“In Search of General Theory”

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Abstract

Most sub-fields of research in political science are today at an intellectual plateau well short of general theory. Many have been at that plateau since about 1980. Several reasons might account for this situation, including the challenge of constructing general theory. I argue, however, that some of our must common educational and research practices also retard theoretical progress. I describe those practices and their unfortunate consequences, but also explicate a series of research strategies that would help advance our theoretical work. As a foundation for the preceding arguments, I characterize the theory building ambitions of the discipline, our progress toward general theory, and how advances toward such theory can be mapped for any science.

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"In Search of General Theory"

I argue, and explicate the argument, that most of the scientific study of political science is at a plateau of advancement well short of the goal of general theory. Further, the fields in which I read for teaching or research appear to have been at that plateau since about 1980. Some research areas are more advanced than this, but very many fields in our discipline appear to be at this same plateau. Doubtless, we know much more today factually and by way of exploratory and some theory building research than we did thirty years ago. But that does not mean our theory has necessarily progressed much in that interval. This state of intellectual affairs is surely attributable in part to the difficulty of constructing a general theoretic formulation of some persuasiveness. Yet I also argue that we are ourselves responsible in part for this situation because of how we often educate young scholars in the state of received knowledge, because many of our common research practices do not contribute to the search for general theory, and because we neglect ones that could be more profitable.

I criticize much current research practice in political science in support of the preceding argument. The reach of these criticisms is broad. Indeed, my own work has suffered from most of the shortcomings identified here. The primary goal of this paper, however, is to suggest educational practices and research routines that might advance our efforts to create general knowledge. If the criticisms here have wide applicability, the ambition of this paper is that the solutions for overcoming them will be equally widely embraced.

Three fundamental topics must be considered, however, before discussing these possible solutions. Thus I first characterize the theory building ambitions of the discipline, our progress toward general theory, and how advances toward that goal can be mapped in general terms for any science. When enough of those fundamental topics has been explicated, I also offer a definition of general theory.

Theory Building Ambitions in Political Science

One could simply take it as a universally recognized assumption that theory building is a primary goal of our discipline. Our leading journals commonly cite theoretical contributions as a major criterion for publication. Our textbook expositions of "how to do" political science, for both undergraduate and graduate students, make that assumption. It is a key part of graduate education in most doctoral programs. And the intention to advance theory is virtually a ritual observation in most scholarly papers in political science. Yet deliberate consideration in print of the character or promise for this ambition is rare. Perhaps the latter circumstance can be explained, for good or ill, by Kuhn's (1996, 47; see also Northrop 1949, 389-391) observation:

> "Though many scientists talk easily and well about the particular individual hypotheses that underlie a concrete piece of current research, they are little better than laymen at characterizing the established bases of their field, its legitimate problems and methods. If they have learned such abstractions at all, they show it mainly through their ability to do successful research. That ability can, however, be understood without recourse to hypothetical rules of the game."

Some comments on the character of our theoretical ambitions will be useful as a foundation for much that follows in this essay. Yet, absent much explicit discussion of this topic, one must infer the disposition of the profession about it largely from what is implicit in its educational and research materials. Principally on the latter basis and from wide discussion with scholarly peers I conclude that three assumptions about our theory building ambitions are widespread, even if they are often held sub-consciously. The first is that our highest ambition is, indeed, to create general theory about real-world political phenomena. This first assumption and the "philosophy of science" associated with it are still today well characterized by the fundamental beliefs that David Easton (1965, 6-8) articulated for the behavioral movement in *A Framework for Political Analysis*:

(1) "There are discoverable uniformities in political behavior. These can be expressed in generalizations or theories with explanatory or predictive value."

(2) "The validity of such generalizations must be testable…,"

(3) "Means for acquiring and interpreting data cannot be taken for granted. They are problematic and need to be examined self-consciously, refined, and validated…,

(4) "Precision in the recording of data and the statement of findings requires measurement and quantification, not for their own sake, but only where possible, relevant, and meaningful in the light of other objectives,"

(5) "Research ought to be systematic, that is, theory and research are to be seen as closely intertwined parts of a coherent and orderly body of knowledge,"

(6) "Because the social sciences deal with the whole human situation, political research can ignore the findings of other disciplines only at the peril of weakening the validity and undermining the generality of its own results."

The preceding list of beliefs and objectives should sound especially familiar to many contemporary political scientists because items 1 through 5 of that list are all essential points in King, Keohane, and Verba's (1994, 3-33) initial chapter on "The Science in Political Science." And observing the commonalities between Easton's and King, Keohane, and Verba's work is no discredit to either. It testifies, instead, to the enduring centrality of these objectives.

The second common assumption is that most of our scientific work is guided by a "neopositivist" philosophy of science that, as Maxim (1999, 10) best observes, "retains the crucial strengths of the traditional scientific method" of logical positivism but without the "simplistic assumptions" also associated with the latter perspective (see also Ostrom 1982). This assumption also means that many political scientists envision that an advanced theory would be stated in something like "hypothetico-deductive" form (Maxim 1999, 21-22). This second assumption is rarely stated forthrightly but is obvious in much work with theoretical ambitions if one "reads between the lines" of what is said forthrightly. At the same time the discipline does not hew to any particular formulaic notion of how covering law theory is to be explicated, a subject to which I return later.

The third commonly held assumption is that political science shares with the physical sciences the same basic research philosophy, general research practices, and optimism for explaining the portion of the natural world we study. Explicit recognition of this third point is rare but is made in contemporary times and in differing ways by Alt (2009, 146-147), Hill (2004), and Rogowski (1995). King, Keohane, and Verba (1994, 7-9) implicitly endorse this point in their list of the essential characteristics of social science research. Kaplan (1964, 30-31) and Deutsch (1986, 5-8) asserted this second assumption, too, and much earlier, especially with respect to the research methodologies we share with the physical sciences.

While these assumptions are commonplace among working scientists in our discipline, they have long been contested by some philosophers and even by some scientifically minded political scientists. The belief that one can account for phenomena in the political world with research based on the same assumptions and methods used by physical scientists has been especially criticized on logical and normative grounds. For good summaries of some of the major lines of criticism here, see Almond (1998) and Diesing (1991). To my taste, however, Easton (1953, 3-36) and Nagel (1961, 447-502) long ago debunked the logic behind most such criticisms.

