Political Science:
The State of the Discipline

Edited by Ada W. Finifter

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Preface

When I was appointed Program Chairperson of the 1982 Annual Meeting of the American Political Science Association by APSA President-Elect Seymour Martin Lipset, I chose as a guiding theme for the meetings, "the state of the discipline." Nineteen eighty-two seemed a good year for some stock-taking. The heady days of the growth of the social sciences in the 1960s were only a faint memory for many of the current members of the profession. Some of the attempts at applying social science theory to public policy problems had produced unclear, and occasionally questionable, results. The social sciences were under great pressure to prove their worth in the more budget-conscious 1970s, and that pressure had increased under the current administration in Washington. It seemed a good time to review where we were and where we might be going. Nevertheless, despite my own conviction that a review of the intellectual developments in the field over the past twenty to twenty-five years was due, I had little inkling that many scholars were feeling a similar need.

The then newly appointed Executive Director of the American Political Science Association, Thomas E. Mann, responded enthusiastically to my choice of a theme and suggested that we capitalize on the attention the subject would be receiving at the convention by collecting a series of articles on the "state of the discipline" in an APSA publication. This idea was supported by the APSA Council, and I agreed to organize and guide the effort. If the volume is successful, a continuing series of this type may be authorized by the Council.

The overall goal for this volume is to begin to fill the continuing need for a frequently published overview of political science research. It is intended to be used in teaching and for research purposes, as well as by foundation and government officials who need regularly updated information on current research in the discipline. I hope that the book will be useful in introducing graduate students to the problems about which political scientists are currently concerned, and that it will help political scientists learn more about each other's work. The volume should also be useful to foundation and government officials, as they consider research opportunities in the discipline, and to people working in government and other applied fields, who wonder what political scientists can contribute to their daily concerns.

Since this book grew out of the theme of the 1982 Annual Meeting, it was designed in conjunction with the program for that convention. In each section of the program, a paper was commissioned to review the state of research and future directions of that subfield. Since most of the subfields include a variety of approaches to their subject matter, and cover enormous bodies of literature, the authors were given latitude to define their scope of coverage in a way that they felt was most meaningful to their own research interests. In some cases, an entire subfield (as defined by the program section title) is covered; in others, the author has defined a specific part of the subfield or some significant intellectual problem for more comprehensive coverage. Therefore, not every area of political science is included in this first review. If the series is continued over a period of years, any important omissions of particular subfields or within particular areas will certainly be addressed.

The authors were asked to define the area being reviewed; to analyze conceptual frameworks employed by scholars working in the area, including classical and older approaches if they were relevant for contrast with newer frameworks; to treat the cumulation of knowledge in the field by discussing the major research findings; to consider how important findings have been changed or modified as knowledge has developed; and to suggest the issues that need to be addressed in the future.

Each of the papers was presented at a "theme" panel at the 1982 Annual Meeting. A large number of colleagues throughout the discipline contributed by acting as discussants on these panels and commenting on the original versions of the papers. Most of the discussants also provided very helpful written comments on the papers. Following the meetings, the papers were reviewed by anonymous peer reviewers. Other colleagues provided informal consultation and advice. All of the papers benefited from this generous peer review.

The authors of the articles appearing in this volume are, of course, the primary sources of any insight and wisdom contained in it. Many of them also served as Section Chairs for the 1982 Annual Meeting of the APSA. In other cases, the Section Chairs contributed to the volume by suggesting colleagues especially well suited to prepare the "state of the discipline" papers, by establishing panels that focused on important issues in the discipline, and by their general encouragement and support.

The members of my own department at Michigan State University proved to be an uncommonly congenial and helpful group. David W. Rohde, the Chairman, was generous in his cooperation and support for this project. Marty Lipset, with his broad view of the development of the social sciences, supported the project from its inception and worked with me in its initial stages. Tom Mann deserves special recognition for his knowledge of the profession and his ever helpful advice and enthusiastic support. Members of the Association are forewarned should they receive a call from him: he's a man who can get almost anyone to say "Yes" to almost anything.

