Is Political *Science* Really Young?

Kim Quaile Hill (*PS: Political Science and Politics*, July 2004) seeks to debunk five "myths about the physical sciences" that "pose notable hurdles for appreciating the social sciences as legitimate scientific enterprise" (467). One of these myths is that "the physical sciences have always been highly successful in explaining their subject matter." Hill complains that political science students are "ignorant of the history of science" and therefore they fail to "appreciate the differences between young and mature scientific disciplines. … If students can appreciate that all sciences were once youthful—as political science still is today—they will have a useful perspective by which to understand why and how the knowledge base of our discipline is limited" (469).

Is political science *qua* science as young as Hill suggests? Perhaps political scientists, too, could benefit from remedial education in the history of science, especially that of their own discipline. Consider the following quotations:

- "Hitherto, the method of the sciences of observation has not been introduced into political questions; every man has imported his point of view, method of reasoning and judging, and hence there is not yet any precision in the answers, or universality in the results. The time has come when this infancy of the science should cease" (Henry Saint Simon, early nineteenth century, quoted in Thiele 1997, 9).
- "The backward state of the moral sciences can only be remedied by applying to them the methods of the physical sciences" (John Stewart Mill, quoted in Thiele 1997, 9).
- "Natural Science will in time subsume the science of man just as the science of man will subsume natural science; there will be *one* science" (Karl Marx, quoted in Flyvbjerg 2001, 26; emphasis in original).
- "The Physiology of Politics" (Title of A. Lawrence Lowell's 1910 presidential address to the APSA, in which he urged political scientists to apply the scientific procedures used in the study of human organs to the analysis of political organs).
- It is "fundamental" that "social science and natural science come together in a common effort to unite their forces in the greatest task that humanity has yet faced: the intelligent understanding and control of human behavior" (Charles Merriam's presidential address to the APSA, 1926).
- "It is to the natural science that we may most profitably turn . . . for suggestions as to the reconstruction of our postulates and methods" (William B. Munro's presidential address to the APSA, 1928).

Hill's claim that "ours is a young scientific discipline" (471)—made two centuries after Saint Simon lamented the "infancy of the science" of politics—confirms Charles Taylor's (1977, 106) observation that the social sciences are "constantly said to be in their 'infancy.'" There may be many reasons why, as Hill concedes, the "knowledge base of our discipline is limited" (469), but tender age and a lack of effort to emulate the physical sciences are not among them.

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A Reply to "Myths About the Physical Sciences and Their Implications for Teaching Political Science"

Well, let us admit, we all suffer from at least a mild case of "social sciences inferiority complex." So it seems does Kim Quaile Hill in his spirited defense of the "less exact" part of academe. He finds his students' skepticism about (maybe even disdain for) political science troubling and frustrating. He is unhappy with students questioning the validity and certainty of social sciences. He fights back. However, his solution to the problem is not only ill-conceived, but it has the potential of harming his (our?) own cause. Instead of dealing with students' skepticism of the scientific character of social sciences, he blames that skepticism on students' misperceptions and misconceptions about physical sciences. Still, initially his argument sounds convincing when the misconceptions are framed in terms of scientific illiteracy, pseudo-science, and superstition. The first impression is—of course, how one can appreciate social sciences without a good understanding of science in the more traditional hard-sciences sense. Yet, we quickly discover that is not the author's point. It is the opposite. It is not so much about distrusting physical sciences, but rather about trusting it too much. With so much faith in physical sciences, social sciences are perceived as soft, inaccurate, and uncertain.

Hill believes that in order to gain a greater respect and understanding of social sciences, we need to demythologize physical sciences. He lists several myths that must be dispelled. But are the myths really myths? And if they are, are they

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really the main culprit in the poor understanding of social sciences among our students? Let us begin with the myths.