Other critics see in the preceding assumption as well an unwarranted elevation of some research methods over others (e.g., Schram 2005). Yet at the heart of some of these critiques are also often different opinions from those of many scientists about what scholars wish to explain about the political world. Whatever the philosophical or other merits of these criticisms, the community of working scientists appears at least implicitly to have accepted Kaplan's (1964, 3-33) argument that there is a defensible "logic in use" of our scientific assumptions and methods regardless of how other communities of scholars view them. Or one might conclude with Einstein (1954b, 19) that most working political scientists put aside unhelpful characterizations of science by "the philosophy police."

There is also notable criticism of some of our conventional research methodologies and practices from the statistical community and from statistically sophisticated political scientists (see, especially, Achen 2002, Brady 2008, and Freedman 2010). The latter criticism initially focuses on the methodologies with which we collect and analyze the non-experimental data that is employed in most of our research. Ultimately, however, such criticism concerns the nature of the hypotheses and theory about the natural world that can feasibly be tested with non-experimental evidence. Thus it raises doubts about all three of the common assumptions for our theory building ambitions. It is presently unclear, however, how the discipline will respond to these concerns. My reaction to many of these criticisms is that they depend too heavily on philosophical and statistical logics and give insufficient attention to how optimal *scientific* research practices can help overcome the challenges of working with non-experimental evidence.

Marini and Singer (1988) offer an especially good sketch of such optimal scientific practices and rationales for them, yet portions of that sketch appear in methodological discussions by political scientists, as well.

There is also lively discussion within the discipline about what research practices can most efficaciously lead to scientific theory, especially with regard to the separate merits of qualitative and quantitative methods (e.g., King, Keohane, and Verba 1994; Brady and Collier 2004). This body of methodological work, in contrast to the preceding one, has attracted a wide audience and, thus, appears to be potentially highly productive. It has the prospects of improving scholarship that relies on many different kinds of research methods, encouraging especially efficacious multi-method research, and bridging long-standing divides between scholars from different methodological schools. Yet the test for this line of discussion will be how deeply its contributions sink into the discipline.

What General Theory Is

Consider a few prominent theories in our discipline – all of which have been especially influential in their respective sub-fields – that will be discussed in more detail later in this essay. Depending on whether one would credit Milbrath (1965, 110-141) or Verba and Nie (1972, 125-137) for the formulation, at least by 1972 research on mass political participation in the United States had accepted the inductively derived socio-economic status (SES) theory as the "standard model" or theory to account for such behavior. A voluminous body of subsequent scholarship has appeared on political participation and related topics that employs this standard model as its theoretical foundation or that attempts to advance the model itself. Yet the SES model has also bedeviled scholars in this field, because it has very high explanatory power but does not clearly identify underlying causal mechanisms (Verba, Scholzman, and Brady 1995, 280-282). We obviously know today much more about the determinants of voting and other forms of political participation by the mass public, but I conclude that we are no closer to a general theory than we were in 1972 (based on criteria that are explicated below).

The study of the U.S. Congress has profited by the creation of several competing theories of lawmaking, including Cox and McCubbin's (2005) procedural cartel theory and Krehbiel's (1998) theory of pivotal politics. Another such theory, for conditional party government (CPG), arose out of inductive research by Cooper and Brady (1981), Brady, Cooper, and Hurley (1979), Rohde (1991), and Aldrich (2011). CPG theory has especially attracted many adherents who have contributed to its development. But, while "conditional party government" was first identified as a goal of reformers in the U.S. Congress with notable theoretical implications by Rohde (1991, 31-34) some twenty years ago, the theory surrounding that concept has not been systematically articulated in the ways that science conventionally expects for a general theory.

The study of national political leaders and national policymaking more generally has led to the creation of a range of theoretical paradigms. One of the most notable is the "selectorate theory" advanced by Bueno de Mesquita, Smith, Siverson, and Morrow (2003), that appears to have been derived by a combination of inductive and deductive reasoning. This intellectually ambitious, highly abstract theory offers an explanation for how the mechanisms by which national political leaders are chosen produce incentives for the kinds of domestic and international policies they will pursue. This is the most sophisticated of the three theoretical formulations I use as examples – at least with respect to how the theory has been formulated, but it does not appear ready either to be designated an unqualified general theory.

Why are these and many other theoretical formulations in political science deficient as general accounts of relevant behavior? Consider what we mean when we use the term general theory *and* the degree to which we have crafted such intellectual products. There is broad agreement in the scientific literature, first, that general theory should be stated in a relatively complete, descriptive way. Yet there is no consensus in the scientific or the philosophy of science literature on the optimal form *or* nomenclature for stating such a theory (see, as some example expositions, Blalock, 1969, 1-12; Gibbs 1994, 279-364; Hempel 1965, 331-354). Since the articulation of advanced theory can be seen as a creative step, some discretion about form seems appropriate. In my own work on such a formalization, that is briefly discussed below, however, I explicate explicitly:

1) Assumptions about the substantive phenomena under examination that are not explicitly tested,
2) Constitutive definitions (that is, verbal definitions) of key concepts,
3) Measurement assumptions either about how optimal operational definitions of the key concepts should be developed or about the character of the most valid and reliable data that should be employed in measurement,
4) Axioms about broadly conceived relations among concepts that are assumed but not explicitly tested,
5) Testable propositions derived from the more general axioms that provide a basis for tests of verification of the theory.

Further, there are criteria by which to evaluate theories *individually* and *comparatively*. Conventional scientific values that are widely if not universally endorsed *in our discipline* suggest that an optimal theory would: (1) incorporate explanatory factors that are commonly suggested by existing research (a criterion I will call theoretical relevance), (2) adopt a nomothetic model of explanation (identifying the substantively most important causes and ignoring minor causes), (3) exhibit parsimony, and (4) have high explanatory power (e.g., Leege and Francis 1974. 33-35; Przeworski and Teune 1970, 17-23; Verba, Scholzman, and Brady 1995, 273-280). These criteria for scientific theory are frequently acknowledged, but some of them have at times been controversial or literally ignored. A brief digression on the latter matter is worthwhile, too, before considering the degree to which we have theories that meet these criteria.

