

**The Discipline's Community:
The Effects of Method and Market on Research Relevance**

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Introduction

There can be little doubt that the discipline of political science has turned inward in recent decades. This inward turn might be explained in numerous ways. We might interpret it in terms of an unwillingness on the part of political scientists to produce research relevant to prevailing social and political problems. We might see it as a sort of naivete on the part of political scholars. Or we might understand the inward turn in the context of a market of ideas in which political scientists seek to increase their “meganyimic capital” through the production and reproduction of trendy or “cutting edge” research aimed at the continual enhancement of their own careers (Luke 1997). All of these factors and more may explain some aspects of the inward turn on various levels, but in this paper I hope to take a different approach. Here I want to explain the inward turn in historical terms. That is, I want to discuss, albeit briefly, how the discipline’s scientific identity crisis, which was temporarily resolved by the behavioral revolution only to be undermined and (re)resolved by the Kuhnian “revolution,” in combination with an increased population of political scientists has resulted in the current state of the discipline in which political scientists “talk” only to other political scientists and then only to those who “speak” their language.

This, of course, is familiar ground. David Ricci (1984) has argued that political scientists, in their rush to churn out massive amounts of publications to enhance tenure possibilities, have sacrificed wisdom as they clamor for “novelty.” Novelty allows a scholar to distinguish herself quickly and to demonstrate her worth in the market of political ideas. More recently, Tim Luke (1997) has expanded on these ideas to explain the discipline’s scholarship as name recognition masquerading as knowledge. Professional political inquiry, in other words, has much more to do with distinction among colleagues than it does with the production and reproduction of useful and relevant research for an ostensibly democratic polity. As long as one publishes copiously in the “right” journals and with the “right” publishers, one need not worry too much about the relevance or the normative content of one’s research. I am sympathetic to both of these accounts, but neither gives proper weight to the fascination that political scientists

have historically had with *science* and to the impact this has had on research practices in the discipline.¹ Indeed, in alliance with the early Western Marxists (Lukács 1971), I argue that the cultivation of a positivist approach to science as evidenced in the behavioral revolution has led to passivity within the discipline. When one need not (and, indeed, cannot) worry about the normative presuppositions and implications of one's research and when one is able to hide behind a veil of expertise, it becomes very easy to ignore the real world relevance of that research. In this way, we can focus on how our research "contributes" to the ever expanding stock of political knowledge over which we claim exclusive control. I wonder, though, to what extent we can take this knowledge seriously if it never reaches beyond the confines of the discipline and its system of internal rewards? That is, when political research becomes focused more around the success of a career than it does around the education of citizens for "democratic" living, can we be sure that our "knowledge" is engaged, critical, relevant and useful? I contend, in this paper, that given the historical development of the discipline and its attachment to its "scientific" identity, we cannot be confident that our "knowledge" can have the desired effect in a "democratic" community, mainly because we are "talking" to ourselves within a closed community that seeks necessarily to exclude "outsiders" as a means of protecting claims to expertise.

I

As we know, the American science of politics found its naturalist, positivist face in the behavioral revolution of the 1950s. Only then did many members of the discipline accept and become comfortable with the scientific identity of the discipline. Behavioralism established an agreed upon methodology (modeled after the natural sciences) which was (ostensibly) value free and emphasized quantification toward the end of generating covering laws to aid in predicting

¹The fact that I am seeking a "novel" approach to a topic that has been covered by others certainly should not be overlooked here. No one in my position (untured) can escape the disciplinary practices of the discipline. When I can be recognized as a contributing member of the discipline I stand a much better chance of finding that tenured spot.

political behavior. Though this has changed in some respects since the behavioral revolution, the positivist model of science still informs the scientific identity of the discipline. The cost of the scientific identity to the discipline has been the marginalization of normative political theory. In fact, behavioralism called for an end to normative political theory since it did not aid the empirical science of politics. And normative political theory has never recovered. Normative political theorizing has certainly not been abandoned and it surely has a home in “postbehavioral” political science. But, we cannot deny that the research accorded the most legitimacy within the discipline and its various departments is that research that makes some claim to being “scientific.” More important, though, is the fact that most political research (and certainly that research which allows one to increase one’s megalomaniac capital) tends to have an inward focus, appealing to the work of political scholars in an attempt to deface, disprove, disparage or disagree as scholars seek to distinguish themselves in the “casino capitalism of collegiate careerism” (Luke 1997: 23).

