The Evolution of Political Knowledge

Mansfield, Edward D., Sisson, Richard

Published by The Ohio State University Press

Mansfield, Edward D. and Richard Sisson.
Project MUSE. muse.jhu.edu/book/28455.

For additional information about this book
https://muse.jhu.edu/book/28455

For content related to this chapter
https://muse.jhu.edu/related_content?type=book&id=1180328
COMMENTARY

On the Rapprochement between Political Philosophy and Empirical Inquiry: A Comment on Rogers Smith

WILLIAM A. GALSTON

Rogers Smith’s essay is an admirably comprehensive and fair-minded story of the rise and decline of “grand” political theory, followed by theory’s nascent turn (back) toward empirical inquiry. I have no serious criticisms of the essay, considered in its own terms, as an exercise in the history and sociology of knowledge. I will therefore divide my comments into two parts: first, underscoring important areas of agreement with, while offering modest supplements to, Smith’s account; and second, going farther than Smith does toward a qualified defense of the rapprochement between political theory and empirical research.

Smith’s Story

Smith offers two important caveats (I agree with both) against an uncritical embrace of the reunion of research and theory. In the first place, there will always be a need for grand theory, both as a mind-opening exercise in political imagination and as a fruitful source of empirical questions. Indeed, Smith might well have said that the significance of particular empirical questions only emerges against the backdrop of general theories. The Baconian thesis that knowledge emerges through induction, through the heaping up of individual findings, is inconsistent with any plausible account of how successful processes of inquiry have operated over the past four centuries.

Second, Smith rightly underscores a tension between the precision of our empirical knowledge and the normative significance of the questions we are addressing. Hard knowledge is confined to clear but narrow propositions about “relatively minor political matters,” while our empirical grasp of larger questions is inevitably “softer.” Smith’s account calls to mind Aristotle’s famous remark toward the beginning of the Nicomachean Ethics that “it is the mark of an educated mind to expect that amount of exactness in each kind which the nature of the particular subject admits. It is equally unreasonable to accept merely probable conclusions from a mathematician and to demand strict demonstration from an orator” (and more broadly, from those who concern themselves with moral and political matters). The classic question of practical philosophy—
moral and political philosophy applied to human action—is, “What should I (or we) do?” In most cases, this question will require us to take a stand concerning some controversial questions of fact or prediction that cannot be resolved with certainty in advance of the proposed action. Consider, for example, the debate over the 1996 welfare proposals. Speaking in opposition, Senator Moynihan predicted that if those proposals became law, millions of women and children would shortly be sleeping on heating grates in the streets of our cities. If this prediction had turned out to be valid (there were some reasons to think it was, and no doubt Moynihan sincerely believed it to be so), it would have been a decisive reason to vote against the legislation. Those who voted for the legislation assumed, implicitly or explicitly, that Moynihan’s hypothesis was false. Neither side knew for sure, and (for Smith’s and Aristotle’s reasons) neither side would have been justified in demanding certainty as the basis of public decision. (There is room for debate about the proper distribution of risks in circumstances of uncertainty, but that is a different matter.)

Or consider the long-running debate over economic relations between the United States and the People’s Republic of China. Those who advocated more extensive ties often argued that trade would spur Chinese economic development, which would expand the middle class, which in turn would push China toward political liberalization and democratization. This argument, though framed by normative aspirations, rests on empirical propositions, some of which can be traced back to Aristotle’s *Politics*. While there is reason to believe that these propositions are true, we do not and cannot know that they are. We are compelled to build a policy that may prove to be of world-historical significance on an insecure foundation.