King, Keohane, and Verba (1994, 20), first, doubt the value of the parsimony criterion and observe that a parsimonious theory might only be appropriate when we correctly judge the relevant real-world behavior to be simple.[1] They note, too, that parsimony is not a priority in some other sciences which, evidently, judge the real-world phenomena they study not to be "simple." Yet parsimony is a worthy goal for theory in all sciences. We cannot know in advance how likely we are to achieve parsimonious theories about particular phenomena. Nor can we know in advance whether those phenomena are truly simple or complex *or* whether a creative scientist might one day envision a parsimonious account for what were once thought to be complex phenomena. Indeed, are not the complexity or the simplicity of real-world phenomena in large part mental constructions? Why, then, should we temper our theory building ambition by assuming a priori that some subjects are too complex to be characterized

[1]Yet King, Keohane, and Verba (1994, 29) advance a criterion for theory testing for "explaining as much as possible with as little as possible," which reads like an alternative call for parsimony.

parsimoniously? The ultimate test, of course, is the theory we can create, and we should pursue that challenge ambitiously instead of timidly.

A parsimonious theory could also be valued for its elegance (e.g., Kaplan 1964, 318-319; Leege and Francis 1974, 35), although the importance of elegance could also be under-appreciated. While many scientists would endorse Polanyi's (1958, 145) characterization that "the intellectual beauty of a theory is a token of its contact with reality," others might think this criterion lame or foolish. But many scientists are inspired by the belief that the real world is governed by elegant laws that it is the goal of science to uncover**.** Thus elegance is widely espoused as a criterion for theory, and here too ambition instead of timidity should be our goal.

As a final example in this digression, explanatory power is sometimes ignored in what claims to be theory building research. Much scholarship intended to create middle-range theory seems to accept low explanatory power as the price of the search for eventually more complete theoretical formulations. At least one could hope that this is the reason for ignoring the explanatory power in such research. Yet considerable work that tests propositions that prove to have little explanatory power often ignores that fact and whatever implications it might have for the scholar's longer term theoretical goal. But advanced theory should be evaluated for its ability to account for relevant real-world phenomena. As Einstein (1954a, 271) said, "all knowledge of reality starts with experience and ends in it." He meant specifically, too, that scientific theory is inspired by observation of the natural world but is not complete until it is verified with empirical tests of its implications about that same natural world.

Explanatory power is important in a second way, as well. It is routinely observed, although perhaps too superficially and even incorrectly for some instances, that many theories might account for the same set of empirical observations. But a widely endorsed criterion for evaluating *competing* theories is to ask which of them accounts for more observable phenomena. That is, we ask which has greater explanatory power. Thus at some point we must take this criterion seriously if we are to discriminate among competing theories.

One could also conclude that any specific general theory may have to seek a balance among the criteria of theoretical relevance, nomothetic explanation, parsimony, elegance, and explanatory power, yet I suspect that theoretical relevance would be the most important criterion.

The preceding, conventionally cited criteria for advanced theory point to but do not fully provide a definition for the term *general theory* as it is most commonly used in science. Based on the consideration of many examples from the history of science one might define a general theory as: *a well articulated (e.g., stated) theoretical formulation that offers an explanation of some real world phenomena of central importance to a given scientific discipline, that meets the four criteria for a good theory discussed above to some significant degree even if some are better achieved than others, and that the majority of scholars in the relevant sub-field acknowledge to be the most comprehensive and satisfactory explanation for the subject matter addressed by the theory.* The last criterion about acceptance over its competing theoretical rivals is not listed in conventional methodological works on advanced theory but is widely acknowledged in more general accounts.

At least in many physical sciences it is also apparent that both the instrumental and the esthetic value of general theories are widely appreciated. High explanatory power most indicates instrumental value. Parsimony, elegance, and reliance on nomothetic explanation imply dimensions of esthetic value. It is curious that the esthetic value of theories in the physical sciences is often acknowledged by working scholars, yet I cannot cite a single such observation about theory from the social sciences.

Returning to my conclusion that we have little advanced theory, the implications of this discussion of what we claim general theory to be should be obvious. Theoretical formulations that meet even a handful of the preceding criteria are rare in our discipline. Consider again my poster-theory examples. Despite near-universal agreement on the value of the SES theory of voting in terms of its explanatory power and parsimony, no formal statement of it exists. Further, in my view, several interesting implications of that "standard model" have also therefore been ignored or under-appreciated. And the formulation that appears to be the leading alternative effort to account for mass participation in the United States – Verba, Schlozman, and Brady's (1995) Civic Volunteerism model – has not been formally stated and would appear not to be parsimonious, elegant, or particularly nomothetic (although absent a formal statement of the theory, the latter characterizations are perhaps speculative).

CPG theory has also never been dressed in the full, precisely specified armament of a general theory that is outlined above. Much of the work of its leading advocates explicates selective aspects of the theory, such as its assumptions about the electoral roots of legislative behavior and those about how variations in the level of conditional party government lead to different structural decisions and rules in the parties in office (e.g., Aldrich and Rohde 2001, 2009). Aldrich (2011, esp. 225-254) offers the most comprehensive account of the theory in print, but considerable detail there on related lines of research and much descriptive, historical information on the House of Representatives meant to illustrate implications of the theory complicate the exposition of the theory. The absence of a systematic statement of the complete theory has also allowed other commentators to describe its core assumptions and expectations in different ways (e.g., Cox and McCubbins 2005, 210-214; Krehbiel 1998, 165-172; Smith and Gamm 2005).

The fundamental concepts and assumptions of selectorate theory, in contrast, have been carefully stated in Bueno de Mesquita, Smith, Siverson, and Morrow (2003), who also provide formal modeling to explicate its logic. Numerous empirical tests have also been advanced that offer evidence for verification of the theory. Yet its authors acknowledge that, "it remains a primitive theory in need of enrichment with more institutional details and improved measurement" (Bueno de Mesquita, Smith, Siverson, and Morrow 2003, 11-12). The theory appears most in need of improved measures of central concepts for verification tests *and* attention to its explanatory power relative to rival theory.

Although this is not an example to be explicated in detail here some of my own research could be evaluated in the same critical terms as are applied to the three formulations above. Patricia Hurley and I have proposed a general theory of legislative representation, outlined its key components, and provided a range of verifying evidence for it (Hurley and Hill 2003). Yet the theory was not fully articulated in that work, and the scope of the empirical evidence was limited. There is work in progress, however, to remedy these deficiencies.