Much has been written of late about the historical development of the discipline. This research has generally sought to accomplish two goals. It has either attempted to trace the development of the discipline as it relates to the structure and aims of democracy in the United States (Easton, Gunnell and Stein 1995; Gunnell 1993; Farr and Seidelman 1993) or it has aspired to track the development of the various research traditions in the discipline and to critique their effectiveness (Farr, Dryzek and Leonard 1995; Seidelman and Harpham 1985; Ricci 1984). Of these works, only Gunnell (1993) systematically discusses the estrangement of normative political theory from the “legitimate” pursuits of an empirical science of politics. According to Gunnell, by the 1960s normative “political theory was well on its way to becoming an intellectually, if not professionally, autonomous field with tenuous links to political science as well as an increasingly problematical understanding of its relationship to politics” (1993: 8). I agree that the pursuit of a scientific identity modeled on the natural sciences has necessarily involved eschewing normative political concerns from the legitimized, scientific approach to the study of politics, but my account differs from Gunnell’s in the sense that I explain the “descent of

political theory” in terms of the legitimation of political inquiry as science, while he explains it principally in terms of the impact that German émigré scholars had on the science of politics during the 1940s and 1950s (1993: 6, 146-220).

Gunnell argues, for example, that the behavioral revolution took place specifically in response to the immigration of German political scholars. He claims that the “insinuation of the ideas of the European Right and Left” into the discipline ultimately “gave form and meaning to behavioralism” (7). In particular, Gunnell contends that the construction of a “more self-conscious scientific identity” in the form of behavioralism was “fundamentally a function of the belief that there was a need to defend the traditional vision of social *science* against the emerging antiscientific philosophies” at the hands of German émigré scholars (223, emphasis mine).

This perspective, while interesting and provocative, neglects to take into account the historical development of the scientific identity in the discipline. My argument differs from Gunnell’s in this respect. I have argued elsewhere that the discipline of political science had been casting about for a coherent scientific identity at least forty years prior to the behavioral revolution (Duvall 1997). Thus, while German émigré scholars may have played a role in the codification of behavioralism, the seeds for the behavioral revolution were planted long before its inception. This interpretation is significant since it deepens the form and meaning of behavioralism. The behavioral revolution, on my account, was not merely a response to the influx of German scholars following World War II. Rather, behavioralism was primarily the fulfillment of the yearning within the discipline for a legitimate scientific status, and the pure science of politics that behavioralism fostered was perceived by those involved to provide that legitimacy. The implication of this scientific legitimation for normativity was the systematic exclusion of normative inquiry from the legitimated pursuits of the discipline.²

²I should comment on the fact that I insist, in this paper, that the science of politics is a positivist science, even today. This may seem odd in light of Thomas Kuhn’s claim that the progress of science is not rational or linear. It may seem to be even stranger given the “postpositivist” movement that Kuhn’s work spawned (Lakatos 1970; Laudan 1977). I contend that postpositivism did not significantly alter the goals of positivist science. Indeed, the main goal of

We can date the beginning of American political science as an organized discipline to December 30, 1903, when John Burgess, Frank Goodnow, Westel W. Willoughby and others founded the American Political Science Association (APSA) (Haddow 1969: 262; Ricci 1984: 63-64; Somit and Tanenhaus 1968: 23). The Association's journal, the *American Political Science Review* (APSR), followed in 1906. Even though most schools still lacked separate political science departments, the appearance of the APSA, and thereafter its journal, legitimated professional commitment to political science as a coherent area of study. The APSA gave political scientists, in and out of the university, a sense of common purpose, and the APSR offered an outlet for original research and scholarly exposure.

The discipline of political science was not initially focused in the university. In fact, in 1912 only 20% of the APSA membership was composed of "professors and teachers" (Somit and Tanenhaus 1968: 55). The academic contingent of the APSA was extremely effective, though, at consistently dominating offices of the APSA and the editorial board of the APSR. Over time, "the presidency [of the Association] was increasingly reserved for de facto professors, with non-academics rarely advancing beyond the rank of second vice-president" (55-56). Eventually, the Association became increasingly populated by academicians, moving the discipline's focus to the university.

As the discipline was established, political scientists increasingly incorporated political knowledge as their peculiar domain. Political scientists, after "authorizing" themselves through the creation of a discipline, began cordoning off political research as their area of study. They defined themselves as holders and keepers of political knowledge. The study of politics started to become a professional pursuit, sanctioned by a professional association. This trend toward professionalism in the field of political research became more clear during the behavioral revolution's move to "pure" science. With behavioralism, the discipline settled on a scientific

postpositivist social science is to salvage rationality in science as well as to guarantee the rational progress of social scientific knowledge.

identity, an identity that has changed little since its inception. Behavioralism, though, has its roots in the “science of politics movement” which began in the 1920s.