I am also very grateful to the following colleagues and scholars for their thoughtful and valuable contributions and would like to thank each of them for their assistance:

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The members of the profession indicated their interest in an intellectual review of the discipline by attending the 1982 "state of the discipline" theme panels in unusually large numbers. Many of the comments I received about the Denver convention related to the special attraction and the utility of having this kind of review take place in a forum that provided an opportunity for discussion and feedback from a diverse representation of political scientists. I hope that the publication of these papers will contribute to the continuation of this dynamic process.

Ada W. Finifter
East Lansing, Michigan
June 19, 1983

POLITICAL SCIENCE: THE DISCIPLINE AND ITS SCOPE AND THEORY

1
Political Theory: The Evolution of a Sub-Field

John G. Gunnell

It is necessary to make a distinction between Political Theory as a subfield of the discipline of political science (*PT*) and political theory as a more general interdisciplinary body of literature, activity, and intellectual community (*pt*). It is also necessary to distinguish between those aspects of *PT* that are closely tied to *pt* and those that are more directly related to various research programs in political science. This is not to suggest that there are not important relationships and points of overlap and intersection both between *PT* and *pt* and between the elements within *PT,* but locating the boundaries is important for an analysis of political theory as a whole and for an understanding of the controversies that have animated it. For example, according to some political scientists, one of the problems has been to make Theory relevant to the practice of the discipline of political science rather than an outpost of history, ethics, and other fields that contribute to the constitution of *pt*. On the other hand, some political theorists, while professionally and institutionally situated within political science, identify themselves intellectually much more closely with *pt*.

While there are recent works that attempt to characterize and survey the present condition of political theory and speculate about what it might or should be (e.g., Nelson, Ed., 1983), less detailed attention has been devoted to the "odyssey" of political theory in American political science (Toth, 1982). Most attempts at the latter have been quite general characterizations in the service of some critique or apology. My basic approach will be archaeological. I hesitate to say historical, since, in many respects, this is more an excavation, or the presentation of the results of such an endeavor, than what many might consider conventional history. I began at the surface and dug downward, even though I am presenting my report in standard chronological order. However much my personal concerns may be reflected in the course of sifting through this dense material, I am more concerned with uncovering previous sites of political theory than with evaluating their contents and achievements.

One characteristic of the subject matter is that the termini are poorly defined. Rather than reaching the "beginning" of Political Theory as a subfield, I stopped when the traces seemed too amorphous to warrant further dredging. On the other end, the deposits build up so rapidly that any definite conclusion is almost immediately obsolescent; I can do little more than indicate what an initial survey of the surface yielded and what prompted my particular angle of entry.

The state of both *PT* and *pt* in the 1980s is one of *dispersion*, and I will risk the claim that this will remain its dominant characteristic for some time. To some extent this is the result of developments within political science as a whole, but it is also a reflection and consequence of trends within the sub-field and its relationship to *pt*. *PT* has broken up and scattered in different directions and adopted various mediums of expression, but the spectrum that can be discerned is not the diffraction of any basic substantive issue or set of concerns. Dispersion is less a symptom than the very condition of the field. This condition grew out of the events of the 1960s, became clearly manifest in the 1970s, and defined the state of the field by the beginning of the 1980s.

Some might suggest that this diffusion is basically the result of increased specialization in political science as a whole as well as greater interdisciplinary involvement. Others would argue that what is happening is, in the still popular Kuhnian terminology, a paradigm breakdown or transformation. Each of these claims may be correct in some respects, or at least convey something about certain aspects of the situation, but rather than pursuing a particular causal explanation, I will attempt to reconstruct the basic contours of the evolution of the sub-field of Political Theory. But, first, it is appropriate to be a bit more specific about the character of this putative dispersion.