The first refers to the question of certainty and determinism. According to Hill it is not true that "the subject matter of the physical sciences is highly orderly, indeed, deterministic in its 'behavior'" (468). He proceeds to dispel that myth with a few examples from quantum physics, theory of evolution, and medicine. He concludes: "the physical and social sciences are not remarkably different in these respects" (468). The question of determinism is not new. One of the more interesting and well-known discussions on the subject is Karl Popper's 1965 lecture "Of Clouds and Clocks" (Popper 1979). I do not believe that getting into an argument that physical phenomena and social/human behavior are "not remarkably different" is beneficial. Our students are mature enough to be engaged in a discussion about differences between the two (especially in classes on scientific methodology). And they are surely able to distinguish among the various parts of the natural sciences. Very few would argue today that "all clouds are clocks," but there is no harm in admitting that certain phenomena are more cloud-like and others more clock-like. The fact that the subject matter of social sciences is definitely more likely to be in the first category is not a reason for despair. To the contrary, it should explain the difficulties with which social scientists struggle in their research.

And, naturally, in such discussions there is also room for examining uncertainties in the physical sciences. Yet, using a few questionable examples should not suffice. One is the tired example of Heisenberg's uncertainty principle. It seems that political scientists do not really understand the principle and invoke it more for the name than its real meaning. As Gross and Levitt remarked: "If Heisenberg and company had chosen a less evocative term, an awful lot of nonsense of this sort might never have seen the light of day" (Gross and Levitt 1998, 51). Looking at other of Hill's examples, we discover that he is rather sloppy in using the term "physical sciences." Throughout the article he moves across a variety of disciplines that can be characterized not so much as physical sciences but rather as natural sciences. Actually, very few of his examples are specifically from physical sciences. What is worse, on at least one occasion an example refers more to non-scientists controversies over science.

The next two myths cited by the author are simply the consequence of the first. If students see the subject matter of physical sciences as highly orderly, they also expect scientific research in these areas to be more successful and effective. Again—is there any harm in admitting that physical sciences (or, say, natural sciences in general) are generally more successful in explaining the subject matter they study? Of course, one could always argue about the meaning of the word 'successful.' It is apparent that Hill sets the standard very high. The argument here is basically this: Oh, well, there is still much to explain, therefore science is not that successful. But that is not the point. The question is not how much has or has not been discovered, but whether once something has been discovered and a theory formulated, how reliable that theory is when applied. I believe this to be an important point at which Hill and his students diverge. He looks at science from a farther distance and sees uncertainties and contradictions. But most students are not interested in philosophical questions—what is more, they are not really affected by the philosophical upheavals. To use Kuhn's terminology, Hill sees "incommensurability" and "paradigm shifts" while his students see science as "normal science" (Kuhn 1962). Further, they surely have a more utilitarian approach to science. They judge science by the practical results. For example, when they see the landing of NASA's Mars Rover, they appreciate the precision of the project. Even if there are mishaps, they know the problems are not the result of faulty science but rather engineering miscalculations or outside factors (who knows, maybe politics involved in approving and financing such projects). They rarely get such precision from political science or sociology.

Again, I want to emphasize the point that this is not an attempt to glorify or paint the natural sciences as infallible. Of course, Hill is right that physical (natural) sciences have their own failings. He is also certainly right that many theories in social sciences offer a very clear and truly scientific picture of the world. However, pointing to weaknesses of "the other guy" does not help in building respect for social sciences. This type of argument is of no use. I would argue that it is very healthy to show the limitations of political science as science. Although difficult ontological and epistemological discussion may not always be effective in undergraduate classes, it is an important debate and an effort should be made to familiarize students with the questions of the philosophy of sciences. The basic guiding principles are the same for all sciences, but whether we like it or not, not all methods can be applied the same way as in physical sciences. As Nagel wrote more than 40 years ago, "perhaps the most frequently mentioned source of difficulty is the allegedly narrow range of possibilities for controlled experiments on social subject matter" (Nagel 1961, 450). And there are always the problems of probabilities in social-scientific explanation, the problems of identifying and operationalizing variables, the complexity of factors influencing human behavior or social events, etc. (Kuhn 2000; Szaniawski 1994; Taylor 1985). That is why many political science theories, despite the best methods and number crunching abilities, are often no less speculative than observations of writers, journalists, and poets.