A Kuhnian or Lakatosian Perspective on Theory in Political Science

A second way to explain how it is the case that theory development is limited in our discipline is to draw upon some little-appreciated observations about the evolution of theory in Kuhn and Lakatos. Recall Kuhn's (1996, 10-11) catch-all concept of *scientific paradigms,* which bind communities of scholars around common theoretical perspectives and research agendas *and* which most working scientists likely think about in some fully realized state. Yet Kuhn (1996, 23) observes that newly created paradigms are typically "very limited in both scope and precision." He goes on to articulate how some paradigms may enjoy further elaboration because of continuing work by scholars in the relevant community. Lakatos (1978, 47) employs the concept of *research programmes*, to characterize much of what Kuhn does as paradigms. Lakatos (1978, 48-51) also observes that some research programmes flourish because their adherents work systematically to extend them whereas others languish because of a lack of systematic developmental research. It is important not to reify these concepts of scientific paradigms and research programmes, the first of which even Kuhn (196, 174-210) admits to be vague. But they give us analytic perspective on individual bodies of scholarship and theory in political science.

Most "theories" in political science reflect the relatively underdeveloped state of young scientific paradigms or research programmes as Kuhn and Lakatos describe them. And this circumstance has important implications. Too often our theories are vaguely stated and articulated. Absent an explicit verbal articulation, a given theory is really many different theories as it is interpreted and employed by different scholars. And when a theory is not precisely identified, there is much doubt about whether and how the accumulating record of empirical findings implies confirmation, disconfirmation, or implications for revision. As the physics Nobel-laureate Richard Feynman (1965, 158) observed, "you cannot prove a vague theory wrong."

Characterizing the Bulk of Our Theory as of the "Middle Range," Or Worse

I have long been attracted to Robert Merton's characterization of middle range theory, in part for want of a better label and in part because it comports well with the vast bulk of good scientific practice in the search for advanced theory. The intellectual progression of even highly advanced sciences like physics has been largely by way of the creation of middle range theories that were eventually incorporated into more general formulations (Weinberg 2001, 187-206). Merton (1949, 5) elaborates his conception of such theory in the following useful way as, "logically interconnected conceptions which are limited and modest in scope, rather than all-embracing and grandiose." At other points he explains that he has in mind theories meant to account for individual and sometimes narrow topics (e.g., Merton 1949, 9). This characterization likely resonates with the majority of working political scientists but in so commonplace a way as to suggest that the label has little merit or interest. I attach much significance, however, to how it applies to our discipline. In all the fields I know best it appears that one can *at most* say that we have some weak approximation of middle range theory (or of competing middle range theories) and that we have been at that plateau for a considerable period of time.

Consider, then, Merton's characterization of a middle range theory as a means for summarizing this argument about our progress in political science. The first point in this argument is that we have numerous attempts in political science to create something like middle range theoretical accounts of the kinds of phenomena Merton believes to be the proper subjects of such theory. The second implication of this discussion is that we have, at best, mostly only weak approximations of good middle range theory. When our efforts at theory construction produce vaguely stated, incompletely articulated formulations, or ones for which validation efforts have been relatively unsystematic, they are not particularly useful guides for research that might lead to more general ones. There may be some shared agreement within scholars working under a given paradigm or line of theory about key assumptions, concepts, and propositions. But the diffuseness of most of these paradigms means that there is as much ambiguity as concreteness in these shared agreements.

My concerns here are not unique either. Various other scholars have observed comparable problems in our discipline as a whole or in their sub-fields. I cite a few example observations of this sort, each with its own particular focus, but in some important way each echoing part of the argument above. Achen (2002, 445) argues that our discipline generally has a "proliferation of noncumulative studies" in good part because too little of our research is based on a rigorously constructed theoretical foundation. Bartels (2010, 252-253) observes that, in the face of conflicting evidence on how to account for fundamental aspects of mass electoral behavior, most scholars in that field have taken up the pursuit of relatively limited questions instead of general theory. Brecher (1999, 217) cites the "low value placed by most IR scholars on the *cumulation of knowledge"* [emphasis in the original].

Edwards (2009) argues that much research on presidential leadership of the U.S. Congress is compromised by poor theoretical foundations. Geddes (2007) concludes that after some 60 years of vigorous research, the causes of the democratization of national governments are disputed and that there is no consensus on a theory that would account for democratization. Sinclair (2010) and Smith (2007, 213-214) independently conclude that we have no theoretical account of the role of political parties in the U.S. Senate, and after much intellectual firepower has been expended in an effort to construct one. Indeed, Smith (2007, 114-147) raises concerns about both the CPG and cartel theories of lawmaking that are in the spirit of the remarks in this essay – that both theories require further elaboration and verification.

The Many Goals of Science, or Theory Isn't Everything But Everything Else Depends on It

I pause in the "long line" of the argument here to address an objection that could have arisen in the minds of some readers by this point. The preceding comments accord a high place to the pursuit of basic science theory. But some might argue, and quite correctly, that science has many goals and that individual scientists may thus have many different professional ambitions. Do I postulate an unfair or unreasonable position for basic science? Consider, however, our other major activities and their relation to basic science.

The most common alternative activity in our profession in my estimation is the pursuit of applied policy research on particular topics that also leads to advice to policymakers and to those who would influence policymaking. Such efforts have a noble place in our discipline, going back at least to the seminal call for such efforts by Lasswell (1951) early in the modern life of our discipline. Indeed, lay people as well as many political scientists often assume this is the principal function of science.

Closely related to explicit policy research are the efforts of many in our discipline to advise the mass public and government policy makers in more general ways about policy choices. Various activities fall under this heading, including among others, efforts to craft a position as a "public intellectual" (Hauck 2010) and engagement with the mass media as a commentator on a variety of political affairs. Some might label these efforts educative, others might call them advocacy, with either a positive or critical implication implied.

A third common activity is puzzle solving. Here I have in mind efforts to account for discrete, notable events by the application of political science knowledge. We do not have a body of work that formally outlines the character of such research or how it might most profitably proceed (but see Grofman 2001). But such research appears even in our leading journals with considerable regularity, on topics such as why the Republican party took control of the U.S. House of Representatives in 2010 (or in 1994, for those who recall the spate of articles on that event), why the Soviet Union collapsed, and which presidential candidate actually received the most valid votes in 2000 in its disputed vote and subsequent recount episode.

Applied research, more general attempts to influence public policy debate, and puzzle solving are important functions of any science for which they are possible. But the quality of all these efforts is dependent on the quality of our basic science knowledge. David Easton in *Framework* (1965, 7) made this same observation succinctly and almost half a century ago for our profession, “The application of knowledge is as much a part of the scientific enterprise as theoretical understanding. But the understanding and explanation of political behavior logically precede and provide the basis for efforts to utilize political knowledge in the solution of urgent practical problems of society.”