Political scientists believed that a scientific, disciplinary and professional identity (i.e., acceptance as “legitimate” producers of knowledge) depended on a common and useful methodology to separate trained “political scientists” from the methodologically untrained amateurs (Ricci 1984: 36-40). Experts in political studies would then use the correct methods of research to engage “in a communal endeavor *deserving recognition and respect* for its original and valuable contributions to American society” (39, emphasis added). Scientific method would allow political scientists to arrive at objective, value-free truth (or truths) about a certain aspect of (usually) American politics in order to aid a modernizing polity in a purely technical way. There could be no normative goals in a value-free science.

The new era in political science which followed World War I, like most new eras in the discipline, repudiated the previous era of political science. Progressive political science was condemned as invalid and partisan, not scientific enough. The new era sought more detached, scientific, methodical, and therapeutic reforms for what was perceived to be a democracy in crisis.

According to post-World War I political scientists, the United States’ “liberal democracy” emerged badly shaken from the war. Political scientists had supported the war “for the usual reasons--it was supposed to end European autocracy and thus end war” (Seidelman and Harpham 1985: 102). Instead, emboldened and effective fascist and communist governments in Europe strengthened their abilities to motivate their populaces to act in accordance with government interests. Post-war political scientists in America noticed a peculiar lack of any such motivational ability in the United States, and their wrath fell on their immediate predecessors. On their account, reform-minded progressive political scientists had not adequately and systematically located receptive reform publics, and their superficial and hasty analyses and proposals had consequently failed to be effective (101-103). In light of this, political scientists of the new era saw the need for scholarly renovation. They renewed their dedication to establishing

scientific inquiry in the hope that "scientific knowledge would emerge and contribute to improving the quality of public life in America" (Ricci 1984: 77). The professional identity of the political scientist became that of political "healer" and political knowledge was to be constructed toward this end. Political knowledge was to be implemented in the governmental system. Political scientists such as Charles Merriam and Harold Lasswell saw themselves as social engineers whose purpose was the "rational" supervision of political actors to order and control a logical, brave new political society.

Progressive political scientists had considered citizens to be "eager consumers of social science messages," whereas the "new" political scientists saw citizens as "objects of study and observation to be 'educated' and controlled" (Seidelman and Harpham 1985: 103). The ultimate question for the new political scientists became how to motivate public opinion to support the liberal democratic state. They operated on the presupposition that humankind was perfectible and thus that a reliance on "scientific" political knowledge would help to cure societal and political ills permanently (Merriam 1934: 184). The "scientific" political scientist constructed political knowledge that could be applied in a technical way to governmental functions.³

In his scholarly work Merriam consistently demanded that political science become more rigorously scientific, but with an applied as opposed to a pure approach. He saw the development of a scientific technique and methodology for political science as a necessity to avoid "speculation and guesswork" (quoted in Crick 1959: 138). Merriam viewed the physical sciences as attempts to benefit, preserve and perfect civilization, and he reserved a place for political science in this process. He consciously sought to control the "evolution of intelligence"

³See Luke 1978: 1, 45. Luke argues that there is a difference between this applied "scientific politics" and political science. At the time, though, the practical application of such political knowledge to public affairs *was* political science. That is, this is how political scientists (especially those influenced by Merriam) identified themselves. They saw themselves as political therapists, political healers, whose professional identity revolved around the construction (they might say "discovery") of political knowledge that is applicable, useful and efficacious.

and human behavior, through civic education, to instill democratic values in citizens in the move towards the perfection of society and humankind (Crick 1959: 136-143).

Merriam was always very vague about the nature of such control and about the implementation and ramifications of such a science in his own work. Even so, Merriam was extraordinarily influential as he broke the ground for later political, scientific excavation. He also helped to propel political science from obscurity into tentative legitimacy, at least in terms of federal funding and acceptance (which is what the discipline had sought after for years). This legitimacy was enhanced by the behavioral revolution of the 1950s.⁴

Merriam's student, Harold Lasswell, held a primary interest in political behavior and its control through "conceptual frameworks" of institutional and societal power (Crick 1959: 177). He was among the first of Merriam's students to argue that society consisted of irrational voters *and* leaders whose behavior must be scientifically studied in order to make it rational (i.e., to control it). Obviously, Lasswell strode a fine conceptual line between democratic and coercive politics. In fact, he often appears to have tumbled over to the coercive side. His belief in popular irrationality immediately called into question the possibility of "popular rule" that democracy purports to require. This clash between the scientific pursuit of human/societal perfection and democratic values illuminated a methodological contradiction that the discipline feared might subvert its commitment to democracy (Ricci 1984: 94-96).