I would offer two modest additions, friendly amendments if you will, to Smith’s history. I would dwell, as he does not, on features of contemporary philosophy that tended to drive a wedge between normative theory and empirical inquiry. Three were of particular importance: belief in the so-called naturalistic fallacy, traceable to Hume; the tendency of British analytic philosophy to treat moral language and propositions as a freestanding arena of inquiry, independent of other concerns; and perhaps most important, the pervasive influence of Kant on leading moral and political philosophers, European as well as Anglo-American, during the late twentieth century. (In different but related ways, both John Rawls and Jürgen Habermas speak for a Kantian approach in which a wedge is driven between the empirical world and the basic structure of moral and political reflection.) In addition, I would be tempted to give more weight than Smith does to the potential impact on political theory of developments in the natural sciences. I have long been struck by the gulf that separated the natural and social sciences in the late twentieth century, because I have never understood how discoveries in the former could fail to affect our conception of the latter. I agree with
Smith that “the visions of George Orwell and Arthur C. Clarke . . . now belong to our past, not our future.” I would not say the same about Aldous Huxley; much of what he prophesied in *Brave New World* will be part of our early twenty-first-century reality, if it isn’t already. To the extent that we are biological beings, today’s breathtaking advances in biological understanding and biotechnical power are bound to exert enormous influence on tomorrow’s politics.

**Why the Rapprochement between Political Philosophy and Empirical Inquiry Is Desirable**

At the beginning of his essay Smith remarks that he will not spend as much time defending (as opposed to documenting) the shift of political theory toward empirical social science as some may have expected. In this section of my remarks, I will try to redress this balance to some extent. There are at least five reasons to applaud the apparent rapprochement of theory and empirical inquiry.

1. Many of the most important theses of grand political theory rest, implicitly or explicitly, on empirical propositions. To the extent that this is so, the truth-claims or validity of grand theory cannot be assessed without reference to findings that only empirical inquiry can provide. Devoid of empirical findings, such grand theses are at most contestable visions supported by aesthetic and rhetorical considerations. To the extent that we are critical of the Kantian theoretical assumptions that generate the sharp divide between normative and empirical propositions, our normative vision will itself be inflected by a range of empirical assumptions. We cannot hope to assess the force of our vision without rolling up our sleeves and engaging (as consumers if not producers) with the findings of empirical research.

2. Conversely, grand political theories are rich sources of empirical conjectures the significance of which rests on their place within a coherent argument. Both theory and social science are impoverished by the failure to investigate these theory-laden empirical hypotheses. As Smith notes, both economic and formal theorists have grown dissatisfied with the construction of ever more refined theoretical structures at the expense of testing the empirical propositions to which these structures give rise. Mutatis mutandis, the dissatisfaction makes sense for traditional political theory as well.

3. The political meaning of a normative vision cannot be detached from the institutions needed to make that vision effective in the arena of collective decision and action. It follows that we cannot assess the vision without examining the functioning of the institutions to which it is linked. For example, Aristotle
assessed Plato’s vision of communal property by considering the real-world consequences of collective ownership. In a similar spirit, Jane Mansbridge has brought the dream of participatory democracy face to face with real-world observations of participatory institutions.

4. Political theory must meet a condition that I call minimum motivational realism. This does not mean that theorists must simply take human beings as they find them, but it does mean that the rigors of complying with proposed norms and practices must be within the trained capacities of most citizens. (Rawls has something of this sort in mind when he speaks of the “strains of commitment.”) We cannot hope to draw the line between reasonable and unreasonable motivational expectations without reflecting systematically on the real-world experience of human conduct, in part through the disciplines of political and social psychology.

5. Considered as a guide to practice, the kind of political theory that does real work is non-ideal rather than ideal, that is, normative assessment in circumstances in which the conditions for best practice are not met. Even if one believes (as some do not) that ideal theory is a meaningful enterprise that should serve as the point of departure for nonideal assessment, it is hard to see how we can move from the former to the latter without examining empirical propositions. Assume, with Rawls, that a certain level of material well-being is part of the circumstances of justice, absent which distributive principles suited to the ideal case must be relaxed. Relaxed in which ways, by how much, guided by what sorts of midlevel principles? Nonideal theory is a form of casuistry, which is an inherently fact-laden enterprise.