Scientists often engage in at least one other role that I discuss separately for its distinctive relation to theory and for its distinctive historical position in the development of our science. Some political scientists engage relatively exclusively or occasionally in data collection efforts, or one could say in description and observation. And this activity is as important for our discipline today as it was in past times. Survey research data sets, events data sets of various types, and collections of other social, economic, and governmental data are common in every sub-field of the discipline. Such data collections have contributed substantially to the collective research enterprise. Data collection, however, stands in an interdependent relationship with theory construction rather than in a dependent one. Relevant data of high quality are essential to theory construction, but theory is essential for directing the collection of data and in imbuing data once collected with meaning, again as Easton (1953, 52-63) likely first observed in political science. Numerous contemporary political scientists, of course, have repeated this observation (although it is not clear that much of the theory testing research that I read considers the observation deliberately). Thus the quality and utility of data collection, too, are in part dependent on the state of our theory.

The Evolution of Scientific Knowledge

If most lines of scholarship in political science are at some modest level of middle-range theory, how did they get there? Can one demonstrate that intellectual progress was made in the course of that development? How do sciences generally evolve in these terms? Answers to these questions can be valuable for many purposes. I sketch answers to them here as part of my intention to describe our state of intellectual progress *and* how we might advance beyond it.

A reading of the history of a variety of scientific disciplines suggests one can objectively identify four stages of evolution for those disciplines that have developed relatively autonomously. In classroom expositions I use astronomy as one example, because that discipline has evolved through all the stages and offers rich evidence for all of them. Yet political science itself is a good example and will be used here for that purpose.

These stages can be objectively distinguished by their most common research activities, yet they overlap to a degree in practice, even in single research paradigms or programs, and for good and ill reasons. They overlap in part because activities in earlier stages remain important in later ones, although they become subordinate to the modal activity in each succeeding stage (Hill and Leighley 2005, 347-350). I label the first stage as one of *uncontrolled observation and description*. Kuhn (1996, 15) observes of this stage that "early fact-gathering is a far more nearly random activity than the one that subsequent scientific development makes familiar." He also aptly characterizes the work in this first stage as being based on "casual observation and experiment." The early period of modern, professional political science – from approximately

1890 to 1950 or 1960 as most authors date it – is described in much the same terms by Deutsch (1973, 2-6), Easton (1953, 37-52), Freeman (1991, 16-17), and Lasswell (1942, 25-30), among others.

Somewhere along the timeline of scholarship, the principal research focus shifts to the second stage of *hypothesis formation and testing*. In subfields of political science I know best this second stage ran from approximately 1950 to 1970, which also fits Deutsch's (1973, 7-11) general assessment of when this transition occurred for our entire discipline. As its label suggests, putative tests of causal connections become the primary focus of research in this stage, but one also observes here more scholars than in the previous stage struggling with efforts to improve conceptualization, data collection, and hypothesis testing. Yet Deutsch (1973, 10-11) correctly characterizes this work in political science as yielding "an accumulation of what J. David Singer has called the 'correlational knowledge' of which variables appeared correlated and how strongly and how significantly with what types of outcomes, and under what conditions." The best work in the second stage also explored multivariate analyses, although it is striking how common bivariate analysis was, even at the front rank.

During the early 1970s or perhaps a decade later, many subfields in political science saw the appearance of their first, if simple, *middle range theories,* the third stage of evolution. In this period, as one example, the SES model of mass political behavior was codified. Competing theoretical formulations with a rational choice foundation such as that of Downs (1957) became relatively well realized somewhat later, because empirical work on them lagged that on the SES model. In the study of legislative representation in the United States, for another example, Kuklinski's (e.g., 1977) several studies of California legislators apparently effectively formalized what I call the "standard model" of instructed delegate representation that has dominated that research field to the present – although one could mis-read Miller and Stokes (1963), as many have, to be the precedent here.[2]

In this third stage, then, we begin to see bodies of scholarship that adopt relatively common assumptions, concepts, hypotheses, and measures that imply an underlying theoretical foundation. As Kuhn (1996, 23) characterized such developments generally, however, this event often arises with "very limited…scope and precision." Some, and perhaps many, scholars who work in a particular paradigmatic line of research may not even recognize the implicit assumptions of the paradigm. This problem could account in part for criticisms of the "Michigan model" for focusing too much on individual level attributes as causes of mass political behavior and too little on the social context influences that were especially of concern to the "Columbia School" perspective that preceded it. Equally, it is a part of the criticism of so-called behavioral research generally by students of the "new institutionalism" (e.g., March and Olson 1984). Some students of legislative representation adopt instructed delegate theory for their research without a conscious recognition that they do so. Thus the assumptions of and possible bounds of applicability for many middle range theories may not be well understood – or explored – by many scholars who employ them.

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[2]This standard model assumes that only instructed delegate representation exists or is to be modeled, includes as standard explanatory variables a measure of constituency ideology or preferences and a measure of the partisan affiliation of the legislators. The dependent variable is one or another measure of legislators' roll-call voting. Additional explanatory variables are added to this basic model in accord with whatever original hypotheses a given scholar wishes to test using the basic formulation as his or her theoretical platform.

The most advanced scientific disciplines, of course, have experienced one or more *general theories*, the fourth stage of evolution. Astronomy is a wonderful classroom example for this reason, too, for its several successive general theories exhibit especially well how knowledge advances *and* how newer theories both incorporate the knowledge in and go beyond that of older ones. While I conclude that CPG theory is still in the middle-range stage of development, Aldrich and Rohde (2001) argue that it demonstrates this kind of progress – accounting for the same legislative behavior that Mayhew's (1974) – also middle range – theory does, but going further to explain behavior that Mayhew cannot.

One might conclude of political science that we have some "candidate" general theories, but it does not appear there is consensus in the relevant subfields of scholarship that the descriptor of candidate should yet be removed from them. At the same time, those subfields of political science with such advanced theories demonstrate especially notable intellectual progress.

