than their weakest empirical and explanatory links. Some legislative systems may be structurally more likely to induce multiple parties than others, or either to facilitate radical changes or produce gridlock; but whether we get any of those results in any actual legislative context will probably depend much more on a whole range of factors beyond legislative structure, such as the outbreak of international wars, the onset of depressions, or the rise of some passionate and powerful social movement. We are unlikely to have well-developed knowledge about many of those factors; but to say anything substantial about major questions such as whether substantial realignments or new parties are likely, or whether the legislative system is likely to generate major transformative legislation in the future, we have to know something about all of them.

Thus, whatever our aspirations to achieve empirical support for rigorously deduced hypotheses, in fact the most we can ever say honestly in political science is very limited. In regard to many substantively important and complex political issues, we can only conclude that some empirical descriptions and explanations and some substantive answers built upon them are, in a crudely ordinal sense, better informed and more consistently reasoned than other, more obviously ignorant and illogical ones, and therefore somewhat more likely to be right. Finally, whatever may be correct about the ultimate status of norms of political morality, we do not have access to any such norms that have been unequivocally established and universally understood and accepted. Hence all the normative assessments that inevitably enter into our professional political analyses and arguments can be and usually are debated as well. We can still do recognizably better rather than worse in our work, and the importance of politics makes it imperative that we strive to do so. Still, academic political analysis faces severe limitations stemming from our dual quest to be both scientific and substantively significant; and these are limitations that, given our particular subject matter, we cannot hope to transcend.
If pressed, most political scientists would probably acknowledge that the above propositions are true, but understandably we do not generally dwell on these somewhat dampening realities, even in our own thinking. I do so here only because I think recalling these sobering truths helps to answer the question at hand. This basic tension in our goals means that in every era since political science emerged as a modern discipline, we can find some work that chiefly stresses obtaining theoretical and empirical scientific rigor; some work that chiefly seeks to make substantive arguments on large, important political questions; and much work that falls somewhere in between. We can also find ongoing sniping among the practitioners of these various sorts of work. And we can, I have previously suggested, detect some cyclical sliding of the bulk of the profession’s work in one direction or the other, whenever we seem to be going so much to one extreme that our other aspirations are in danger of being lost altogether (Smith 1998, 275–76).

In the ensuing discussion of “why empirical engagement by theorists now,” I note some points and some ways in which I think this internal tension in the endeavor of academic political analysis has played a role in prompting change. I also note how the specific internal logic or substantive agenda of particular scholarly enterprises within political theory and political science sometimes seems to account for subsequent developments. But again, like others I suggest that the greater part of the explanation for our evolving professional trajectory lies in the broader worlds of politics in which we operate. Ultimately, not only our topics but also our modes of inquiry are most shaped by what we see as the great political issues of our day, and by our prospects for addressing them in ways that can best advance our intellectual and political causes, while staving off their often powerful opponents.

To make this case, let me pick up the story of our profession with some standard accounts of the rise of “grand theory” in political science and other disciplines beginning in the late 1960s. Many scholars (including me) have documented how “political theory,” understood either as the analysis of great works of political philosophy or the construction of new great works of normative political argument, seemed on the wane in the United States during the 1950s and early 1960s. Plausible explanations for this development include the intellectual and political discrediting of “grand ideologies” like fascism and communism in the wake of World War II, and the emergence of an American “consensus” on basic liberal democratic norms; the undeniable limitations of preexisting modes of political analysis; the rise of positivist understandings of scientific inquiry, which left no space for many kinds of grand theoretical claims and tended to confine philosophy to linguistic and conceptual analyses; the development of new, more mathematical forms of economic modeling and analysis, which appeared to promise more scientific rigor in social inquiry and
policy recommendations than “traditional” political theorizing could provide; and the desire of both American governments and American foundations during the Cold War to finance what they saw as genuinely “scientific” studies of human behavior, especially studies that promised both to improve American managerial efficiency and to justify market systems, thereby helping the nation to overcome its Communist rivals. All of these factors contributed to the “behavioral revolution” that was perceived then as well as now to be hostile toward traditional work in political philosophy and “grand theory” thus understood.

To be sure, these outlooks were actually consistent with rising optimism about other sorts of rather grand theorizing. But these new theories were efforts, such as “structural-functionalism,” “systems theory,” and still-nascent rational choice theories, that presented themselves as fundamentally concerned to categorize and explain human political behavior, not as prescriptive or normative enterprises. Their practitioners were often dismissive toward such enterprises and toward political theory understood as the study of great philosophic works (Gunnell 1983, 18–21; Easton 1993, 294; Farr 1995, 203–4, 213). Then, many have argued, things changed.

In 1983 John Gunnell noted this “widespread belief” in what he called an “alleged upturn” in “political and social theorizing” and “political philosophy” beginning in the late 1960s. He observed that this “upturn” was “usually linked with the publication of John Rawls’s A Theory of Justice (1971) and related works of a similar genre” and with “the popularity of so-called Critical Theory and the works of individuals such as Jürgen Habermas” (Gunnell 1983, 5). Gunnell called the upturn “alleged” because he was skeptical about the quality of these new theoretical works, but he did not dispute their proliferation. He was not alone in his doubts. But when the intellectual historian Quentin Skinner put together a 1985 edited volume entitled The Return of Grand Theory in the Human Sciences, he argued that such disagreements merely meant that we should expand the list of resurgent “grand theorists” to include various “skeptics” such as Hans-George Gadamer, Derrida, Foucault, and their admirers, among others. He also mentioned, though he did not excerpt, the works of Lacan and “feminist” scholars left unnamed (Skinner, ed., 1985, 1–20).