Conclusion

Smith praises the great works of political philosophy as “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 agree. At the same time, the history of the past two centuries suggests that unchecked by a realistic sense of the limits of individual and institutional malleability, the exercise of imagination can lead to unspeakable brutality undertaken in the name of a glittering but phantasmal political vision.

There is no algorithm by which we can neatly distinguish between productive aspirations and destructive delusions. It is clear, however, that any responsible effort to define and defend this boundary is in large measure an empirical matter. The exercise of political imagination, of visionary theory, is not self-validating.
COMMENTARY

The Mutual Engagement of Political Theory and Empirical Research

JACK KNIGHT

After participating in the 2001 APSA panel on Rogers Smith’s excellent paper on the apparent trend among normative political theorists toward greater engagement with empirical social science, I had the opportunity to serve as chair of the selection committee for the 2002 David Easton Prize. As Smith notes in his paper, the Easton Prize was established in 1997 by the Foundations of Political Theory Section of the APSA in order to honor outstanding works of political theory that explicitly incorporate empirical research into their analysis. From that experience I learned three things relevant to the primary topic of Smith’s argument. First, the trend is real, at least it appears to be the case if the large number of books that members of the discipline deemed eligible for the award is any indication. Second, and related to the first, judgments about what constitutes both “political theory” and “serious engagement with empirical research” vary widely. That this is the case is not surprising given the sheer size of the profession and the ambiguity that characterizes the use of the label “theory” in political science, a fact duly noted by Smith. That this has been the case for some time is reflected in the variety of books that have received the award, from Jürgen Habermas’s all-encompassing account of facts and norms to David Laitin’s painstakingly detailed analysis of language politics. Third, and probably most important, doing this well is very hard to do.

And yet, as Smith documents in his paper and as our committee deliberations on the 2002 award further indicate, many political theorists from various theoretical perspectives and differing ideological persuasions are trying to seriously engage relevant bodies of empirical research in the course of constructing their normative arguments. And the results, while mixed, are, on the whole, very promising. What explains this trend is the major focus of Smith’s paper. I find his basic argument convincing. Without dismissing factors internal to our discipline, Smith argues that the most important factor explaining changes in the intellectual pursuits of political theorists is the political environment in which we work. That these circumstances changed from the 1960s and early 1970s, when what Smith calls grand theory was in vogue, to the period at the end of the old century and the beginning of the new is clear. That changes in the levels of both ideological conflict and political optimism might explain the trend towards more theoretically focused and empirically engaged political analysis makes a lot of
sense. And so I see no reason to use this brief commentary to quibble with Smith about some marginal details of disagreement. Rather I want to take this opportunity to push his argument in two directions, one to further clarify a pervasive confusion that seems to dominate contemporary discussions of the discipline of political science and the other to reassert the central role of normative political theory in that discipline.

At the outset of his analysis Smith offers an important distinction between engagement with empirical research and engagement with important, substantive political problems. He suggests that while both forms of engagement are highly beneficial, he would place greater value on engagement with substantive political problems. That, he contends, is what political theorists ought to be doing. I agree. The primary task of a political theorist ought to be that of addressing the major substantive political questions facing the society in which he or she lives and works. The ability to address such questions in an informed way is our comparative advantage to the extent that we have one. My own preference is for theory which actively engages both of these dimensions because I find that normative theory which explicitly incorporates the best information and knowledge that empirical research offers makes for a more compelling and persuasive argument. But note that this is really a question of method.

There does not appear to be any necessary connection between the two forms of engagement. There is no reason, in principle, to hold the view that grand theory is any less able to address important political questions than is a more focused approach. Both are capable of addressing substantive political questions; they differ in terms of the level at which they cut into the analysis. Consider, for example, the challenge that social diversity in its various forms presents for a democratic society. Two theorists that Smith takes as exemplars of the grand theory versus practical focus divide, John Rawls and Ian Shapiro, have spent much of their intellectual efforts on addressing the normative implications of that challenge. Both of their works can, in my view, be interpreted as efforts to assess the problems of institutionalizing democracy in the face of fundamental conflicts of values and beliefs. However, they differ in their levels of analysis, Rawls at the level of basic constitutional principles and Shapiro at the level of actual democratic practice, and this leads, in this case, to differences in their normative claims and prescriptions. In the end, the relative merit of their work rests on the differences in how they guide us in the pursuit of democracy in a socially diverse world and not on the differences in their methods. More generally, whether the most beneficial answers to our substantive political questions will emerge from theory engaged with empirical research, as I suspect, or from purely abstract theory is a question that only time and the competition of ideas will ultimately answer.