In recent years, there has been a great deal of enthusiasm about the future of political theory. This is in sharp contrast to some of the attitudes that characterized the 1950s and 1960s when, in some quarters, political theory, at least in the form of political philosophy, was "thought to be on the verge of extinction" (Richter, 1980, p. 6). For a long time thereafter, it was considered to be an endangered species. While political theory, conceived as part of the scientific study of politics, was vigorously propagated, other modes of theory seemed to be on the decline. It now seems that, even among many of its former advocates and supporters, the commitment to "scientific" theory is somewhat muted, and many would argue that there is now, and should continue to be, greater complementarity between empirical and normative theory. For example, the idea of political science as the study of public policy, which has provided a self-image for the discipline in recent years, might imply a closer relationship between *PT* and *pt* or between political science and political philosophy. But it has also been claimed that "the great vitality in the field of political theory during the last fifteen or twenty years" is the result of a divergence between *PT* and *pt* and the fact that much of political theory has "turned away from" and become "indifferent to much of academic political science." Both "may prosper, but not interdependently" (Kateb, 1977, pp. 135, 136).

It will be necessary to consider such positions more carefully, but, for the moment, it is enough to note that there is a continuing ambivalence about the relationship between political theory and political science that has persisted since the beginning of the Post-World War II period. There are sentiments for secession and exile as well as for unification and integration. Today, it is clear that at least in *pt*, and much of the literature of *PT* that is most closely allied with it, there is a sense of movement whose source is in some respects quite easy to identify but whose significance and direction are more nebulous.

There is a widespread belief that during the 1970s there was an "upswell of political and social theorizing" and that "political philosophy" now "obviously flourishes, all over the English-speaking world and outside it too" (Laslett & Fishkin, Eds., 1979, pp. 2, 5). This alleged upturn is usually linked with the publication of John Rawls's *A Theory of Justice* (1971) and related works of a similar genre such as Robert Nozick's *Anarchy, State and Utopia* (1974); the popularity of so-called Critical Theory and the work of individuals such as Jurgen Habermas; and the variety of critiques, summaries, and commentaries that grew up around this material and the pursuit of similar themes. Various explanations were advanced to account for this "upsurge in creative political theory" (Freeman & Robertson, 1980, p. 11) such as the release of moral philosophy from the grip of positivism and the shock of political events in the late 1960s, but it was generally acknowledged that political theory was born again. This sense of having overcome the past was quite evident by the end of the 1970s, but the present constitution and future form of *pt* is far from evident. The situation in *PT* is no clearer. Since most of the developments in *pt* are reflected there, its literature is equally diverse.

Dispersion does not mean that the universe of political theory cannot be charted, but it is difficult to discern some overall form. Collections purporting to represent the "frontiers" of political theory leave one in doubt both about the territory that these margins circumscribe and the nature of the terrain that is being penetrated. It is suggested that there is a "return to political theory in the grand manner" but that it is now "pluralist" to an unprecedented degree and can only be characterized by looking at "what political theorists do" (Freeman & Robertson, 1980, pp. 11, 1). An attempt by the journal *Political Theory* to project the "Prospects and Topics" of "political theory in the 1980s," seemed to reach the same conclusion, or non-conclusion. The editor decided simply to "invite a number of thoughtful senior colleagues . . . to get on with what they were thinking about," and claimed that the diverse contributions were "representative of both the scope of the field and the pushing forward of its concerns and frontiers" (Barber, 1980, p. 291).

THE EARLY YEARS: BEFORE 1899

The idea of political theory as a distinct kind of activity, vocation, and product is of relatively recent origin. The concept of political theory in its contemporary sense or senses has not only been largely a creation of the subfield of Political Theory in political science but a convention that can be attributed to the debates about the character and status of political theory that began in the 1940s and 1950s. Notions of theory that emerged at that time were read backward into the past and projected into the future. Even after the establishment of the American Political Science Association and the constitution of Political Theory as an official sub-field, political theory and/or political philosophy was basically a category that referred to certain types of claims and kinds of literature, various elements or functions in politics, and some reflections on the study of politics.