Skinner himself, moreover, was along with J. G. A. Pocock and John Dunn a leader of a different sort of return to political theory. They practiced and preached in favor of the study of the history of political thought with much greater attention to what the writers of “great texts” and other political treatises could be understood to be doing in relation to the political controversies, concepts, and vocabularies of their times. The rise of their “Cambridge School” represented a challenge to various other modes of studying the history of political philosophy, such as those influentially advocated by Sheldon Wolin and Leo
Strauss during the 1960s (Saxonhouse 1993, 6–14; Gunnell 1993, 252–60). For Wolin the modern history of political theory was a tragic tale of lost appreciation and support for meaningful political action; for Strauss it was an even bleaker story of lost appreciation and support for true philosophic virtue. The Cantabrigians were skeptical about all efforts to portray modern intellectual history as a story with any enduring moral. But despite their substantial intellectual and political differences, all these figures agreed on the philosophic inadequacy and frequent political irrelevance of behavioralist political science. Collectively they carved out renewed space for studies of historical political thought within political science. It was both this varied resurgence in the study of “great texts” and the diverse efforts to produce new forms of “grand theory” that created the widespread sense of the latter’s “return” by the 1970s.

Why did all this occur? Here the standard responses are counterparts to those just listed. Positivism and behavioralism underwent philosophic assaults for failing to produce much in the way of reliable lawlike findings, and for failure to capture the interpretive dimensions of human life, including intellectual enterprises. These criticisms reflected in part the internal tension sketched above. Just as postwar scholars had found existing political science to be insufficiently scientifically rigorous, so many scholars from the mid-1960s on found the results of trying to make political science more “scientific” in the behavioralist sense to be unimpressive. The behavioral revolution seemed to have produced few if any major scientific results at all, and it was especially deficient in results pertinent to the contemporary political upheavals that increasingly concerned political scientists in those years; so they sought new directions. Similarly, a wide range of scholars also deemed the application of economic methods to political phenomena unduly reductionist, even if to growing numbers of others rational choice approaches still seemed the most scientific form of “grand theory” available. Hearing these vocal concerns, foundations and some government agencies also began to support greater varieties of scholarly work, thereby insuring that a greater variety would exist.

Though each of these factors probably played some part, most writers trace all these developments back to the broader political environment, to what Samuel Scheffler calls the “backdrop of intense social conflict and rapid social change” during these years (Scheffler 2001,1). That backdrop included in America the Vietnam War, the civil rights and women’s movements, the War on Poverty, the “sexual revolution” and the late 1960s “counterculture.” Internationally there were counterparts to many of these movements, along with the great transformations involved in the accelerating processes of decolonization and new nation building. These were developments that cried out for normative as well as empirical analysis. Positivistic, behavioralist political science seemed most vulnerable because it failed to address any of these topics very
fully, and its failures seemed to betray its tacit normative biases toward the status quo. To the many critics, both right and left, of the mid-1960s liberalism that seemed to dominate much empirical political science, those biases were deplorable (Farr 1995, 203–4).

In these years rational choice approaches also seemed especially structured to favor market solutions at a time when capitalist inequalities, within and across nations, were coming under heightened attack. To be sure, that very leaning explained formal theory’s appeal for some scholars; and for those more left-minded, forms of rational choice–infused “analytical Marxism” would eventually emerge in the work of G. A. Cohen (1978), John Roemer (1981), Jon Elster (1985), and others. Their recasting of Marxism using the analytical tools favored by the liberal mainstream reflected both intellectual conviction and desires to give radical economic egalitarianism fresh intellectual credibility. Their writings are therefore consistent with the notion that new kinds of “grand theory” arose in part in reaction to the political context of the times. It was probably also this context, especially, that prompted philanthropic foundations and some governmental funders to begin to support different kinds of academic work on politics. Thus it was the politics of the era above all, many scholars have contended, that led to the revival of explicitly normative theorizing, to new approaches to the history of political thought, and to critical philosophic reflection of many sorts (Skinner 1985, 5–12; Galston 1993, 28–29; Gunnell 1993, 263–74; Ball 1995, 45–53; Farr 1995, 216–18; Smith 1998, 277–81).

Those explanations are in conformity with my contention that broader political problems most shape both the substance and methods of the discipline’s work, but they raise a further puzzle. Why were so many works embodying this new normative theorizing, critical philosophizing, and studies of the history of political thought themselves so abstract and/or apparently disconnected from direct engagement with current political problems—the very problems that supposedly inspired these enterprises? Though, as we saw at the outset, laments about these characteristics have been rising for a long time, this question has not been sufficiently addressed.

Three answers may quickly spring to mind; but without rejecting them entirely, I will emphasize a fourth. The first answer is that the “rise of grand theory” in this era took place precisely against the perceived limitations of empirical social science. Hence, one might think, it is not surprising that few of these scholars extensively engaged empirical work. Especially to some more radical writers on both the left and right, American empirical political science seemed too much part of the problem to be part of the solution.

The second answer charges that in the work of at least some of the more mainstream “grand theorists,” such as Rawls and Nozick, as well as formal theorists like Riker, abstraction was a means of ideological mystification. It helped
theorists to avoid having to adopt the genuinely radical critical postures that would be demanded by direct confrontations with contemporary political and economic realities in the United States and the rest of world. One need not talk much about the oppressed conditions of workers, Third World peoples, racial and ethnic minorities, and women and other disadvantaged groups, so the criticisms ran, if one could shift attention to a veiled “original position,” “state of nature,” highly simplified models of rational action, or even to “ideal speech situations.” One could be all for justice, but only in the abstract. Habermas, in turn, has charged that many postmodernists have effectively supported political conservatism by discrediting all endeavors at rational reform and that their modes of discourse have masked this reality (Hoy 1985, 61–62).