⁴It is interesting to note that the emphasis placed upon science by Merriam and others belies an apparent political agenda. Merriam, in particular, insisted that he was interested in using science to support the liberal democratic state. To do this, however, would require that scholars seek to control citizens, to somehow "make" citizens. This need to control certainly seems to contradict the liberal notions that Merriam repeatedly claimed to support. How can such a contradiction be explained? One particularly interesting plausible explanation is that perhaps Merriam, and others of his ilk, wanted to hide behind science. With science they could claim to be neutral; they could claim to have the answers to the problem of instituting liberal political ideals. As such, only they could know what it takes to institute the liberal state and thus they ought to be allowed to control citizens for their own good. In this way, Merriam and other "progressive" political scientists appear to be "liberal" in their minds only. One might say, of course, that contemporary political scientists are "liberal" in the same way.

In *Psychopathology and Politics*, Lasswell makes an argument, based on certain aspects of Freudian psychoanalysis, that politics needs to be studied with an emphasis on psychology and its relation to the personal and political behavior of individuals. Lasswell was not as vehemently opposed to formalism as Merriam was, but he did argue that institutional analysts frequently overlook "the 'personal' influences which modify the expected behavior of 'legislatures,' 'executives,' and 'judiciaries'" (Lasswell 1977: 2).

According to Lasswell, politics was the realm of the irrational (184). The irrational displacement of affects was brought into the open in the arena of politics. As such, political solutions are frequently not the best *rational* decisions, but the best *emotional* ones (185). And irrelevancy accumulates in political symbols due to their emotional appeal (193; Lasswell 1938; Lasswell 1950: 38-51, 64-75). Consequently, Lasswell sees little use in democratic discussion as a means of dealing with political problems. If the individual is shown to be a poor judge of *his* needs and interests, how effective can discussion be in resolving issues of conflict? In response, Lasswell does not condone the resolution of conflicts. Rather, he sanctions their prevention.

Lasswell argued that certain "objective investigators," schooled in the fields of psychology, psychopathology, physiology, medicine and social science, will be able to "deal objectively" with themselves and with others in order to discover the "truth" of harmonious human relations and thereby to obviate political conflicts (Lasswell 1977: 193, 196-197, 200-203). The interdisciplinary political scientist, then, will be able to discern the cultural patterns of individual/social conflict and function as therapist to prepare people to manage their emotional conflict "objectively" and thus make them and society more rational and purposive. Lasswell envisions a utopia, one where radical political action is not necessary or desired. Rather, the education and research of rational social scientists is offered as the cure for the irrational bases of society. In this way, a utopian society will emerge and flourish (Crick 1959: 199).

Lasswell, like Merriam, had noble and admirable goals in his pre-World War II work. He was also a talented and innovative social theorist. Moreover, it is difficult to argue with his or with Merriam's desire to find the perfect society, free from conflict and irrationality. Merriam

and Lasswell simply wanted to install a professional identity for political scientists based on a science that was organized to aid the liberal democratic state. As such, political knowledge was to be organized for the same purpose. And this is part of the reason why such a formulation of science caught on in the discipline. It was constructed to correspond to the technical needs of society and therefore it became the accepted (funded and legitimated) identity for political scientists (Luke 1978: 3-4). But after World War II, this identity began to crumble. Behavioralists wanted to purify scientific political knowledge, while retaining an implicit normative commitment to liberal politics.