The preceding observations evoke a question raised earlier: how can one demonstrate intellectual progress, and especially in political science? We can point to the creation of specific, even modestly developed theories as one important kind of evidence. But there is an additional way that I think to be very enlightening (for a third approach, see Deutsch, Markovits, and Platt 1986). Reading most journal articles from the 1950s and even the succeeding few decades in those fields where I toil as a researcher leaves me struck by how relatively primitive most of that work is in conceptualization, data collection, and hypothesis testing. Another remarkable deficiency of much older scholarship is how ill-explained its scientific procedures are. The idea that such procedures should be public was recognized, but essential details of data collection, measurement and estimation are often absent in much published work from the 1950s and even

1960s. Scholars still today, of course, debate the best definition of and measure for many fundamental concepts, and they argue still over optimal theory testing methods. But reading old literature is both a sobering and yet encouraging experience. We know much more today than in the past about the majority of topics we study. Our present knowledge may be less systematic than this essay argues is desirable and possible, but it is generally an advance over that of the past in other respects.

We should also recognize that our contemporary debates about concepts, measures, methods, and the like are not necessarily a sign of intellectual weakness. Weinberg relates how Einstein's theory of relativity threw into doubt for a time the meaning of the seemingly prosaic and fundamental concept of *mass* in physics, which was already a quite advanced science. But Weinberg's (2001, 194) conclusion about how that episode was resolved is also instructive, "Meanings [of concepts] can change, but generally they do so in the direction of an increased richness and precision of definition."

Failings of Pedagogy

Three observations suggest that the way we educate young scholars often limits their abilities to contribute meaningfully to theoretical advancement. The first observation comes from extensive study of graduate course syllabi – from a random selection of *U.S. News & World Report* "top twenty" Ph.D. departments. In that effort I uncovered some syllabi for what appear to be exemplary courses that introduce students systematically to both the substance and the leading theories of their fields. Such courses, however, appear to be in the minority. A high percentage of courses on substantive topics offer instead "topics on parade," with select and usually few readings per topic, all marching quickly by (the phrase in parentheses here is a characterization of undergraduate American politics survey courses from a paper published long ago in *PS: Political Science & Politics* and whose clever author I do not recall). Admittedly, there might be lecture content in some of these courses that provides theoretical connective tissue to undergird these topics. Yet rare was the syllabus of this sort that suggested the existence of that undergirding. In such courses, then, I suspect that the extant body of competing theory is not systematically taught. Indeed, the word *theory* in the title of a course does not seem to guarantee that there will be much of that in its content.

The preceding observation and evidence might be discounted as not reflecting the corpus of any particular doctoral student's education. Perhaps the accumulated product of coursework, independent study, and mentoring by one's faculty overcomes the deficiencies in individual courses. A second observation, however, suggests that this is not necessarily the case. My department has had very many junior faculty searches over the last decade or so, and thus we have seen doctoral research presentations by very many young Ph.D.'s or ABD's. I also review a notable number of journal manuscript submissions, presumably from scholars of all ranks, and on a range of related topics.

Many young scholars we interview for faculty positions do not evidence an understanding of major theories in their field, cannot relate their own research to one of those theories, and have difficulty even discussing these matters in a meaningful way. Equally, it is remarkable how many journal article manuscripts I review cite scholarship only published within the last five or ten years, cite odd works to substantiate notable research findings for which there are other, seminal citations, and cite many seminal works incorrectly. As one elaboration of the latter observation for the study of legislative representation, Hurley and Hill (2010, 716) observe that the seminal article by Miller and Stokes in that literature "is one of the most cited, if not one of the most carefully read, publications in political science." Even more to the critical point, I referee many papers for journals that do not know the frontier of knowledge and whose authors don't recognize where their work might fit in the larger body of extant scholarship seen as a collective whole. Of course, such work is rarely at or near the frontier.

A third observation concerns how many young scholars are allowed or encouraged to create their research agendas with little regard for the frontier of theoretical knowledge. We have a widespread custom of allowing graduate students to follow their personal, undirected curiosity to a research topic for their dissertation – and for other parts of their research agenda. I style this practice to my students as research on my favorite city, my favorite nation, or my favorite and surely neglected independent variable – regardless of its relevance to the frontier of theoretical knowledge. Lazarsfeld, Berelson, and Gaudet (1948, viii) lamented this practice, too, at the dawn of the modern age of social science. One has to wonder how far we have advanced in this respect since that time.

Many young scholars who have interviewed for faculty positions at my institution, however, claim to be testing a theory – and usually one of their own *ad hoc* construction – for their personally chosen topic. Yet when they are pressed to elaborate that theory, it often turns out to consist of only one or two hypotheses whose conceptual foundation is weak or ill thought out. This observation suggests that the teaching of what theory is, what the typical structure of a theory might or should be, and how one might test theory is commonly as fragmentary as my dissection of sample doctoral course syllabi also suggests.

This catholic posture about topics for doctoral research in graduate education also contributes to the construction of eclectic research programs that may provide, in the best cases, interesting, particularistic findings on some topics but that rarely move the theoretical frontier.

In contrast, it appears that only a minority – although perhaps the intellectually strongest minority – of scholars follow the path of *programmatic research* that is so vividly advocated by Aronson, Wilson, and Brewer (1998, 133-134) for experimental research. Aronson, et al.'s advice translated into a general research philosophy would be, first, that no single research study can provide adequate evidence for the test of an individual hypothesis or theory. Instead, we should pursue multiple tests of our theoretical propositions, ideally and as possible, with multiple methods that differ as much as possible from each other. And we should seek to test what many have referred to as multiple implications of theory. This philosophy, then, directs us to follow a logically ordered and systematic program of research as the optimal route to the discovery and verification of theory. This path also seems the most profitable one for making meaningful contributions to substantive or theoretical knowledge, yet it appears to be the research path of but a minority of the members of our discipline.

The several observations here, taken together, have cumulative implications for the advancement of theory. If many scholars are not well grounded in extant theory in their field, the principal works that establish the major lines of theory, and the body of work attempting to advance individual lines of theory, then they are ill-prepared either to do the latter kind of research themselves or to educate future generations of scholars to do so. Literally broad knowledge about relevant theory seems important, too, in order to have a vision of what new research might most meaningfully contribute to theoretical advances. The latter thought evokes Charles Rosen's characterization of how Haydn, Mozart, and Beethoven created a new style of music, the "classical style," which many of their peers failed to recognize or grasp. Rosen's remarks on this topic could just as well apply to a new theoretical advance in science with but a few changes of nouns. One of his comments in the discussion of this topic contrasts the new classical style with the compositions of "the mass of minor composers, many of them very fine, who understood only imperfectly the direction in which they were going, holding on to habits of the past which no longer made compete sense in the new musical context, experimenting with ideas they had not quite the power to render coherent" (Rosen 1997, 22). If we educate young scholars well, and indeed ourselves too, in extant theory, our scientific discipline will not merit a comparable characterization. Every scholar would then be prepared both to teach and to research efficaciously in one or another middle range "style" of theory.