Why, exactly, the discipline of political science was officially born in 1903 with a sub-field called Political Theory and, precisely, how it came to receive this name is difficult to say. In part, it probably reflected the traditional theory/practice distinction in nineteenth century philosophy and the idea of social science as being concerned with the theory of the state. But to some extent, political science at the time of the creation of the APSA was less a distinct discipline than a holding company for a variety of endeavors that were in various ways related, but no longer easily resided in other disciplines. Political Theory was in part such a field.

Up through the late 1700s, the study of ethics and moral philosophy had included politics and political philosophy. When Francis Lieber, "the beginner in the United States of the systematic study of politics" (Haddow, 1939, p. 139), was appointed in 1857 as Professor of History and Political Economy at Columbia, he indicated his intention to teach and lecture on Political Philosophy which was concerned with the theory of the state and with political ethics. After political science became a distinct discipline in many universities after the Civil War, political theory began to find an even more definite place, and by the early 1890s, a "History of Political Theories" course appeared at Harvard (Haddow, 1939, p. 175). Political scientists, looking for their ancestors, would find them in the classics of moral philosophy that were concerned with politics, and thus began the idea that there was a tradition of political thought to which political scientists would understand themselves to belong.

In 1876, John Burgess succeeded Lieber at Columbia where he established the graduate school of political science which opened in 1880 and by 1891 consisted of three departments including the department of History and Political Philosophy. Courses dealing with the history of political theory and the philosophy of the state appeared here and at other major universities. The influence of Bluntschli's *Theory of the State* was evident as well as Yale's Theodore D. Woolsey's widely read text on *Political Science, or, the State Theoretically and Practically Considered* (1878). W. W. Willoughby, one of the most important figures in the early discipline of political science, particularly in Political Theory, was offering courses in both areas, first at Stanford in 1894 and then at Johns Hopkins in 1895; he published *An Examination of the Nature of the State; A Study in Political Philosophy* in 1896. With the establishment of the APSA, the general context for the development of *PT* took determinate form.

THE BEGINNINGS OF A DISCIPLINE: 1900-1919

During the early years of the discipline, political theory was still viewed more as a subject matter than a mode of analysis. Munroe Smith had stated in 1886, in the first issue of *Political Science Quarterly,* that the "domain of political science" was the historical and comparative study of the state, and this included what people had thought about it. To write the history of political theory was at once to write about the history of democratic institutions and about the development of political science which from the beginning was already traced through the canon of classic texts from the Greeks to modern political science. The influence of the evolutionary theories of Comte and Spencer as well as the Hegelian analysis of the state, with its subjective and objective division, was strong and provided the basic intellectual context in which both political science and the study of the history of political theories emerged.

The APSA was created for the purpose of "advancing the scientific study of politics in the United States," and six sub-fields, including Political Theory, were established with corresponding committees. The first Political Theory committee consisted of Willoughby, Charles Merriam, and William Dunning. In his presidential address to the APSA in 1904, Frank Goodnow said that political science was the study of the state and the "realization of the State will," and he viewed political theory as a special discipline concerned with the authorities that express that will. Furthermore, he suggested, "however contemptuous may be one's belief in the practical value of the study of political theory, it is none the less true that every governmental system is based on some more or less well-defined political theory" (1905, pp. 37-38). Political theory was understood in one way or another to be concerned with *ideas* in and about politics. Willoughby's 1904 article on the newly formed APSA claimed that the field consisted of three basic parts, and the first was the "province of political theory and philosophy" which aimed at "the analysis and exact definition of the concepts employed in political thinking" and a consideration of the "nature of the state" (p. 118). Most of the material published in Political Theory through 1920 was historical in the sense of Dunning's paradigmatic treatises on the "history of political theories" (1902, 1905, 1920). Although there was some precedent and parallel in European literature, the history of political theory was a distinctly American genre (Gunnell, 1979).