And here we come to another point: What social sciences? What political sciences? I am sure that we both, Hill and I, see the social sciences by the prism of the best work based on sound scientific methodology. But that is not necessarily how our students (and the public for that matter) see the social sciences. For our students, social sciences can be associated as much with highly structured and methodologically rich studies in economics, psychology, or some areas of political science as with the eccentricities of Jacques Derrida, feminist theories, or even the ideological tirades of political columnists. What adds to the confusion is that social sciences and philosophy also take on another role—the one of arbiters of what exactly social sciences should be. Maybe we could start with helping students to sort out these matters.

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In Response to "Myths about Political Science"

Kim Quaile Hill (*PS: Political Science and Politics*, July 2004) makes a somewhat nuanced contribution to the appraisal of epistemic controversies in the discipline of political science. His primary concern is for the manner in which erroneous presumptions about the nature of the scientific method in fields such as chemistry or physics have encumbered the effort to instill students with the habit of applying similar approaches in the study of politics. At first, the argument seems directed at uninformed students who do not recognize the social sciences as legitimately "scientific." Upon further reading, it quickly becomes clear in her elaboration of five common "myths" about science that the more implicit intention of this piece is to challenge the assertion of some in the field that the abstract, variant qualities of political phenomena make these applications somehow inappropriate. The point that he proffers—that the physical sciences are hardly as rigid, finite or objective as some may assume—is certainly a valid one.

Yet, in my view this essay fails to address the more critical issue of how these scientific approaches are actually being represented in some political science graduate programs. From my own experiences as a doctoral student, it has become apparent that, despite Hill's assertions, a certain number of advocates of positivism really believe that the natural and physical sciences are relatively more "intellectually powerful and successful," and that social science disciplines such as the study of politics are in their infancy because they have only recently begun to adopt their previously established and rigorous standards of inquiry.

Such proponents also seem to suggest that the purpose of the field should be to mirror the ambitions of the physical sciences in establishing universal explanatory constructs, and that only those agendas that are directed at accomplishing this task are truly relevant. This often accompanies the opinion that scholarship that does not proceed from these tenets is only "descriptive" (i.e., the recitation of names, places, and events) or relies on "inductive reasoning" that is incapable of producing logically consistent and empirically supported theory. Those academics that focus on the politics of particular countries or regions are regarded as being especially guilty of such inadequacies. While it may be true that the aims and emphases of these authors may differ from those sought by positive and formal theorists, I find it difficult to accept the assertion that all small-N, discursive writing in comparative politics does not concern itself with systematic analytical logic or applying and developing theory. I would suggest, in fact, that this charge constitutes a myth.

Corollary to this attitude is the belief that those academics who are critical of reifying general, deductive models as the singular goal of the discipline are so only because they, like the Copernican view of the solar system, have been outmoded by superior approaches and are therefore whining about their obsolescence, rather than voicing an informed concern that such a view might mischaracterize or suppress research interests that do not conform. Worse, such a mindset overlooks the distinction between rejecting prior perspectives because they contain ideas that have been repeatedly proven false by subsequent studies, and engaging in what Imre Lakatos referred to as "naïve" or strict falsificationism, in which a theory that does not pass muster in the first trial is quickly discarded rather than salvaged for its potentially novel content which might produce other testable propositions. Positive theorists are of course aware of this difference, but the danger of committing that absurdity is only reinforced by such attitudes.

What is equally troubling about the fallacy that research agendas in the physical sciences are never conditioned or even motivated by subjective biases is that it fosters the incorrect assumption, or myth, among some students that the investigation of normative concerns in politics—what people believe and the role of those beliefs in constituting political association—somehow cannot be objective, or that it simply involves the scholastic promotion of one's individual preferences rather than impartial and critical analysis. A perusal of the work on normative political theory that is regularly published in the *APSR* or any number of other peer-reviewed journals should demonstrate fairly quickly that this, too, is a falsehood.

Despite my own conversion to empiricism as a result of exposure to and training in statistical and formal methodologies, I refuse to adopt these narrow, unsophisticated impressions of the field. In sum, Hill's concern for the obstacles to training political science students in the intellectual habits of scientific reasoning does not take into account a related problem: the misrepresentation of this reasoning among faculty and students who are committed to one perspective to the exclusion of all others, and insist that the rest of us should reflect similar attitudes if we expect to be taken seriously in the discipline as a whole.