II

Numerous factors emerged to help establish behavioralism as a force in political science: political scientists perceived that they were not considered legitimate scientists and consequently had problems securing research grants; they believed that the other social sciences (particularly psychology) were making broad advances while political science lagged behind; the reformist, normative nature of the discipline was generally considered speculative and unscientific; research technology (survey techniques, statistical computations, computers) became much more refined and available; and they pursued a "pure" science which operated on the presupposition that democracy is *the* best system of government due to its open and scientific qualities (Somit and Tanenhaus 1968: 184-185). In short, post-World War II political scientists sought to define the science of politics from the standpoint that science should be pure. The science of politics should be interested only in *explaining* the workings of American democracy in order to understand the (American) system better.⁵

Post-war political scientists believed that political crises remained because pre-World War II political scientists had allowed their reformist aims to occlude their understanding of

⁵This, of course, carries with it a normative commitment, one that a pure science of politics is unable to recognize.

politics. Many post-war political scientists wanted to embark on the pure scientific project of analyzing the workings of the American system without tainting the analysis with speculative notions of reform. Here we see the first self-conscious attempts to push normative political theory to the margins of the discipline. The assumption that American democracy is *the* best political system in the world expels the normative determination of value from the discipline's activities. A pure science, after all, cannot consider such a claim. Rather, it must presuppose its end as it determines how best to reach or enhance it.

Although it did ultimately become a driving force in political science, behavioralism did not begin as a coherent movement in the discipline. Rather, post-World War II political scientists began rejecting formalist, reformist, normative inquiry and relying more upon explaining the workings of the American political system. This phenomenon is exemplified by David Truman's revival of Arthur Bentley's *The Process of Government*. And behavioralists like Heinz Eulau and David Easton furthered what ultimately became a movement by explicitly championing behavioral research. Still though, behavioralism assumed many faces. It was a broad enough phenomenon to allow several different pursuits.

Somit and Tanenhaus have been able to combine these numerous strands into what they term the "behavioral creed": (1) Political science should search rigorously for regularities in political behavior in order to facilitate prediction and explanation; (2) Political science should concern itself with empirical political phenomena, that is, with the behavior of individuals and political groups; (3) Data should be quantifiable in order to aid predictive capabilities; (4) Research should be theory driven, in other words, research should begin with a theory that yields empirically testable hypotheses; (5) Political scientists should avoid applied (reform-minded) research in favor of pure scientific research; (6) Values such as democracy, equality and freedom cannot be scientifically established and should thus be avoided (or assumed?) unless they can somehow be made empirically testable; (7) Political science should become more interdisciplinary, at least at the behavioral level; (8) Political science should place more

emphasis on methodology and make better use of multivariate analysis, sample surveys, mathematical models and simulation (Somit and Tanenhaus 1968: 177-179).

The tenets of this "creed" did not necessarily delineate behavioralist methodology. Often, it seems, the practice of behavioralism reified these trends. Furthermore, many goals of the behavioral era were organizational. For instance, behavioralists were intent on building a scientific community which was centered around behavioral inquiry. They could do this by further institutionalizing political knowledge. Therefore, the research skills that behavioral inquiry required served to exclude those who did not possess the proper training and to solidify the scientific identity of political scientists, leaving normative political theory behind as a casualty.

One example of this can be found in the work of David Truman. Truman is probably best known for his book *The Governmental Process*, which revived Arthur Bentley's group process theory of government (Truman 1971 [1951]). Truman's argument, although less polemical, closely resembles Bentley's and is offered in response to the expanding role of interest groups in American politics and the public's growing fear of their influence. *The Governmental Process*, by Truman's own account, contributed to the "political behavior movement" in political science by increasing "the analytical strength and usefulness of the discipline" (xix-xx). It also triggered the growth of the study of interest groups in the United States and abroad (xxviii). Like Bentley's work, *The Governmental Process* offers a tool for analysis, a theory to drive systematic behavioral research. It contains many "testable hypotheses" ranging from the political orientations of groups to the internal politics of the group process to the influence of groups on the legislature, the executive, the judiciary and elections. Research into these areas, with the group emphasis, has increased tremendously since the publication of *The Governmental Process* in 1951.

Truman's basic argument revolves around the notion that since every individual attempts to become an accepted participant in a group or a set of groups, it makes sense to study political behavior in terms of groups and group interactions (18). He argues that "the patterns of action

and attitude among individuals will differ from one another in large measure according to the clusters of group affiliations that the individuals have” (16). Individuals define themselves based on the opportunities that groups afford. In Truman's words, "It appears...that the group experiences and affiliations of an individual are the primary, though not the exclusive, means by which the individual knows, interprets, and reacts to the society in which he [sic] exists” (21). Like Merriam, Truman believed that society had become sufficiently complex to necessitate an interdependent approach to the analysis of political behavior and government (11). In other words, any social or political action involves a complicated series of interactions, particularly at the group level, which affect individuals and the government (45-52). With this in mind, the purpose of Truman's book, he argued, was to analyze rigorously both the operations of representative government in the United States and the character of the groups' relationships with the governing process. Truman's behavioral tendencies are clearly present in his emphasis on political behavior *and* in his purpose. He does not intend to offer any normative prescriptions for reform. Rather, he seeks to offer an empirical and conceptual analysis of the group process in government in order to develop and provide an understanding of the operations of American representative democracy (12). He does not desire progressive reform. His research seeks "pure" explanation.