The third, perhaps more mundane, answer is that, with the extraordinary proliferation of specialized disciplinary publications in modern academia, few scholars can be expected to have the intellectual capacities, the time and energy, or the incentives and will to master significant bodies of work outside their designated area of scholarship. Lack of extensive engagement with empirical studies is simply an inevitable by-product of the modern academic division of labor. There is probably something to each of these explanations, but neither individually nor collectively do they seem to me really convincing. Whatever the inadequacies of existing empirical work, most of the contributors to the “return of grand theory” made arguments that rested on empirical and historical characterizations that they could reasonably be expected to justify as rigorously as possible. The radicalism of one’s outlook provides no exemption from defending the key factual claims one is undeniably making. In any case, the problems of behavioral scholarship do not explain why theorists like Rawls, Nozick, Derrida, Strauss, and even Habermas chiefly or exclusively wrote works that did not address very directly many of the major political issues of their day. It is that choice that seems especially surprising in light of the widespread indictments of behavioral empiricism as politically irrelevant. It is also this feature, even more than lack of empirical engagement, which has been especially prominent in the polemics against “grand theory” in recent years.

It is also difficult to view the abstractness and political disconnectedness of many of the “Grand Theory” writings of the 1970s simply as features of works of ideological mystification on behalf of the status quo. Many writers who are clearly in some respects radical in their politics, from Marxist economists like Roemer through postmodernist theorists like William Connolly to philosophic aristocrats like Allan Bloom, have long found rational choice and Rawlsian, Derridian, and Straussian modes of analysis and discourse highly congenial for their enterprises of quite overt and sweeping political criticisms. Finally, there may be more truth still to the third argument’s appeal to the finitude of human capacities and the disciplinary organization of modern academia. But this di-

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sion of labor is at least as much a product as it is a cause of scholarly judgments about how academic inquiries should be structured to generate valuable work. Notions of how far serious normative arguments, in particular, must be empirically knowledgeable have shifted over time and they seem to be shifting again, this time toward greater empirical engagement, even though the piles of articles and books confronting a theorist who seeks to be well informed have only grown. Those shifts therefore need explanations that go beyond the material conditions of modern scholarly production.

Before turning to my own preferred account, let me acknowledge that it is possible to dispute the premise of this discussion: that the forms of “grand theory” that “returned” after the late 1960s were excessively abstract and disengaged from empirical social science findings, pertinent history, and immediate, concrete political problems. William Galston and Terry Ball have contended that the theories of Rawls and Habermas, among others, though presented via “awesomely abstract formalisms,” are motivated by and speak to concrete, contextual political controversies (Galston 1993, 33; Ball 1995, 51). As noted above, Arlene Saxonhouse also believes that many studies in the history of political theory speak to enduring and therefore current political issues. And as rational choice scholarship has matured, more and more attention has been paid to a range of actual electoral, judicial, and administrative institutions and to many policy issues (e.g., Mueller 1997). As Galston has also noted, the upsurge of feminist theory from the 1960s through the 1980s was explicitly informed by engagement with, and often participation in, the “consciousness-raising” feminist social movements of the era and directly contributed to that movement’s activist political and legal agenda (e.g., MacKinnon 1979, 1987). William Connolly has insisted that postmodernist scholars raise deep problems about contemporary political life even when they do not discuss current political developments in detail (Connolly 1995). It is also true that Habermas has in his extraordinary oeuvre of scholarly and journalistic writings grappled with an enormous variety of empirical studies and current political problems (far more than Rawls; though Ronald Dworkin has long written influentially, often in more popular publications, on an equally impressive range of contemporary legal and political controversies, and Bruce Ackerman is catching up). For a time, moreover, Wolin edited a journal devoted to contemporary political affairs. Foucault’s writings have also had a justifiably large impact on historical as well as critical theoretical scholarship; while scholars influenced by Strauss have played prominent roles in a large number of American policy debates. Indeed, scholars inspired by all these writers and approaches have gone on to discuss an impressive range of historical and contemporary political, legal, and moral issues, sometimes delving far into pertinent historical and empirical literatures in order to do so.
It nonetheless remains true that major works like Strauss’s *The City and Man* (1964); Rawls’s *A Theory of Justice* (1971); Nozick’s *Anarchy, State, and Utopia* (1974); Derrida’s *Of Grammatology* (1976); Dworkin’s *Taking Rights Seriously* (1977); Foucault’s *Discipline and Punish: The Birth of the Prison* (1979); Ackerman’s *Social Justice in the Liberal State* (1980); Riker’s *Liberalism Against Populism* (1982); Habermas’s *The Theory of Communicative Action* (1984); and other pillars of modern “grand theory” display only limited direct engagement either with contemporary political issues or with empirical social science. Commentators like Galston, Saxonhouse, Ball, and Connolly concede as much, and I cannot imagine that any readers of these works will disagree. But beyond any disdain for empiricism, cynical ideological motives, or constraints of academic specialization, I see in the works of this era a mix of some intellectual and political optimism and a much deeper sense of desperation inspired by the profoundly conflictual period in which most of these projects originated. That mix is, I believe, rather typical of eras when works of highly abstract “grand theory” have been written historically, such as the declining days of Athens, the English “century of revolution,” and the Weimar Republic, though I will not try to defend this more general proposition here.