For Dunning, the study of political theory was the study of transformations in political consciousness and a way of scientifically grasping the dynamic character of politics that resulted from the interaction of institutions and ideas. Not only were science and history understood as integrally related, but the idea of political science as a practical science was a regulative assumption. This was true both in the older sense of providing political or citizen education and in the somewhat newer sense of a concern with participating in social reform and modes of social control that characterized most of the social sciences of this period. Between 1900-1920, the progressivist ideology consistently found expression in social science which was viewed by many as the link between knowledge and politics. Henry Jones Ford (1906) argued that political science must be universal in its scope, put on "an objective basis," and "experience the reconstruction which the general body of science has undergone at the hands of inductive philosophy," but the purpose was to "bring political science to a position of authority as regards practical politics" (pp. 203, 206).

After 1910, political theory as a specific subject was increasingly neglected as political scientists focused on practical issues in domestic and international politics, but the views of the formative period persisted. R. G.

Gettell, who wrote one of the most popular political science texts (1910) and one of the major works in the history of political theory (1924), offered an extended analysis of the nature and scope of political theory which reflected the dominant view in the discipline. Political theory in general was understood as reflection on the institutional state or the "objective" phase of the state which grew out of the need to cope with the environment. Thus political theory was not ultimately true but "relative in nature" and both cause and effect in that it simultaneously influenced and mirrored politics (1914, pp. 48, 50). At the same time, he noted that a fundamental and revolutionary change was taking place in political theory whereby it was being transformed from a deductive, normative, and idealist enterprise to an inductive and realistic one concerned with observation, classification, description, and generalization. Gettell distinguished three distinct but related elements of political theory: historical, analytical or descriptive, and applied (pp. 52, 54).

THE FIRST REVOLUTION IN POLITICAL THEORY: 1920-1929

In 1921, Merriam, in his presidential address to the APSA, announced that while he had intended to survey the field over the past four decades, he had decided, instead, to speak to the more pressing problem of the "reconstruction of the methods of political study" (p. 174). This would require that the "theory of politics" be transformed so as to reflect the substantive modern doctrine that "political ideas and systems . . . are the by-products of environment" and the methodological advances that had been made possible by "statistical observation" and the more accurate measurement of "facts and forces" (p. 174). Merriam's particular concern was with developing a theoretical "medium" (p. 175) for selecting and classifying the mass of facts that social science was accumulating and with releasing political theory from ideology or the "service of class and race and group" (p. 178). For Merriam, the basic aim was not pure science but rather the "cross fertilization of politics with science" and the more effective control and organization of practical matters in domestic and international politics. The goal of political science was to "interpret and explain and measurably control . . . the forces of human nature" (p. 183). But Merriam also believed that before the "processes of social and political control" could be grasped, it was necessary to have a "better organization of our political research." It would be impossible to contribute to "political prudence" if there were "anarchy in social science, or chaos in the theory of political order" (pp. 184-185).

Merriam's claims and concerns did not represent the disciplinary consensus. He was the major figure of that period in political science, and he influenced both his contemporaries and later phases of the discipline. But his ideas did not reflect the dominant research, publications, and curriculum of his own time. On the other hand, it is easy to mistake the degree to which Merriam's notions of theory and science diverged from his predecessors and contemporaries. What is striking, if compared with the later tension between the ideas of science and history, is that although Merriam attacked the historical-comparative approach in political science, this did not entail a confrontation with the history of political theory as conducted by individuals such as Dunning, Gettell, and C. H. McIllwain. All of these men agreed with regard to their assumptions about the relativity of ideas and institutions, the evolution of political thought toward science and democracy, and the practical mission of political science.

The APSA Committee on Political Research in 1923, under Merriam's leadership, identified social methodology as the "recent history of political thinking" (p. 275), and Merriam stressed the need in political inquiry to draw upon the methods of economics, statistics, history, anthropology, geography, and psychology as a basis for the "observation and description of actual processes of government" and for eschewing the older "a priori speculation" and the juristic and historical/comparative approaches (pp. 281-283). Here Merriam first offered his famous historical typology of the development of political inquiry that would appear in *New Aspects of Politics:* a priori deductive up to 1850; historical and comparative between 1850-1900; a tendency toward observation, survey, and measurement from 1900 to the present; and the future pointed in the direction of the "psychological treatment of politics" (a characteristic of the work of Graham Wallas and Walter Lippmann) (p. 286). Like later attempts to transform the discipline and distinguish what was conceived as innovation from the burden of the past, the image of the past was a somewhat contrived one, but the rhetorical point was clear.