Moreover, the distinction between method and substantive focus has important implications beyond debates about the proper approach to normative theory.
In the last few years there has been a major controversy, popularly labeled the “Perestroika” debate, over the nature and direction of research in the political science discipline. The initiators of the controversy leveled a broad attack on the alleged dominance of formal theory and quantitative analysis, arguing against its abstract and ahistorical focus. They argued for the superiority of other methods, what Smith labels “softer” methods, which emphasize the importance of interpretive meaning and historical detail. From this initial challenge the debate commenced throughout the discipline, taking many forms and producing much intellectual and professional engagement. In my view this ongoing controversy has been and remains a highly beneficial one and the advocates of “Perestroika” have raised a number of significant and valuable points. But, this debate has been couched in terms of method, when I think it is really a debate about the substantive focus of our discipline. Closer attention to the distinction offered by Smith in his paper would show that the real debate in the discipline is over the relative value of substantive focused political analysis as opposed to analysis that seems disengaged from contemporary political concerns. The excessive pre-occupation with methodological differences obscures the real issue: the substantive relevance of contemporary political science, regardless of whether the particular focus is on normative theory, positive theory, or substantive empirical research. Here the concerns raised by Smith in regard to the relevance of political theory translates into similar concerns about the discipline as a whole.

Smith offers the following description of the products of our collective labors: “Whatever our aspirations to achieve empirical support for rigorously deduced hypotheses, in fact the most we can 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 open 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.” Many (and I am not really sure where Smith himself actually stands on this) take this claim to support arguments about the inherent superiority of certain of the “softer” methodological approaches. Thus, abstract theory out, history and interpretation in. Who knows whether this may prove to be a fruitful strategy in the long run, but we surely do not have the evidence to draw these conclusions at this point in time. No one, to my knowledge, especially in the recent debates, has been able to make a persuasive case for the inherent superiority of any particular approach. The case has not been made by the historians and it has not been made by the advocates of rational choice.

As a philosophical pragmatist, it seems to me that the best that we can say is that different methods are more or less appropriate for different aspects of the task of answering the important substantive political questions that face us
today. And, while this might sound either pathetically trivial or weak in methodological conviction, there are as yet no compelling arguments for rejecting the possibility that we can gain important knowledge from the approaches on both sides of the “Perestroika” divide. If there is a real message to be drawn from Smith’s description of the discipline of political science, it is not about method but rather about the status of our explanatory and normative claims. All of our explanations, regardless of approach, are inherently fallible. Acceptance of this fact should increase the humility with which we conduct our research and posit our explanatory claims. This does not suggest that we cannot be analytically rigorous, methodologically systematic, and theoretically demanding if we so choose. But it does suggest that we must always be open to the challenges of others and willing to reconsider our most settled beliefs. And it further suggests that we must be open to the possibility that the relevant challenge may come from unexpected sources and from quite different methodological perspectives. If we would concentrate our energies on establishing such tolerance and openness across these methodological divides (leaving the salutary methodological debates to those which are internal to a particular approach where they might produce real intellectual progress), we could shift the focus of our intellectual debates from method to questions about the substantive relevance of the discipline as a whole.