The intellectual optimism is visible in the ways these authors thought that an insightful blend of adaptation and criticism of the analytical tools forged by their recent predecessors would generate perspectives capable of major intellectual accomplishments. Thus Rawls sought to use a modern analytical “thought experiment” to frame the problem of social justice as a kind of grand exercise in rational choice theory. Nozick was encouraged by Rawls’s example to propose alternative thought experiments, and Dworkin felt able to treat legal decision making as a Herculean philosophic exercise and one that ought to privilege a Rawlsian right to “equal concern and respect.” Derrida and Foucault in different ways offered analyses of language, subjectivity, and power that drew upon concepts derived from Heidegger and ultimately Nietzsche; and Strauss built both on and against those earlier philosophers’ portrayals of Western intellectual history. Habermas managed to find inspiration in social psychology, in the sociological theorizing of Talcott Parsons, and in the cognitive development theories of Jean Piaget, as well as in Weber and Frankfurt School Marxism. What is common here is real hope, visible also in the ambitions of Riker and others for formal theory in both its liberal and radical variants, that these scholars were in possession of fairly novel philosophical insights and analytical tools that could make grand intellectual endeavors possible. Such hopes are not always with us; indeed I will suggest that they are not with us to nearly the same extent today.

By themselves, however, those promising tools and insights are probably insufficient either to motivate projects of “grand theory” or to explain why they might be pursued via quite abstract and politically disengaged modes of
argument. As to motivation, I think it likely that the political circumstances of the 1960s fostered a certain kind of limited political optimism. In those turbulent times, it seemed possible that significant transformations in our political lives might occur, for good or ill. Hence it was imaginable that work could be done that might really persuade many people to think about their lives and politics in quite different ways. Put crudely, that possibility, which again is not always with us, can spur at least some scholars to “think big.”

Yet the abstract, at most indirectly engaged character of these writings manifests, I believe, a deeper desperation in the authors concerning their political times. These were years of sharp, often bitter polarization over political issues that were often seen as matters of definitive, irrevocable moral choice. Though the McCarthy era had receded, the Cold War still made liberal-capitalist or socialist-Marxist commitments into matters of fierce global struggle. Expressions of strong partisanship could be met with professional ostracism and sometimes harsher sanctions from powerful public and private actors. Supporters of radical changes in racial and gender statuses were often derided as hate-mongering, fanatical agitators; their opponents, and many who sought to remain passive or moderate, were in turn denounced as racists and misogynists. Many strong proponents and opponents of the Vietnam War saw the “other side” not as a “loyal opposition” but as reprehensible, immoral criminals. And violence seemed to be erupting everywhere: not simply in Vietnam, but in America’s urban slums, in the brutal repression of sometimes wantonly destructive student demonstrations in the United States and also in Europe, and in the repeated and often successful assassination attempts on presidents, presidential candidates, and other political figures including numerous civil rights activists and eventually a prominent civil rights opponent.

In such a climate, I submit, it can seem sensible, even necessary, for thinkers with grand aspirations to transform the ways of thinking and acting dominating the politics of their times to avoid direct engagement with these severely polarized issues. To do so might well make it impossible for the writer even to raise the deeper issues with which he or she is concerned, much less to be taken seriously by any not already on the writer’s side. Whether consciously or subconsciously, I believe that many contributors to the era’s “grand theory” revival felt that they could do more through abstract, relatively disengaged forms of political writing than through more immediate political commentary.

I have little doubt, for example, that A Theory of Justice was inspired extensively by the movements for greater racial and economic equality of the 1950s and the 1960s. Rawls’s device of abstraction from particular social identities to win support for his “difference principle” was at least in part a means to side with reformers in a way that might seem fair-minded and reasonable even to many who had reservations about actively doing so. Similarly, Habermas’s invocation
of the regulative conception of an “ideal speech situation” surely represents in part an effort to find a more widely agreeable and defensible foundation for economic egalitarian commitments than any form of explicit Marxism was likely to provide. Analytical Marxists, conversely, did choose to be more explicit, but only while using increasingly fashionable formal modes of reasoning that they believed could not be easily dismissed. In contrast, formal theorists like Riker (1962, 1982) and James Buchanan and Gordon Tullock (1962) sought through abstraction to discredit in principle the wisdom of activist, egalitarian governance.

I think it likely, moreover, that Strauss’s later writings were also spurred in part by concern about the extremes to which modern egalitarian movements might go (concerns which, to be sure, he had long harbored). But he could have seen little benefit in directly challenging the civil rights movement, the women’s movement, or Great Society poverty relief measures; it may well have seemed far more important to articulate more philosophically what modern life risked losing. Similarly, French writers like Derrida and Foucault could use their Nietzschean analyses of texts and discourses to reinforce varied challenges to the reigning bourgeois political, economic, and cultural systems without being written off as narrow partisans of any particular insurgent movement. The American theorists in political science who in various ways annexed these European writers have sometimes been accused of transposing them to a national context where their political significance vanished and only their abstractness and disengagement remained (e.g., Gunnell 1993, 273). I believe, however, that the Americans, too, were using “grand theory” to foster a critical and transformative intellectual atmosphere that seemed more likely to further the political directions they favored than any thrashing out of narrower empirical controversies or straightforward partisan advocacy might do. These hopes may have been mistaken; indeed, I think that has proved to be the case; but they are certainly comprehensible.