In the "Report of the National Conference on the Science of Politics" in 1924 (previous conferences had been held in 1922 and 1923), it was maintained that "the great need of the hour is the development of a scientific technique and methodology for political science" (p. 119). But this was still understood as serving the practical concern of providing a scientific guide to legislation and administration which government had neither the time nor capacity to develop. What was required was intellectual authority if political science was to play a role and, therefore, a "fact-finding technique that will produce an adequate basis for sound generalization" and put "political research upon a scientific, objective basis" (pp. 120-121). Merriam argued that "the perfection of social science is indispensable to the very preservation of this same civilization" that created modern science.

THEORETICAL CONTINUITY: 1930-1939

Substantively, as well as methodologically, the 1930s were years of affirmation: affirmation of science, democracy, and their complementarity. Political theory was also still largely a functional or analytical category, and as a classification of literature, it referred to subjective matters, both cognitive and ideological. This tendency gained support, and maybe some new meaning, with the psychological concerns of Merriam and others in the 1920s, but long before and after, dissertations in Political Theory in the *APSR* were listed under "Political Theory and Psychology."

It was not until 1930 that Political Theory was offered as a distinct section in the annual meetings of the APSA, yet by the end of the 1930s, political theory as a category disappeared from the annual program as increasing pressures of domestic and international affairs drew attention away from issues about the scope and method in political science. Disciplinary concern in political theory was concentrated in two areas.

It was during the 1930s that the history of political theory fully crystallized as a genre of literature, and this literature largely embodied attempts to demonstrate and celebrate the development of liberalism and its divergence from fascism and communism. Many textbooks with similar versions of this history were published during the 1930s; the genre culminated in George Sabine's *A History of Political Theory* (1937) which, as David Easton (1953) has noted, "exercised deeper influence over the study of political theory in the United States . . . than any other single work" (p. 249). Writers of this period were untroubled about a simultaneous commitment to the study of the history of political theory and to the development of a scientific study of politics. For proponents of scientism, such as George Catlin (1939), the two endeavors were mutually confirming.

With respect to the trends initiated by Merriam, no one in the 1930s contributed more to their perpetuation than Harold Lasswell. The commitment to the idea of a science that could play a role in the reformation of society was not only sustained but in some respects radicalized. Lasswell (1939, 1941) put less emphasis on education than on the development of theories and testable hypotheses that would expose the reality behind politics and political ideology—largely a psychological reality—and provide the basis of a therapeutic policy science. Some of the work of Lasswell in the 1930s would recede as a matter of disciplinary concern, but his behavioral realism, as well as his emphasis on science and policy, would make a lasting impression on the aspirations of political science as a science.

Sabine (1939) suggested that political theories characteristically consisted of three logically distinct kinds of propositions: factual, causal, and valuational. He stressed the need to distinguish these aspects in both understanding the past and analyzing contemporary claims. The question of the status, relationship between, and priority of these aspects would occupy both *PT* and the discipline as a whole for some time, but even by the end of the 1930s, many political scientists were unhappy with what the discipline had achieved with regard to fulfilling the vision of science.

Some saw the problem as the failure of science, and others saw it as a failure to be adequately scientific. For the latter, who would eventually dominate the discussion, the problem seemed increasingly to be one centered around the issue of theory. What they perceived as a theoretical core in other disciplines seemed to be missing in political science. By the 1950s, many individuals would again be stressing that what was required in political science was a theoretical revolution, but the seeds of that second revolution were sowed in this much earlier period.