And this is where my second point comes in. Debates about the relevance of political science research will almost invariably come around to questions about the fundamental value of the findings and conclusions of that research. Although one of the main issues will of course be the criteria by which we assess fundamental value, it is hard to see how such a debate can take place without significant reference to the benefits that the research provides for the resolution of the major substantive political problems which we face. Here normative political theory has a central role to play, reflecting an intellectual place that it has long held in theory and in fact, even if it has been increasingly lost in practice in our efforts to establish the scientific study of politics. For it is the substance of normative theory that provides both the motivating questions and the intellectual justification for the scientific analysis of politics. The most compelling reasons for studying politics in its various forms are to be found in questions about governance, social order, ethics and justice, the rule of law, international peace and cooperation, and the like. This is true whether the object of our analysis is a voter, a political party, a bureaucracy, a court, an interest group, nations in conflict, political associations, or the myriad of political issues surrounding ethnicity, race, and gender today.

To say that normative theory sets the underlying agenda for both positive theory and substantive empirical research does not imply that normative concerns will determine the nature and substance of our positive conclusions and findings.
This is not a claim relevant to the old debate about "value-free" social science. One can be scientific in the study of politics if one wants (at least by any reasonable standard of the profession) and still accept the basic thrust of my claim. For it is ultimately a claim about justification: normative theory gives political scientists a reason for doing what they do. The knowledge generated by both theoretical and empirical research in political science is valuable to the extent that it helps us to understand the political problems facing us and to develop solutions to those problems. If we as a discipline are going to be able to establish and maintain our relevance beyond our own professional boundaries, then we must at some point turn to normative political theory as an important source of that effort.

What this implies is that the organizers of the APSA panel on which Rogers Smith’s paper was originally featured only emphasized one aspect of the necessary engagement between political theory and empirical research. For, while political theory should, for all the reasons that Smith and others have argued, engage empirical research in an ongoing and systematic way, it is equally important that empirical research engage political theory. It is an artifact of the present distribution of power in political science departments around the country that the burden has been on political theorists to engage empirical research in an effort to justify their continuing relevance within these departments. If we ever have a serious debate on the contemporary relevance of the discipline as a whole, the terms of engagement may be quite different ones.
COMMENTS ON ROGERS SMITH
RECONNECTING POLITICAL THEORY TO
EMPIRICAL INQUIRY, OR, A RETURN TO THE CAVE?

ARLENE W. SAXONHOUSE

Rogers Smith has written a wide-ranging reflection on the status of political theory today and yesterday chronicling the demise of grand theory from the 1960s and 1970s. Rather than bemoan that demise, he urges us instead to move in new directions, most specifically to descend into the cave, the world of multiple experiences and of politics. Smith repeats a fear familiar after the Second World War about the totalitarianism of grand theories. At the same time, he is attracted to the “messiness of politics.” Underlying his paper is a worry that theory means an enslaving order, while politics and empiricism allow for disordered freedom. Thus, he urges theorists to welcome the messiness of politics and to be wary of grand theories. He analyzes historically, sociologically, and even psychologically the splits and reconnections between political theory and empirical work in the discipline, applauding the reconnections, suggesting that the future of political theory resides in political theorists’ willingness to enter and learn from the messy empirical world of politics.