Furthermore, the American postmodernists, at least, were also joining in another enterprise. They were responding to the forms of abstract liberal theory that writers like Rawls, Nozick, Ackerman, Riker, Dworkin, and others advanced in the 1970s and early 1980s. It is not surprising that, at least initially, the very success of Rawls and Rawls-influenced writers in apparently rehabilitating normative political philosophy with a broadly liberal content should spark efforts by opponents to discredit these works at a theoretical level. When “communitarians” like Charles Taylor (1985, 1989), Alasdair MacIntyre (1981), and Michael Sandel (1982) challenged liberal conceptions of “the self”; when feminists like Carole Pateman (1985, 1988), Susan Okin (1979, 1989), and Catherine MacKinnon (1987) challenged the conceptions of the patriarchal family and of gender equity in liberal theories; and when various American partisans of Derrida and Foucault also worked to expose the class, racial, and gender biases
and disciplinary impulses built into liberal presentations of universal rationalistic standards, most did so with the hope that demolition of the elaborate theoretical underpinnings of liberal “grand theories” would assist delegitimation of repressive political practices and policies those theories were thought to serve. During, roughly speaking, the 1980s, the era of “grand theory” was thus perpetuated via philosophical assaults on the theoretical constructions of the preceding decade and by their renewed defenses. But for many participants, these debates still seemed a way to address pressing contemporary political issues most effectively, by simultaneously rising above and digging below them.

The Decline of Grand Theory

I have sought to show in the first section of this paper that since at least the mid-1990s, the mood of many political theorists and probably more political scientists appears to be shifting once more. Cries are mounting against most if not all of the forms of “grand theory” that resurged in the preceding two decades, and some genuinely more concretely and empirically engaged works are appearing. So again the question arises, why is this now happening?

Here, too, we can perceive certain internal logics at work, along with what I see as still more fundamental forces. Though it is understandable that normative theoretical arguments were challenged on various theoretical planes, over time those clashes tended to throw off sparks that frequently whirled upward into higher and higher levels of abstraction. Some theorists did indeed come to seem most concerned about problems that were far more internal to particular theoretical enterprises than motifs for discussing “real world” political issues productively. Because such work has often not only failed to further professional aspirations to achieve a more rigorous empirical science of politics, but also has not seemed a credible means of exploring substantively important political matters, many scholars have understandably reacted by urging a shift back toward more empirically focused theorizing.

I also perceive, though I cannot document here, an internal logic at work more specific to the content of the particular forms of “grand theory” that became prominent in the 1970s. Although many tried to move from Rawlsian-style “ideal theory” and “ideal speech situations” not to higher abstractions but to analysis of actual political, legal, and moral controversies, building intellectually credible ladders from “ideal theory” back down to our less than ideal empirical world often proved hard to do. Rawls told us that we were not supposed to decide policy questions behind his “veil of ignorance,” but he said little about how we should do so; and the implications of Habermas’s regulative ideal were also often unclear. Somewhat similarly, as proponents of formal theory sought to make their models less simplified and more realistic, they often encountered
frustrating mathematical complexities without improving the performance of their models in empirical tests. That is one reason why many economists are coming to accept, like Bewley, that some of the longstanding doubts about the empirical payoff of the rationalist modeling enterprise can no longer be dismissed as premature.

As for the practitioners of the historical study of "grand theory," the Cambridge school seems to have paved the way to its own diminished influence by too often suggesting that genuinely historical accounts of philosophic texts reveal them to be of little relevance beyond their own time (Saxonhouse 1993, 10–12). Proponents of Straussian, Wolinite, and kindred readings of great works have not advanced that self-defeating message. But too often, in my judgment, many such scholars have seemed to reach exactly the same quite general lessons for contemporary politics over and over, no matter which great text they have been discussing at the time. These repetitive conclusions cast into doubt the distinctive contribution of their close readings of particular philosophers. Adherents of feminist and postmodernist critical perspectives have also had more success in repeatedly laying bare indefensible elements in many dominant discourses and practices than they have in suggesting and defending alternative policies and institutional arrangements.

All these experiences have, cumulatively, significantly diminished the spirit of intellectual hopefulness visible in the early works of the modern "grand theory" revival and also in many of their "first wave" critical theoretical responses. Today, any faith that the previous generation of scholars has devised tools and concepts that the bright and energetic can use to go far beyond them appears faint. Instead there is a widespread sense of grim acceptance that the previous generation's various debunkers have had the stronger end of the arguments, though even the debunkers themselves give us little guidance on where to go or even how to look. I would not be surprised if many readers of this essay judge this factor to be the most important one in explaining the turn away from grand theory that does seem evident now.

That is, however, not my view. Here, too, my best guess is that the circumstances of our political world are most decisive. My belief, again, is that one key and enduring feature of all academic political analyses, constructive and debunking alike, is their inability to make anything more than rough, probabilistic arguments for their preferred conclusions. Hence the positions of debunkers, though perhaps they may seem somewhat more convincing than the views they attack, are rarely so decisive as to render it obviously foolish to pursue the enterprises that comprise their targets. What political scholars do choose to undertake is therefore most likely to reflect the political conditions of their times, rather than the vulnerabilities of certain options to intellectual challenges.

In contrast to the 1960s and early 1970s, American politics today is less visibly
polarized, and before September 11, world politics seemed to be also; even in its wake, those nations identified with the newly central cause of antiterrorism far outnumber those labeled as terrorism’s allies. And even in the context of a new sort of global war, there is also less sense than in the 1960s that dramatic transformations are possible in the near future. The most recent American presidential election featured candidates with bases in the rather similar centrist wings of their parties, however much George W. Bush may be suspected to be more at home with conservatives. The days of intensely mobilized social movements; massive, violent demonstrations; and raging urban riots are largely past (and when they recur they are largely ignored); and though matters may be changing rapidly, thus far the new international context does not resemble the sharp division of the world into two massive, opposed ideological camps that characterized the Cold War. In such contexts, it has been reasonable for contemporary scholars to hope that well-informed discussions of contemporary issues might find a hearing among a wide range of political participants. Thus, to a greater degree than in the 1960s, there has seemed to be little risk and some real possible political benefits from pursuing more directly engaged and empirically grounded scholarship.