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"Science and Political Science *Redux*"

I am pleased to have the opportunity to respond to Oren, Ozminkowski, and Strake's comments on my recent article on myths about the physical sciences. All of them in my judgment either misperceive parts of my original argument or raise concerns that allow me to extend that argument. To the degree that others share their views, this essay may address widespread differences of opinion or misperceptions about these matters.

I begin with some observations in response to Ozminkowski. He takes exception, and in several ways, to how I characterize and compare the social and physical sciences. He thinks my use of example concepts and episodes from, and my general characterizations of, the physical sciences "questionable." So be it. We can all read the history of science and reach our own conclusions. And we might come to different interpretations of how to generalize the implications of such work as that of Heisenberg, Popper, and the like. I stand by my examples and characterizations. Indeed, I disagree quite pointedly with Ozminkowski in regard to some of his conclusions about the physical sciences, his distinctions about physical science versus technology, and his characterizations of the social sciences. More importantly, however, and behind my own discussion of such matters, are three general, interrelated points that Ozminkowski either ignores or fails to recognize.

First, I am hardly the first to observe that the mass public, secondary school students, and university students commonly misunderstand the physical sciences. I cited as much prior work to that effect in my article as *PS*'s length limits and an effort at graceful balance in a short essay allow. This point, then, is not original, but it is central to my argument.

Second, all the sciences share some common goals and procedures, and their intellectual histories share striking similarities. If we can educate our students about those shared properties, they will have a more sophisticated perspective than they otherwise would for understanding political science. This point is not entirely novel to my essay either. It is widely noted, either directly or implicitly, but I am unaware of any work that explicates it fully. My original essay does not develop it fully either, but that essay and other of my writings on political science education share this theme and pursue it in one or another way (Hill 1997, 7 8; Hill 2002).

Third, students with poor understanding of the physical sciences are especially ill-prepared intellectually to grasp the character of the social sciences *qua* sciences. This point *is* original to my *PS* article. Recognizing that hurdle for teaching political science was almost a revelation for the insights it gave me about effective pedagogical strategies. And the point of my article was to share this recognition and some insights about how it can prove useful for teaching in our discipline.

Ozminkowski suggests that he prefers a more direct approach to engaging students about the scientific character and success of our discipline. Evidently, he would employ such a strategy in discussing how he characterizes the phenomena we study as especially "cloud-like" and in evaluating the "reliablity" of our theory in applied use. Many routes to knowledge can be fruitful. Perhaps I betray my scholarly origins in comparative politics in thinking that a comparative understanding of the sciences would be especially valuable. Even Ozminkowski's "direct" approaches must rely on comparisons with other sciences. How will students appreciate the implications of attempting to study "cloud-like" phenomena if they do not contrast it with the stereotypic notion that the physical sciences study only "clock-like" phenomena?

I have a number of ideas for a yet-to-be-written essay on students' misperceptions of the social sciences explicitly. Such an essay might have better pleased Ozminkowski. But it is the more obvious alternative, bordering on the mundane, given that others have laid down many of the basic points of such an argument. The challenge for that argument is saying something novel.

Finally, I reply to Ozminkowski's complaint that I am "sloppy" in my use of the term physical sciences. He apparently prefers the label "natural sciences" for all those disciplines outside the social sciences. Labels can be a bother for all concerned in such situations. But I use the terms physical and social sciences quite deliberately. I consider the label *natural* sciences a universal, and thus quite meaningless, one. Does Ozminkowski imply that we are among the *unnatural* sciences? As I observe to my students, all sciences study natural phenomena. The "things" that political sciences observe and seek to understand are every bit as natural as those that biologists, chemists, and geologists study. Nature is at work in politics as it is in physics.

On, then, to Professor Oren. He contests one claim in my article that ours is a young scientific discipline. Youth and age, doubtless like beauty, are judged differently by different observers. I note two points, though, in response to Oren's judgment on this matter. First, a string of citations, some going back to the early nineteenth century, of arguments to the effect that the study of politics *ought* to become more scientific is no evidence that it *did* become so in response to those exhortations.