With such exhortations, Smith reminds me of the advice I received when I was an assistant professor in the early 1970s. It was a time when, as Smith describes it, political theory was embattled by the so-called behavioral movement and supposedly relegated to the garbage heap before the altar of empirical (usually meaning quantitative) research in political science. The then-chair of my department, in an effort to give me what I think he intended as fatherly advice, recommended that I work on “building bridges” to the rest of the department. I, a theorist studying Plato and Hobbes, was an anachronism. Without realizing it, this fatherly chair was playing the role of Callicles in Plato’s Gorgias or Adeimantus in the Republic in viewing as useless the philosopher who was unfamiliar with the pitfalls of political life. For Callicles, philosophy was a pleasant diversion appropriate for young boys. In the parable of the boat introduced in response to Adeimantus’s worries about the philosopher-ruler in Book 6 of the Republic the philosopher is “a star-gazer” who would never know how to steer the boat. Though my chair was neither as bold as Callicles nor as blunt as Adeimantus, the implication was that a theorist who spent her time reading the classic texts should figure out how to reach over to the empirical and quantitative world of my colleagues by building the bridges to them. I was a distant island; they were the mainland.
Untenured at the time I did not reply as I would have liked, saying that I thought his metaphor of bridges was completely wrong-headed and misguided. I wanted to say I should not need to build bridges. Rather, I am the foundation upon which the rest of the discipline is built. Smith is certainly not suggesting that the philosophy is useless, or, in Callicles’ words, something “graceful” for when we are young, but which corrupts if we spend too long on it. And yet, in some ways, Smith is giving us the same sort of fatherly advice that my former chair did with the language of “reconnection” replacing the metaphor of bridges. Smith finds in this reconnection a salutary development in political science which allows him still to praise (just a bit) the grand theories and to urge that we not completely abandon them. I wonder, though, first whether the language of bridges and reconnections is the most helpful and then whether the connections were ever lost. The civil engineering metaphor may be appropriate, but I am more concerned about what this engineering project will design—bridges or foundations. Smith sides with bridges, I with foundations. Nevertheless, we both resist the implication that theory can stand on its own, that the star-gazer can solipsistically gaze upward. There never could be the isolation between the two parts of the discipline that is part of a common mythology and, as Smith points out (Smith p. 86, n.2 in this volume), has unfortunately been institutionalized in departmental job searches and in disciplinary journals.

When I thought of political theory as the foundation for (not the bridge to) my colleagues’ work, I thought of it as defining what these more empirically oriented scholars should be interested in in the first place: Why should they care about what congressional committees do or do not do? Why, if they were to be anything more than political consultants, should they study whether Catholics voted one way and Jews another? Why should they spend time with elaborate content analyses to study the role of the news media in framing issues? What were all those numbers for anyway? This was me wishing to say to the empiricists, pay attention to us theorists for without us your work simply destroys our forests.

Smith is urging us to look the other way to see how the empirical world informs the political theory we do. I wonder, though, whether he is not affirming a greater dichotomy than there ever really was. On the one hand, empirical work has never been theoretically or normatively barren nor can theoretical work have any power without empirical foundations. Smith’s history goes back to the late 1960s and 1970s and the appearance of Rawls’s path-breaking book A Theory of Justice, but of course the history of the relation between political theory and political science goes much further back.

If we start with the origins of political science departments in universities in the United States in the mid-nineteenth century, we find the Germanic influence and especially Hegel with his understanding of the “state” appealing to a nation
grappling with the challenges to concepts of sovereignty and nationhood posed by the Civil War (see Burgess 1994). The Germanic influence meant that political science departments devoted themselves largely to the classic texts in political theory as a way of addressing questions of comparative regimes and their constitutional status. Even Charles Merriam, known as the father of behavioralism, wrote books entitled *History of Sovereignty since Rousseau* and *A History of American Political Theories* (see Karl 1974). While his students may have collected the quantitative data intended to make governmental bureaucracies more rational, they did so within the normative guidelines of the progressive movement, eager to address economic and political inequalities, a fact that raises some questions about the now familiar readings of the behavioral movement as defending a status quo. The claimed opposition between political theory and political science has less to do, I would argue, with the emergence of the behavioral movement than with the impact of a positivism that resisted the normative judgments motivating the early work of the behavioralists.

It was the move to an unalloyed positivism that alienated the political theorists, particularly those who had fled Germany like Arendt and Strauss. Such theorists brought to the United States a continental philosophy, grounded in phenomenology, that questioned the positivism adopted by the midcentury practitioners of American political science. For Arendt, statistics, eroding the individuality of the particular human being, recalled the totalitarianism of fascism and for Strauss attention to the sub- and pre-political cheapened the agenda of the discipline.