I am persuaded, too, that broad knowledge of extant theory is especially important for creative scholarship that might notably advance the theoretical frontier. We do not teach about the creative process in science, and the standard observation, widely made, is that both philosophy of science and the teaching of scientific practice focus on how we attempt to verify knowledge but not on how we generate insights of a substantive or theoretical sort for verification. Kaplan (1964, 13-180), as one example, refers to these concerns as relating to the "context of discovery" and the "context of justification." Yet a thorough knowledge of the character and bounds of contemporary theory may be essential for insights about how to advance such theory in a major way. Even serendipitous findings may only have deep meaning for those with broad knowledge of the relevant field, as suggested by Pasteur's observation that "In the field of observation, chance favors only the prepared mind" (Beveridge 1980, 33).

Research Routines to Advance General Theory

It would seem a formidable goal to reach the intellectual plateau of general theory in any science. Doubtless, it is a rare achievement we often associate with the work of single, presumably brilliant individuals. I argue, however, that the latter perception about how rare such achievements are may not be entirely accurate, that the task is as much a collective as individual one, and that there are deliberate research routines that can help us reach this goal.

There does not appear to be much systematic advice in our discipline, however, about how to do this. Absent such advice, I offer six maxims about how we might shape our research practices to be more successful. *The first maxim is that for some subfields of political science the road to general theory is likely shorter than the customary view of that goal would imply.* Many sub-fields of political science have already gone some distance toward such theory. While we may have many ill-developed theoretical paradigms, they are sensible starting points. They imply some consensus on fundamental assumptions, critical concepts, and at least some propositions that link concepts causally. Formalizing these fragments into a systematically stated whole is far less challenging than starting the process *de novo*. And more systematic statements of such theory could direct the search for verification along especially efficacious paths. But I mean here, of course, literally *systematic* efforts both at the formalization and the verification of theory.

The second maxim is that successful theory construction must depend on a balance of inductive and deductive reasoning. I offer this maxim precisely because of an intellectual divide on this matter in our discipline. Many political scientists conclude that the optimal route to theory would be based on a foundation of “formal” or *deductive* theorizing (for a careful statement of this view see Morton 1999, 3-24). At another “philosophy of science” pole from the deductive theory camp is an even larger number of scholars who appear to believe that the simple accumulation of more and more *inductively* derived evidence about relations among prominent variables will somehow add up to one day suggest general theory.

My perspective is different, although it is at the foundation of many discussions of the scientific method. Many textbook as well as more advanced discussions recognize that both inductive and deductive reasoning are essential for the task of theory construction, and in more than just a superficial way. We do not have at hand histories of how some of our most prominent middle range theoretical formulations came about, but my suspicion is that some were the work of scholars whose initial thinking was highly inductive and others were the work of scholars who begin with relatively deductive conceptualizations. Neither route – as a starting point – seems privileged in any science. Yet both must play a notable role in successful theory construction. Einstein's explication of the scientific method, presented in Holton (1986, 28-56), is especially insightful about the critical value of both inductive and deductive thought.

My third maxim is that theory must trump, or lead, methods. This maxim is also motivated by a widely held view that especially advanced statistical methods will most help us advance theory in our discipline. I can cite no explicit statement to that effect, but the philosophy appears widespread and is reflected in the doctoral education programs of many Ph.D. granting departments. Such methods are important and valuable, and they reflect the tendency in all sciences to become more mathematically sophisticated over time. Yet common, advanced statistical methods have recently come under scrutiny in our field. Recall how, as cited earlier, Achen (2002) and Brady (2008) have raised doubts about the value of many of our advanced methods for making sound causal inferences and thus verifying theoretical formulations. Achen (2011) has even observed that virtually all the most important discoveries in political science were the product of cross-tabular analysis. My addition is that methodological concerns instead of literally theoretical ones appear to motivate a high percentage of the unpublished and published papers that I read. Like inductive and deductive reasoning, theory and methods somehow have to be balanced for successful research. Yet in this case the balance is not one of equality but of theory dictating optimal methods. This is a conventional textbook dictum, yet it seems often ignored in our discipline.

A fourth maxim is that we should practice what we should preach (teach). That is, some of the advice offered above for doctoral education should also be embraced by scholars generally. One key strategy there is *programmatic research* on notable lines of middle range theory. This advice, however, has parts that are as important as the whole. One part is that we should especially embrace notable, extant lines of theory instead of crafting *ad hoc* (and usually far more fragmentary) theoretical foundations for our scholarship. The other part is that we attack the construction *and* validation of theory systematically – through a series of analyses that rely on multiple tests and multiple measures and that assemble a substantial body of evidence about causal effects and processes.

Relatedly, we should incorporate replication tests in the latter kinds of research in a systematic way. Conventional advice about the value of replication typically inspires little enthusiasm – and for good reason, since it is not imaginative. "Mere replication" is also widely thought to have modest prospects for publication, and thus it is undervalued. Our goal, however, should be to build replication tests into individual journal articles and books – with multiple tests of critical propositions with alternative samples, measures, and time periods. We must learn to design parsimonious research reports that incorporate multiple replication tests or tests of multiple implications of theory. There are examples, too, of such work. Consider how Patricia Hurley and I have presented evidence in a single journal article for fundamental propositions for our theory of legislative representation with multiple tests at multiple time periods, and thus with multiple, alternative measures of key concepts, for members of both the U.S. Senate and House of Representatives (Hurley and Hill 2003).

My fifth maxim is that we should not dismiss the textbook literature on theory construction. One could worry that such works are trivial "cookbook" treatments. Yet I see various kinds of value in such explications as those of Kaplan (1964, esp. 294-326), Jaccard and Jacoby (2010), and Lave and March (1975), among others. Intellectual chestnuts such as Platt's (1964) paper on methods of "strong inference" are also relevant here. Such works may not plot the route to theory with certainty. Yet they might help inspire one or another part of Pasteur's "prepared mind."