Second, my judgment on this matter is not unusual. A host of contemporary observers echoes the conclusion that ours is a young scientific discipline. Consider a variety of histories of political science on this point, as well as various shorter assessments of the state of the discipline, whether friendly or not to its modern scientific disposition (Boston 2002, 280; Crick 1954; Kirkpatrick 1962, esp. 10 11; Lynn 1983, 96 97; Somit and Tanenhaus 1967, esp. 109 133). Consider the conclusions of a range of historians and philosophers of science as they discuss in more or less detail the character of the social sciences (e.g., Kuhn 1996, 15; Lakatos 1978, 9; Nagel 1961, 447).

Finally, I am surprised that Oren did not raise another objection to my conclusion that ours is a youthful scientific discipline that our scientific roots are ancient and begin at least with Aristotle. Thus I address this common belief, as well. To conclude that we have forebears in ancient times is doubtless correct. To conclude that the intellectual procedures employed by these forebears accord with those of science as we know it today is arguable and likely not supportable (Schlagel 1995, 297 324). To conclude, finally, that a small number of thoughtful, pre-scientific observers of political phenomena, often separated from dialogue with each other by leagues and centuries, constituted a scientific community as we know such today is also insupportable.

I come, then, to Mr. Strake's lament. He is disheartened by the fact that

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certain "advocates of positivism," specifically "positive and formal theorists," discount other modes of scholarship in political science and their value. I conclude that Strake's education as a scientist is incomplete. He cites Lakatos on a particular point, and I wager that he has also read Kuhn's *Structure* given its signal status. But I conclude that he has not read *between the lines* of the cool, abstract analytics in such works.

Consider one set of *written* lines from Kuhn (1996, 12 13) when he discusses pre-paradigmatic research on physical optics before Newton and what we might infer is implied *between* those lines:

> No period between remote antiquity and the end of the seventeenth century exhibited a single generally accepted view about the nature of light. Instead there were competing schools and subschools, most of them espousing one variant or another of Epicurean, Aristotelian, or Platonic theory. . . . Each of the corresponding schools derived strength from its relation to some particular metaphysic, and each emphasized, as paradigmatic observations, the particular cluster of optical phenomena that its own theory could do most to explain.

It is difficult to imagine that scholars working in one of these separate schools felt particular respect for those in other schools. Kuhn implies, too, that differences of method as well as substantive focus divided these groups. Reading *between the lines*, then, in Kuhn or Lakatos (1978) or any similar discussion of competing schools of research in any scientific discipline leads to the same conclusion. Varying degrees of indifference, disrespect, or outright hostility are common across schools. And the latter observation does not deny that there always exist some scholars whose work seeks to unify and transcend the views of competing schools. Such individuals exist in political science today, but so do many who are hostile or indifferent toward scholars pursuing explanatory structures or employing methodologies different than their own. Indeed, some practitioners of "small-N , discursive" research who Strake defends are quite hostile to the work of scholars pursuing positive and formal theory.

In sum, I encourage Strake and others who share his concern to read between the lines of conventional histories of science in other fields. Perhaps, he's also missed the direct, explicit discussion of the sociology and, indeed, politics of science in such works as Watson's *The Double Helix* (1968). There the *realpolitik* of competing scientific schools is addressed *in the lines*. What Strake accurately perceives in political science, then, is common in all the sciences.

I conclude with a few comments about a concern implied but not fully developed in Strake's argument. He defends, if I represent his various comments correctly, scholars interested in relatively more particularistic instead of generalizable phenomena, those concerned with normative matters, and those who might employ qualitative, inductive, or discursive research methods. He echoes a common complaint that such scholars feel scorned and undervalued by those in other schools. My advice above is to recognize how common this situation is. Yet I am a practitioner of relatively inductive research methods myself. I exalt the particular advantages and power of such methods and am spurred all the more to demonstrate them given the popularity of deductive theory building research in our discipline. Thus I would offer the same counsel to those about whom Strake is concerned as I could in principle to myself. If you value your own scholarship, don't complain about its alleged second-class status. Instead, get on with your work. Do your best to craft clever and interesting research on important questions. Your success will be judged by your peers, but meritorious scholarship will be recognized regardless of its theoretical or methodological orientation. Some school will embrace you.

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