In contrast to some of the tone of Smith’s argument, neither Strauss nor Arendt was constructing a “grand theory” abstracted from the empirical world of politics. Each was deeply enmeshed in the experiences of the mid–twentieth century. And it was the horror of such experiences that gives so much resonance to their writings. This makes problematic Smith’s claim that Strauss and others like him “did not address very directly many of the major political issues of their day” (Smith, p. 74 in this volume). Indeed, they did address the “major political issues of their day” by looking at the foundations, exploring the limits and possibilities particularly of democratic institutions and liberal regimes. When Strauss wrote his infamous comment about the “new political science,” that “one may say that it fiddles while Rome burns [but political science] is excused by two facts: it does not know that [it] fiddles, and it does not know that Rome burns” (Strauss 1962) he was speaking of the failure of political scientists of his time to look beneath their studies for the context that could give meaning to their work. He was not dismissing the empirical work, but rather condemning the failure of empirical work to recognize the way in which it was conceived and done. Nor was he discouraging theorists from attention to the world of experience. It was the empiricists who did not see the very world they
COMMENTARY ON SMITH

were experiencing, who had tried to remove from the study of politics that which made it a core experience of human life.

Smith’s agenda is in some ways similar to the demands for relevance familiar to those of us who taught in the 1970s, when theorists had to justify teaching Thucydides with reference to the Vietnam War or the Apology and Crito with reference to civil disobedience. I question, though, whether theorists then or now resist the messy world of politics to the degree that Smith suggests. Smith tends to lessen the engagement of some authors such as Strauss and Arendt (and I would add the unmentioned Judith Shklar here) by failing to acknowledge how their writings in quite distinct ways respond to the traumas of fascism that they all experienced. Those traumas explicitly evoked the exposition of theories that were meant to help us resist circumstances that might allow for the reemergence of totalitarianism. These theorists were well aware that Rome was burning, but did not think that the fires could be quenched without theoretical insights into what had caused the conflagrations around them.

The language of “reconnection” that Smith uses recalls for me the metaphor of “bridges” that I had reacted to so strongly earlier in my career, but let me change metaphors and turn to the one that Smith uses in the title and the conclusion of his paper—the cave. Smith proposes a return to the cave on two grounds: (1) à la Gunnell (1983, 1986), as the demand that theorists engage in the messy world of politics rather than stare at the stars; and (2) as the locus of the empirical work on which theorists should ground our activities. The image from Plato’s Republic is a potent one that has seared itself into our intellectual imagination. The cave is dark, illuminated only by fires well up the cave behind a wall. The fires playing off objects carried by puppeteers cast shadows on the walls. The inhabitants of the cave sit, bound, looking only at the walls, seeing only shadows. Deep in the cave, in this realm of shadows, is the world of politics. This land of opinions is no more than Hades with spirits wandering aimlessly in the afterlife.

In Socrates’ parable the philosopher is forced to break from his/her chains and ascend out of the cave, past the fires, into the sunlight. The philosopher learns to recognize the shadowy world for what it is. And yet, in one of the great puzzles of the Republic, the philosopher is forced back down into the cave, becoming the laughingstock for those chained to see only shadows. Smith follows Gunnell and others in calling for and applauding the theorists’ descent into the cave, into the political life of the city so that the theorists acknowledge that, whatever their endeavors may be, they are grounded in the needs and experiences of the cave.

Smith, though, is saying something more important than that we should simply go down and “engage” in the world of politics. This is the second part and more difficult use of the cave metaphor. Whereas Socrates’ philosopher is pulled
by someone out of the cave, Smith suggests that the experience of the cave itself leads to the contradictions that take us up out of the cave, that the tensions in the world we see around us demand our ascent. Only by studying the cave can we become aware of the need for theory, Smith seems to suggest. But we are caught here in a conundrum: how do we recognize the contradictions without a theoretical framework? How does political engagement lead to change and improvement without the theoretical understanding that allows us to see the contradictions? How do we have access to the concepts of inequality or international justice without having an initial understanding of those concepts we wish to apply to the shadows we experience? “Wisdom” may not come from enmeshing ourselves in the political world of shadows, but from the prior familiarity with the world outside the cave, a world that enables us to state that this is unjust or unequal or antidemocratic.