The Governmental Process was Truman's most influential and noteworthy work. His commitment to a "pure" science of political behavior that sought to examine and explain the uniformities and regularities of politics helped touch off the broad emphasis on political behavior that distinguished the post-World War II research of political science. In his theories we can see expressions of four of the first five tenets of the "behavioral creed." The emphasis on quantified, value-free and methodologically sound research became clear later. Another behaviorist, Heinz Eulau, openly criticized the reformist ("utopian") political science of the pre-World War II era (Eulau 1969a: 372). He argued that science can only function "in an environment that permits freedom of inquiry and freedom of speech” (Eulau 1969b: 12). American liberal democracy allows such freedoms and thus is most suitable for scientific work. Political *science* can never

undermine liberal democracy, as Ricci reported that pre-World War II political scientists feared (Ricci 1984: 74-75). Political *scientists* assumed, then, that American democracy must be alive and well as they pursued the new, non-reformist, scientific goal of analyzing and explaining the ways that the American political system functioned.

Based on his experiences with Harold Lasswell at the "Experimental Division for the Study of War Time Communications," Eulau claimed that the task of science is to convert "belief" into "knowledge" (Eulau 1969c: 359). Belief thrives on ignorance and the goal of systematic science was to remove as much ignorance as possible in order to expose knowledge. Ultimately, the goal of this science is causal explanation. In other words, tentative, "functional," "probabilistic" knowledge should eventually give way to causal knowledge. Causal knowledge, Eulau argues, is never attainable but scientific endeavors will invariably bring us closer to the goal of perfect, universal, causal explanation (Eulau 1969a: 388-389). Such theoretical descriptions of science do not help much with implementation so Eulau, again following Lasswell, made a practical argument based on "micro-macro political analysis."

From Eulau's view, the traditional separation between the "micro" and "macro" levels of political analysis represented a problem that needed resolution. Macro level analysis aimed at institutions, while micro analysis focused on individuals. Typically, these two approaches operated dualistically; they were kept separate because political theorists had not built a conceptual link between them. But Lasswell, in *Psychopathology and Politics*, attempted to combine the micro and macro levels of analysis to explain political action. Eulau saw this conceptual connection as vital to the science of politics (Eulau 1969d: 122-137). He argued that these two levels of analysis can be merged through studying political behavior. The study of political behavior focuses on individuals' attitudes, actions and psyches *and* on the political institutions which frame, affect and are affected by individuals' personal and political dispositions. Eulau located the science of politics in the study of political behavior that collapsed micro and macro level distinctions. Such a science would ideally be empirical and focused on finding uniformities and regularities in political behavior. The careful documentation of regular

and uniform behavior would help explain and eventually predict institutional influences on behavior, and vice versa (Eulau 1969c: 358; 1969b: 15; 1964: 33, 44, 64). Empirical methods which were theory-driven (that is, based upon testable hypotheses) give *knowledge*; they strip ignorance from belief and produce political *knowledge* (Eulau 1969a: 390; 1969b: 8-9, 15-17; 1963: 9, 26, 34, 69).

Political knowledge, according to Eulau, is necessarily "probabilistic." That is, political *scientists* are *certain* that their knowledge is *probably* correct. But they seek to be *certain* that their knowledge is *definitely* correct. They seek *certainly*, "*universal validity*," for political knowledge. Eulau claims that this quest for certainty is implicit in empirical, behavioral methods. But, for now, political scientists must settle for probability until political knowledge progresses enough to allow for universal validity (Eulau 1969e: 366-367; 1969a: 15-16, 19; 1963: 10, 35).

Accompanying the quest for certainty and the self-conscious employment of methodology is the separation of fact from value. In other words, political scientists must eschew values in their detached, scientific work. As such, political scientists seek "neutrality" toward their research. This neutrality can be guaranteed through the researcher's openness about his biases and by treating "latent" biases as "errors" which can be "isolated and discounted" (Eulau 1969e: 366-369; 1963: 95, 137).