But in the United States, probably more so than in many other parts of the world, the flip side of this lack of visible intense polarization is a sense of diminished options. Though few Americans were strongly attracted to the Soviet model by the time of its collapse, that event still made it easier to believe that, in ideological terms, we were at a liberal democratic “end of history.” Today, it is still hard to perceive either in the array of existing political forces or the ideas of current leaders many possibilities for major political changes of any sort. Addressing existing practical problems in empirically informed ways that may perhaps help political debate to inch a bit further in one’s preferred direction often seems like all one can hope to do. Contemporary American political scientists are operating, in sum, in an environment in which it is not easy really to believe in any grand theories intellectually, and it is even harder to believe that American politics has much space for such theories to have much even indirect influence. Only less grand but more empirically sophisticated and directly engaged endeavors promise to be intellectually and perhaps even politically fruitful. When great firestorms have finally sputtered out, it can seem that there is little left to do but to light some small candles if we do not wish only to curse the darkness—especially if we have survived the storms by ducking back into our cave.

Closing Question: Should We All Be Troglodytes?

I believe that some great thinkers can genuinely transcend their times; but I also believe that I am not one of them. Hence I acknowledge that I am on balance a
committed member of the growing chorus that calls for political theorizing to address more practical problems and to be more empirically informed and that tries to do work meeting those standards. I will not detain readers with my reasons for advocating a more practical, empirical turn for contemporary political theory, because in fact I endorse all the negative assessments of the various types of “grand theory” that I have just rehearsed. For the time being, at least, I think that, in particular, the benefits of the sorts of highly abstract “constructive” grand theorizing that emerged in the 1960s are largely played out. Too often, they achieved not greater theoretical power, but only grand rhetorical presentations of particular political commitments as the philosophically “right” answers in political and judicial decision making, discerned with Olympian disregard for empirical realities. We can contribute more both intellectually and politically today by seeking to link theory more closely to major concrete issues and to what the best research tells us about their conditions and causes, just as many scholars are now urging us to do.

I am not at all inclined, however, to end this essay on what may sound like a negative note. I think the turn in contemporary political theory that I am supporting is in fact a highly promising one; and I also believe that there nonetheless remains an important place, probably a growing place, for at least two sorts of “grand theory” enterprises, if appropriately pursued. In this final section I will briefly elaborate these three positive possibilities, with the honest hope of lifting some professional spirits that the foregoing may have somewhat discouraged.

There is a worry to be confronted when political theorists attempt to focus on practical problems and to incorporate empirical historical and social scientific research into their endeavors. The worry is that they will contribute nothing by doing so. Perhaps political theorists cannot hope really to master such problems and such research sufficiently to make their discussions anything more than amateurish. Perhaps they have no “value added” to provide even if they do get “on top” of such topics.

Two recent books provide particularly strong evidence that this is not the case. The English political theorist David Miller’s *Principles of Social Justice* (1999) explicitly seeks to give discussions of that topic “a less abstract character” by taking “empirical evidence about how the public at large understands justice” seriously. He also urges paying “closer attention to the social contexts in which principles of justice are applied” (Miller 1999, x, 42–43). Miller fully recognizes that survey data and other forms of empirical research are at best imperfect windows into people’s moral thinking, and that it is in any case questionable to define justice simply in terms of people’s conventional beliefs. But he argues powerfully that philosophers have long held that our moral intuitions are among the appropriate reference points in ethical deliberations, and that political principles are not likely to have much real-world potency if they are not expressed in
ways that show understanding of prevailing beliefs. Critical engagement with sophisticated, contextually sensitive empirical research into popular beliefs thus has the potential to move political philosophy beyond elaboration of what highly educated elites find abstractly appealing. As a result, Miller both calls for and makes considerable progress towards “a normative theory that works the findings of empirical research into a consistent whole” (Miller 1999, 60).

To my mind, an even more compelling example of empirically engaged theorizing is another 1999 book, my erstwhile colleague Ian Shapiro’s Democratic Justice. As in his earlier work, here Shapiro actively deploys an extensive knowledge of the history of canonical “great works” like those of Hobbes, Locke, Kant, and Marx and also a wide reading in contemporary political theory. He also continues to construct his own version of democratic theory, premised on democracy’s role in combating indefensible power hierarchies. But in this work he also engages extensively with legal, historical, and social scientific works laying out various dimensions of current issues. They include the power of the state and parents over children; the structure of rights and duties in marriages; the powers and protections legally afforded to workers and employers; and the structuring of health-care decisions in terminal cases.