Socrates presents the image of the cave in part to clarify the far more challenging image introduced in Book 6 of the Republic of the divided line, another metaphor offered in an effort to articulate a framework for understanding our capacity to know. At the bottom of the divided line are shadows and reflections which we perceive, Socrates tells us, with our capacity for imagination. The shadows and reflections, in turn, depend on the “things” which create shadows and which themselves change over time. This is the empirical world to which we are being encouraged to reconnect. This world of “things” known by our senses has no inherent order. It is only when we jump over the line from perception to thought and move to what is not subject to the senses that the objects of perception can be put into any framework and become more than just one sense experience after than another.

Socrates deserts us when he does not fully reveal what is entailed in jumping over the line except to say that we move from using our senses to relying on our mental capacities. Here, though, is the crux of the issue concerning the interrelationship between the theoretical and the empirical: How do we get over the divide? The issue for Socrates is not whether we should. If we do not, we are simply the slaves of our senses with no capacity to arrange and order. If we do, if we insist on going beyond our sense experiences, we find a cognitive and normative order—cognitive in the sense that we can see similarities and differences between one tree and another, between one political regime and another, since we know now what a tree, what a political regime is, and normative because in Socrates’ model we make the grander—and more obscure—step of seeing the good and the beautiful by which we can judge the tree and the political regime according to whether it reflects that form of the good and the beautiful. Central to the epistemology of the Republic is jumping over the divide, switching from sense experience to the mental experience. It is the latter that gives us the foundations for organizing cognitively and evaluating normatively the objects of our senses. To the dismay of his interlocutors and the readers of Plato’s dialogues,
Socrates does not tell us how to accomplish this jump apart from some obscure discussion concerning the study of dialectics and ungrounded hypotheses.

Reflecting on the Republic as an often-read book may be of some help here. It purports to tell us the story of Socrates’ night in the Piraeus at Cephalus’s house. It is a reflection through the Socratic narrative of the events that took place the night before. As an object, the Republic which we read lies on the very bottom of the divided line and yet through our engagement with the book we are supposed to be able to ascend and jump over the divide to the mental process that will lead us to conceptions of the good. The starting point is the cave, images like the Republic, then the senses that make us aware of our own surroundings. But we do not stop there. Plato, so often tarred as an idealist philosopher who scorns the empirical world of the senses and the body, has a similar message, at least in the Republic, to Smith’s. The Republic already teaches that we must use the senses before we can theorize; it is the effort to create order in the world we experience that leads to the theoretical leaps. Smith toward the end of his paper pays brief homage to the great texts of political theory and perhaps reflection on the cave and the divided line in the Republic indicates how political theory as the reading of texts can be a foundation for political science to understand some of the more problematic issues underlying its efforts.

I want to defend a reading of texts as a practice of political theory but I would resist a proposal such as Smith makes that we turn to Adam Smith for insights into the issues of campaign reform (Smith, p. 84 in this volume). Such recourse to the classic authors can lead to humorous results. What would Plato say about stem cell research? Nothing; he couldn’t even imagine it. But he might be able to help us address the issue of how we define a category such as “human” or address issues of technological responsibility. The classic texts raise for us questions which force us up out of the cave into the concepts that we can only find on the distant side of the divided line. Smith is appropriately concerned that the close readings of texts often just “rehashes the same lessons” leading to “repetitive conclusions” (Smith, p. 80 in this volume). The lack of originality in some work, though, does not mean that all is to be discarded.

The allusion to the cave in Smith’s title and conclusion may have been a casual gesture on his part, an effort to draw his readers together through a familiar image. It appears only in the title and the last page, so I may have made too much of it, but it captures well what Smith is trying to articulate and maybe through its own inability to show us how the ascent is made without the forceful dragging up or an undefined jump across a divided line, it points to a far deeper challenge, to the question of how the recommended reconnections are to be achieved. Smith is upbeat about the potential and the models of such work as he proposes; I am still perhaps mired in the challenges Socrates posed more than 2,500 years ago, wondering whether we have begun adequately to climb out of cave; if that is the case, can we even talk about a return?