My sixth maxim is that we should ignore the real world, or at least much of the information it presents to us, more often than we do. This advice wants careful parsing. We must rely on evidence from the real world, faithfully acquired and represented, as one foundation for both theory construction and verification. But preoccupation with the real world can lead us astray from the task of theory construction, and in two ways. First, and as discussed above, very much research in our discipline is concerned with explaining particular puzzles that often have no clearly explicated theoretical implications *or* for which theory is not employed to tackle the puzzle. The everyday world of politics is doubtless fascinating, at times uplifting, at times frustrating, and frequently puzzling. Many political scientists are driven by curiosity and by a sense of a professional role to account for notable happenings there. But at times preoccupation with everyday politics distracts from our other professional role as scientists in search of basic knowledge.

Second, even when our avowed goal is theory construction, we are presented with an abundance of particularistic evidence from the observable world on virtually every important political phenomenon. Yet often we fail to abstract from that evidence only its essential parts that are necessary for general theory. Too often political scientists adopt a view of their subject matter that is reflected in the comment by Dahl (2004, 377) that "politics is a subject of exceptional complexity." But even physical scientists who have been successful in theory construction underscore how the same view of complexity could be adopted for their research subject matter, but must be transcended.

Einstein described the natural world as presenting us with a "labyrinth of sense impressions" and then went on to say that "Science is the attempt to make the chaotic diversity of our sense-experience correspond to a logically uniform system of thought" (Holton 1986, 32). Gazzaniga, Ivry, and Mangun (1998, 11) discuss how early twentieth century research on the human brain assumed that one must account for how *all* the billions of neurons in the brain interact to explain its functioning. Then they observe how successful accounts for brain functions were based on more abstract conceptions, "to think that we need to know the actions of all [neurons] to figure out how the brain works would be preposterous…. Advances are made by working at different levels of organization. By knowing what behavior is actually produced, we need not know all the possible interactions that occur with underlying elements. In this manner a problem becomes constrained and solvable." King, Keohane, and Verba (1994, 9-10) offer useful advice, too, that, "what we perceive as complexity is not entirely inherent in phenomena…. the perceived complexity of a situation depends in part on how well we can simplify reality."

There are at least three interconnected points at which we must both recognize and embrace abstraction from particulars. First, in the construction and measurement of individual concepts that are central to a theory we must recognize that, as Kerlinger (1986, 29) observes, "operational definitions yield only limited meanings of constructs. No operational definition can ever express the rich and diverse aspects of human prejudice, for example." That is, concepts are rich in literal conceptual content, whereas operational measures capture, or abstract out, only a part of that richness.

The preceding observation is widely made in relevant methodological literature, but its implications for the creation of working measures of concepts is often ignored or even resisted in political science. An example of this circumstance is the pursuit of measures of notable concepts with high particularistic content (e.g., Coppedge and Geering 2011). Yet the quality of operational measures is best ensured by theoretically focused content validation in the conceptualization of the theory (and especially for latent concepts) supplemented with extensive construct validation tests in the effort to validate the theory. Construct evaluation effectively adopts the assumptions of a given theory *and* the constitutive and operational definitions of its theoretically embedded measures. Strong empirical evidence for the theory, and especially for its explanatory power, is also strong evidence for the validity of the measures of its concepts. Thus the evaluation of rival theories is in part an evaluation of rival measures, and the evaluation of measures of concepts is best made by this theory laden process. Thus parsimony and abstraction are goals for both measurement and theory construction.

Second, whole theories – ordered systems of expected relations among assumptions and concepts – are necessarily significant abstractions from the particulars of the natural world. In the social sciences one version of this position was first stated effectively by Friedman (1953, 30-39): that assumptions of theories need not be "realistic" to be useful. His argument has been especially adopted in defense of rational choice models in political science (e.g., Moe 1978, 221-226). Yet the more general position here about abstraction is echoed by numerous physical scientists. Indeed, this is the ultimate point of the quotations above from Einstein and from

Gazzaniga, Ivry, and Mangun about the power of science to make sense of complex empirical phenomena. A related point, widely made in discussions of scientific theories, is that they should be abstract because they aim to be explanations of both what is known and what is unknown about a related set of phenomena (e.g., Holton and Brush 1985, 31; Kerlinger 1986, 30-32). Thus general theories of political phenomena will likely be highly abstract, too, but this is in the nature of the intellectual construction that theory is.

Finally, the verification of theories is also a process that lacks the kind of particularism with which some political scientists are most comfortable – and as a product of the preceding two points. As Leege and Francis (1974, 42) observe, “We never actually *test* our substantive theory. Rather, through empirical operations we test a ‘test theory.’ We test a posited relationship between sets of indicators which we feel exemplify each concept.” This observation might instill in some uneasiness with the *general process* of verification. Yet if we build the kind of body of evidence for a particular theory that is called for in various ways in this essay, we reduce the skepticism about it that we might otherwise have for this and many other reasons.

Further, verification tests of individual hypotheses that might have larger theoretical implications are often jeopardized in another *particular* way – by a perversion of the principle of testing for spuriousness by taking account of “plausible rival hypotheses.” The latter principle was first especially well explicated by Webb, Campbell, Schwartz, and Sechrest (1966, 10), but it has been reiterated by many others down through time (e.g., King, Keohane, and Verba 1994, 32-33). Yet in many verification tests the principle is often bastardized into one of controlling for any even flimsily plausible hypotheses. Achen (2002, 441-449), in particular, has explicated the riskiness of this research strategy for causal inference. Thus theoretically directed parsimony and abstraction are essential for verification tests in this second way, as well.

Conclusion

The descriptive, critical, and prescriptive arguments above are sufficiently straightforward that they do not require summary or elaboration. Some reflection on the intellectual philosophy behind them, however, would be valuable. Science has many goals, all of which are meritorious. Individual scientists are motivated by their personal curiosity and ambitions, as well, to pursue one or several of those separate goals. Yet it is virtually universally recognized that general theory is the highest strictly intellectual goal. I reference Easton (1953, 4), who stated over half a century ago that “All mature scientific knowledge is theoretical,” as a reminder of how venerable this observation is in our own profession.

The most successful scientific disciplines owe their success in good part to the work of scholars who were inspired by the preceding observation. They were driven to succeed at theory construction *because* it is the most challenging and exalted goal. The intention of this essay is to inspire more political scientists to have the same ambition – and to consider deliberately how best to realize it.

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