In each area Shapiro makes a credible case that the sorts of democratic commitments that he defends suggest certain new structures of legal rights and governmental policies, while arguing strongly against others. The analysis as a whole serves both to add flesh to the bones of a particular theoretical conception of democracy and to provide informed, practical, novel reform recommendations on important contemporary issues. Shapiro’s Deweyan choice to begin with current felt problems and the conflicting claims and interests they involve, and his quest to find the most democratically just accommodation of those interests, probably does work against his analysis moving in sharply radical directions. Furthermore, his particular empirical characterizations and normative arguments can of course all be disputed. Still, there seems to me no question that this endeavor, unlike many exercises in more abstract grand theory, succeeds both in advancing our understanding of the possibilities and problems of democratic theory and our notions of how we might best respond to an impressive range of practical issues. If younger political theorists take works like Democratic Justice rather than A Theory of Justice as models for how to do political theory today, I believe political theorists will produce more works that clearly speak usefully to major political concerns of empirical social scientists, the broader citizenry, and also themselves.13

Even so, I continue to see much of value in the studies of the “great texts,” for many of the same reasons that Arlene Saxonhouse laid out in 1993. Though such comprehensive philosophic works always rely on a wealth of historical and empirical assumptions that their authors have never subjected to careful, rigorous
testing, I have already indicated that we must expect this to be true of any attempt to address political questions of great breadth, depth, and import. A major work of political theory, in my view, is one that rests on coherent and plausible yet distinctive characterizations of human beings, their political circumstances, varieties, and tendencies. It then builds on those premises a compelling if inevitably controversial vision of what human political life can be and should be. Very few writers are capable of generating a comprehensive account that at once seems, on careful examination, credible, yet also genuinely original in its account of humanity’s empirical and normative possibilities.14

Many of these works, moreover, articulate political visions that have in one way or another gained expression in both dominant and dissenting institutions and practices in many societies, past, present, and potentially future. That is why grand theoretical depictions of alternatives like Plato’s philosophically ruled and ordered city in speech, Aristotle’s prudential polity, Machiavelli’s tense political world of virtu and glory, Hobbes’s order-inducing Leviathan, Locke’s industrious and free bourgeois commonwealth, Rousseau’s demanding republic of free and equal citizens, Nietzsche’s harsh universe of clashing will to power, Rawls’s neo-Kantian “well-ordered society,” and other “great texts” remain fruitful sources of both empirical questions and insights and normative reflection (S. B. Smith 2000, 189–91). I think there will always be a place for scholarship that helps us grasp the array of alternative vistas of human possibilities opened up by such works, and such scholarship must always involve extensive, careful, informed textual exegeses. I also see much value in experts on writers like Plato, Rousseau, Kant, Locke, Hegel, and others helping us to think about how persons with the assumptions and normative commitments of those authors might perceive and appraise contemporary issues. I would like to see scholars of the history of political thought pursue such discussions of current problems through the viewpoints of major philosophers more extensively than they often do.

But if these sorts of political theorists are to undertake these tasks convincingly, today they face the same burden that formal theorists and other “constructive” theorists do. They must make themselves sufficiently knowledgeable about the contemporary issues they discuss so that when they suggest to us, for example, what the beliefs and values of Adam Smith imply about campaign finance, they can meet two burdens of proof. They must be able to convince us not only that they really know something about how Adam Smith thought, but also that they are really informed about the empirical realities and current debates on campaign finance reform.

Yet precisely because I see great works of political philosophy as in part extraordinary exercises in political imagination, evocations not only of what human existence is but what it might be if we appreciated its true character and potential more fully, I do not think work in “grand theory” need be confined to
the study of past greats or to informed efforts to work out what their visions suggest about current problems. Some deep features of the human condition may have long endured, but many things change over time; and already in this new century we are witnessing transformations that may eventually go deeper than any that have occurred in a long time. Many basic alternatives for conceiving of the character and goals of human life have long been profoundly explored and depicted, but others may exist or may arise. Already, the broad-ranging phenomena of globalization are sparking new theoretical efforts to imagine alternative institutional arrangements and political norms (e.g., Held 1995). Changes that are potentially even more radical are underway. I hope it will not seem too apocalyptic to say that we may well be on the brink of an era when cell-sized computers, implanted in us and almost everywhere else, will link human beings to vast stores of information and calculating capacities, to a bewildering variety of technological devices, and to each other, instantaneously and constantly. We may also be on the verge of experiments with and, eventually, improvements in genetics that will make our species as well as others significantly different than what the world has seen for the past few dozen millennia. We are certainly likely to see the proliferation of new forms of politically pertinent human organizations and associations transcending traditional governmental structures and posing challenges to those structures’ persistence.

I do not know how far or where all these trends are going, and neither does anyone else. But under these circumstances, I cannot help but think that the recent sense of political gridlock and stagnation prevalent among, at least, American academics will prove only temporary. It is possible, and I think it is certainly wise, to become more empirically informed on what is happening and where things may be going; but it is not possible to think very deeply about what the future may bring without having to rely on assumptions and speculations. All this is frustrating for the quest to achieve rigorous social scientific understandings of our circumstances. But these conditions are likely, I think, to make some forms of “grand theorizing” more useful once again, if undertaken by the relatively few among us genuinely capable of doing such work as it should be done. We may well find ourselves in need of imaginative new depictions of what the political lives, institutions, and dreams of human beings can be and should be as the twenty-first century and the extraordinary changes it promises proceed.

Again, I believe any such theorizing must accept the obligation to take as full advantage as possible of the wealth of empirical knowledge now becoming available. Impressionistic armchair theoretical speculations will probably not help anyone very much. But informed, insightful, and creative theoretical speculations may prove indispensable. We are, after all, now well beyond both 1984 and 2001. In important respects, the visions of George Orwell and Arthur C. Clarke, much less those of Locke, Rousseau, and Marx, now belong to our past,
not to our future. It may well be time to have some new compelling projections of what might be coming and what we might try to achieve as we enter a world perhaps more new, if not clearly more brave, than any our species has met or made for a very long time. We are not, I think, ever likely actually to escape our cave. But its walls are shifting; cracks are appearing; and beyond them some new horizons are beginning to come into view.