Confessed and discounted biases contribute to the quest for certainty and so does quantification. According to Eulau, political scientists should seek to quantify their data and their results. Quantification, using the most advanced research technology, empirical methods and testable hypotheses, introduces exactitude and reliability to political knowledge (Eulau 1969c: 361-362; 1963: 122).⁶ Eulau argued that quantification allows political scientists to be more certain about the legitimacy of political knowledge. "Political knowledge," realized through behavioral methods, emphasizes reason. It represents the belief that employing rational,

⁶This point has been echoed by many, including Evron M. Kirkpatrick (1962: 13, 23, 29).

rigorous methods of inquiry can help us discover the underlying, uniform and cosmic order of political things. For Eulau, political knowledge represents truth in a probabilistic sense, but its quest for certainty makes it valid and authoritative. This formulation of knowledge left little room for normative political theory. In fact, in many ways normative political theory was consciously excluded from the normal practice of political scientists (Gunnell 1993: 223-225), and the postbehavioral era under which the discipline now operates has not necessarily altered this situation.

The behavioral movement in the discipline took its methodological cues from Logical positivism, which was spawned by the Vienna Circle during the 1920s and 1930s. The Vienna Circle, under the intellectual leadership of Moritz Schlick and Rudolf Carnap, sought to elevate scientific method and mathematical logic to preeminent status as generators of knowledge. Indeed, logical positivism “stigmatized metaphysical, theological, and ethical pronouncements as devoid of cognitive meaning and advocated a radical reconstruction of philosophical thinking which should give pride of place to the methods of physical science and mathematical logic” (Achinstein and Barker 1969: v). Logical positivism claimed to be the “philosophy to end all philosophies” (Feigl 1969: 4), and it thereby obviated Husserl’s phenomenology as well as all other metaphysical attempts to investigate the production of knowledge from the relationship between knower and known. Logical positivists simply emphasized empirical tests. The dictates of scientific method indicate that a theory only has meaning when it can be verified by observation. Thus, theology, ethics and metaphysical expressions in general are cognitively meaningless since they cannot be empirically verified and therefore cannot be part of any scientific inquiry. The formal expulsion of metaphysical expressions from scientific inquiry renders science pragmatically inept. The purification of knowledge that logical positivism wrought means that knowledge must be sought for its own sake (or, perhaps, for the sake of truth) rather than for the sake of something else (such as for the sake of human good).

This is precisely the notion of knowledge that behavioralists in the discipline of political science adopted during the 1940s and 1950s. This should come as no surprise, though, since we

know that the scientific identity of the discipline had historically followed developments in natural science and that logical positivism was an important philosophical perspective throughout the world by the 1930s (Blalock 1982: 12; Achinstein and Barker 1969: v). The positivist version of knowledge clearly informs the tenets of the “Behavioral Credo” and it is quite clear that behavioralists were focused upon pure knowledge and the expulsion of metaphysical “guesswork” from the practice of political science. This focus came under some scrutiny, though, during the “Postbehavioral Movement” of the 1960s and 1970s, and “postpositivist” science followed. Postpositivism, though, as I have already indicated, did not change the essential principles of scientific knowledge and therefore did not change the primary characteristics of the scientific identity of the discipline.

III

The 1960s witnessed the most impressive and widespread pessimism regarding American politics and society since perhaps the Civil War. The Vietnam War, student rebellions, the civil rights movement, feminism, urban riots, inflation, unemployment, and a host of other factors combined to raise serious doubts and questions in the minds of many people concerning American democracy (Ricci 1984: 176). People were becoming more critical of the government and society, and political scientists were not exceptions. Political scientists wanted to question and criticize American democracy, too, and behavioralism provided no means to do so. As such, many members of the discipline began explicitly to denounce and move away from behavioral research. In the wake of these events, postbehavioralism was born.

Postbehavioralism differs from behavioralism in several ways. If behavioralism was difficult to define, postbehavioralism entirely defies definition. No creed (or credo) can be accurately attached to post-behavioral research. Postbehavioralism was also never a research program. Those who endorsed “it” sought relevance, action, and openness in the discipline, but there certainly was no postbehavioral method to be followed nor was there any organized postbehavioral identity. Rather, the discipline fragmented into specialties, which employed their

own (often behavioral) methods. The discipline's research agenda (and political knowledge) expanded as more viewpoints and research areas were included. And, as the number of political scientists grew, the work of political scientists was increasingly divided formally.