Notes
1. Without holding them at all responsible for what follows, I am grateful to Elizabeth Cohen for excellent research assistance on this essay and to Cohen, John Gunnell, Rogan Kersh, Robert Lane, John McCormick, Corey Robin, Ian Shapiro, and Steven B. Smith for useful suggestions. I have also benefited from the excellent commentary provided at the paper’s initial presentation at the 2001 APSA Theme Panel by Jack Knight, Arlene Saxonhouse, and William Galston and panel chair Nancy Rosenblum.
2. Let me stress at the outset that my analysis does not endorse any sharp distinction between “empirical social science” and “political theory” or “normative political theory.” Rather, I assume throughout that “empirical” work always expresses theoretical and normative assumptions and commitments and has theoretical and normative implications, and that every “theory” always embodies “empirical” claims about the character of human and natural realities. There is nonetheless a sociologically meaningful distinction between political scientists who are professionally categorized as “theorists” and those who are “empiricists.” My concern here is chiefly with how far the discipline’s “theorists” are engaging and should engage with the work of “empiricists,” though I think the analysis is also relevant to how far the latter should engage with the work of the former.
3. Both Jack Knight and William Galston made this point persuasively as commentators on the first draft of this essay at the 2001 APSA panel.
4. William Galston at the APSA panel and John Gunnell and Corey Robin in correspondence have properly stressed this distinction, present but not adequately highlighted in the essay’s first version.
5. These contributions of traditional “grand theories” have been appropriately stressed to me by Arlene Saxonhouse at the APSA panel and by Rogan Kersh and Corey Robin in correspondence.
6. See Finifter (1983, 1993). I have read some of the pertinent essays in a third volume of this sort (Katznelson and Milner 2002b); but they tend to be focused more on substantive issues and less on the evolving profession than their predecessors, so I do not discuss them here.
7. Brian Barry launched what still promises to be a multivolume “grand theory” of social justice in 1989, but so far he has chiefly produced critiques of other grand theories (Barry 1989).
9. Galston (1993, 27–53) analyzes the state of political theory via a three-part framework that includes its internal logic of theoretical development; its disciplinary locations,
intellectual and geographic; and the worldly political problems that are its subject. Here I combine his first two categories by analyzing the internal logic of political theory chiefly as an endeavor within the American political science profession.

10. This point is a variation on some of my earlier contentions, so let me clarify their relationship for any overly obsessive graduate students who might notice. I have previously argued that American political science has been shaped by a basic tension between achieving a political science that is genuinely scientific and one that clearly serves democracy (Smith 1998, 271–77). I have also contended that this professional tension is a particular national manifestation of an even more profound one, between desires to explain human conduct scientifically and desires to affirm meaningful human agency (Smith 1992, 6–11). That same deeper tension underlies the one I stress here. Our judgments that many political issues affecting human life are important are dependent, I believe, on some sort of faith in meaningful human agency. In American political science, that faith is often expressed specifically as belief in democracy.

11. See, e.g., Gunnell (1993, 199–250); Skinner (1985, 3–5); Ball (1993); Farr (1995, 211); Smith (1996); Lindblom (1998); and sources cited in these works.

12. Let me note, however, that other analysts of the history of political science have stressed that the discipline generally is most shaped by such external circumstances. See, e.g., Jones and Willingham (1970, 31–35); Dryzek and Leonard (1988, 1257); Walton, Miller, and McCormick (1995, 147).

13. Though Democratic Justice is in certain respects exemplary, there are many other recent works by political theorists that move commendably in these directions. Charles Taylor (e.g., Taylor 1993) and William Galston are probably the leading examples of political theorists who have actually become significant players in the politics of their societies, and Galston in particular has not only (like Taylor) extensively addressed policy issues but absorbed vast amounts of empirical research in doing so (Galston 1991; Galston and Baehler 1995). Bruce Ackerman has also moved from abstract "grand theory" to extensive engagement with policy issues, while continuing his seminal constitutional scholarship (e.g., Ackerman 1991; Ackerman and Alstott 1999), and Michael Sandel has also turned to history and policy issues (e.g., Sandel 1996). In their desire to address the contemporary problems of women and other disadvantaged groups, feminist scholars like Iris Young (1990, 1997), Nancy Fraser (1997), and Jane Mansbridge (1986) have long theorized in ways that are more directly politically and often empirically engaged than many contemporary "grand theorists." The problems of Quebec, especially, have inspired many Canadian theorists, including Will Kymlicka (1995, 1998) and Joseph Carens (2000) as well as Taylor, to address their theorizing rather immediately to contemporary quarrels; and the simultaneous rise of neoconservatism in several European countries and increased power for the European Union and other forms of transnational political organization have had similar consequences for the work of European theorists like David Held (1995) and Philippe van Parijs (1995) as well as David Miller (1995, 1999). The political embattlement of modern welfare states has also long won more empirically engaged attention from Australian and American theorists like Robert Goodin (1988) and Donald Moon (1988), among others. Like Miller, George E. Marcus and his colleagues are also pioneering more rigorous connection of democratic theorizing with empirical
And if I may be permitted yet another parochial observation, many scholars made sensitive to the political roles of ideas by the teaching and example of the late Judith Shklar have in recent years chosen to engage quite extensively with history, law, and political sociology. Works like Gutmann and Thompson (1996), Yack (1997), Smith (1997), Rosenblum (1998), Herzog (1998), Williams (1998), Holmes and Sunstein (1999), and Macedo (2000) are examples.

14. Here I agree with McCormick (2000) against Mayhew (2000a) that the contributions of political philosophy in articulating normative perspectives are of fundamental importance to political science.