The expansion of the division of labor in the discipline affected the professional identity of political scientists and the formation and content of political knowledge. Part of the logic behind the division of labor is to allow space for more and unique work. As such, the division of labor engenders expansion, but also contraction. Political knowledge was expanded as the pursuit of it was divided. That is, more viewpoints were represented and more areas of interest were pursued as political knowledge. Concomitantly, though, the breadth of any one researcher's work contracted as political scientists specialized. The content of political knowledge, then, expanded while its formation fragmented. Professional agendas affected by the expanding division of labor combined with predominant social and political occurrences to alter the structure of political knowledge as postbehavioral interests surfaced in the discipline. Yet, the role of science as legitimator remained intact.

The main problem with discussing postbehavioralism is that it cannot be described as a coherent movement within the discipline. Aside from some oblique references to it (Easton 1969; Graham and Carey 1972), we cannot say with any certainty that any distinct movement known as postbehavioralism has ever existed in the discipline. Rather, postbehavioralism can only be said to exist insofar as the behavioral era has been followed by an era in which political scientists undertake research in a markedly different way and a slightly altered methodology has accompanied this new approach. It has been during the postbehavioral era that "postpositivism" has taken hold in the discipline of political science. The opening of the discipline to new approaches to studying politics also extended the discipline's tolerance to some of the emerging critiques of social scientific practice. The main attack on positivist doctrine originated with Thomas Kuhn's *The Structure of Scientific Revolutions* (1970 [1962]).

Kuhn argues that science does not progress by the piecemeal accumulation of knowledge. Instead, scientific development and change occurs through revolution (1970: 111-173). Kuhn

postulates three phases in the development of a science. The first is “pre-paradigmatic” in which various “schools” compete to attain the status of “paradigm.” A paradigm is a particular world-view. It provides a model from which “particular coherent traditions of scientific research” spring (1970: 10). Ptolemaic astronomy, for example, was a paradigm and it operated from the perspective that the earth was the center of the universe. The Ptolemaic paradigm was replaced via scientific revolution by the Copernican system which contended that the earth and the planets revolve around the sun. Revolution is the second phase of scientific development. The third phase is the paradigmatic phase. Kuhn calls this “normal science,” indicating that a given science has matured, that it has progressed from its pre-paradigmatic immaturity (1970: 11). This perspective clearly undermined the positivist notion of scientific development portrayed by Heinz Eulau who claimed that the “discipline is built by the slow, modest, and piecemeal accumulation of relevant theories and data” (1963: 9). The “building-block” theory of the behavioralists certainly was subject to Kuhn’s critique, but so was the idea of a social science itself, since Kuhn indicated that social science does not yet appear to have developed any paradigms at all (1970: 15). Political science, then, must be an immature science (or prescientific) by Kuhn’s account.

Kuhn’s oblique dismissal of the social sciences from his account of “normal science” touched off furious attempts by political scientists to locate paradigms within the discipline. Otherwise it might once again be subject to the arguments (which certainly exist to this day) that political science cannot be thought of as a science in any “real” sense, arguments which might scare away sources of funding by undermining the discipline’s legitimacy as “science.” The field of international relations seems to have been particularly affected by this possibility insofar as it has been characterized by its desperate (and apparently fruitless) search for a paradigm (Ferguson and Mansbach 1993: 14-31; Rosenau 1971; Ashley 1977: 150). But political scientists of all stripes jumped on the Kuhnian bandwagon of science for fear of being left behind among the

rabble of non-scientists. In so doing, the discipline informally oriented itself around a new “postpositivist” or “postempiricist” science.⁷

Conclusion

What we have seen in recent years, then, is political research with an inward focus. Given the lack of normativity that positivism requires, political knowledge cannot legitimately seek reform and given the need to protect expertise, political knowledge must appeal to the disciplinary marketplace in which ideals (but neither normativity nor practicability) rule the day in the form of mathematical models, path analyses, case studies, and jargon (to name but a few methods of exclusion); all of which appeal much more to (certain) other political scientists rather than to democratic citizens. Thus, while postbehavioralism has prompted us to emphasize the relevance of topic, professionalism and positivism have combined to force us to ignore the relevance of action. Accordingly, professional political analysts produce research that fewer and fewer people have an interest in or a use for, and the consequence of this seems to be the increasing insignificance of work on which individuals stake their careers. The discipline ought to take notice of this development. There are important political and cultural ramifications when students are ill-prepared for everyday life in an ostensibly democratic political system.

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⁷⁸ Richard Bernstein refers to the Kuhnian revolution as “postempiricist philosophy” (1983: 20ff.), but he means the same thing I do with “postpositivism” which is the term of choice among political scientists (Ball 1987; Fischer 1982; Vasquez 1995).

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