THE PRELUDE TO BEHAVIORALISM: 1940-1949

The claims about theory and science that are so familiar from the 1950s did not appear on the scene as suddenly as we are often led to assume. Benjamin Lippincott (1940) argued that, for the most part, political scientists still equated empiricism with fact collection and that this had been the case since the turn of the century. Political scientists still looked upon "theories or ideas about the facts [as] not only unnecessary but positively dangerous" (p. 130). Lippincott pointed out, however, that theories are always implicitly involved in the selection of facts and, consequently, the biases of the discipline are often concealed. But if theory was required, it was to be theory of a particular sort. William Foote Whyte (1943) represented an increasingly popular point of view when he claimed that political scientists should "leave ethics to the philosophers and concern themselves primarily with the description and analysis of political behavior" (p. 692).

In the 1940s there was manifested a new and focused concern with theory, but there was, from the start, a somewhat different emphasis on just what it was and how it should be related to political science. The pressures associated with the early years of the war had curtailed collegial discussion of political science and political theory, but by 1943 the Political Theory panel of the APSA Research Committee attempted to pick up and sort out the issues. Although no uniquely new idea of theory emerged from these discussions, there was an emerging sense of a "deep cleavage" on an "ultimate issue" which even the participants had difficulty articulating (Wilson, 1944, p. 726). This cleavage was variously understood in terms of "philosophical" reflection on the study of politics as opposed to "logical analysis"; the "theological approach" versus the "empirical" or increasingly popular " 'positivistic,' scientific, or *liberal* technique of social study"; the idea of a value-free science seeking laws of political behavior as opposed to a more evaluative set of concerns; and the conflict between the "philosophy of history" and an emphasis on "ends-means relationship" (Wilson, 1944, p. 727, emphasis added).

Although the behavioralist attack on the history of political theory in the 1950s is often characterized as an "offensive," there is reason to suggest that it was a preemptive strike or a conservative reaction in defense of the traditional "liberal technique of social study" that had dominated the discipline long before the war. With the influence of emigre scholars such as Eric Voegelin and Leo Strauss, new and somewhat "foreign" issues dealing with natural law, relativism, and positivism were being introduced. There is no doubt that at this time Political Theory did equal, in large measure, the history of political theory, but the tradition of which Sabine was a part was not hostile to the equally traditional commitment to science. Something new was happening in Political Theory which was not easily defined. One can point to very little in the way of concrete literature, but individuals such as Voegelin were becoming active in the profession. Marcuse's work would not become a strong influence in political theory until the late 1960s and early 1970s, but *Reason and Revolution* with its anti-scientific implications appeared in 1941. Although the great tradition of political theory was deeply embedded in American political science, the new generation of philosophical historians would soon give that tradition new meaning, and the study would take on a different significance that involved less the celebration of modern values and social science than a critique not only of modernity but even of the liberal vision of science and democracy.

There is little question but that the impending conflict between "scientific" and "traditional" theory was also a consequence of the increasing shift toward a vision of pure as opposed to applied science, and a perceived need, for several reasons, to place theory on an even firmer footing by demonstrating its "scientificness." However, the discourse about political theory through the 1940s increasingly revolved around questions of ethics, relativism, and positivism which tended to produce a redefinition and reconstruction of many issues in American political science (Hallowell, 1944; Brecht, 1948).

Historically, these issues had not been a matter of great concern to American political scientists. While in Europe fundamental value choices and the grounds for them were practical as well as philosophical problems, the American consensus and the belief in progress had made pragmatism and instrumentalism a reasonable position. The relativism of political values and beliefs had been constantly stressed by historians of political theory from Dunning to Sabine as well as by the more scientific school of individuals such as Merriam and his successors. Although the atmosphere preceding and during the war had raised questions about the ground of democratic values, relativism had been basically viewed as in some way essential to liberalism. For many of the Europeans and others touched by the transcendental urge, the issue was somewhat different. Their philosophical background lent great suspicion to the very enterprise of modern science which Americans had trusted so basically, and, particularly, to its impact on politics and society. Issues that concerned positivism, relativism, and historicism, were world-historical matters of political as well as philosophical urgency that manifested themselves in modern events.

The decade ended with the growing conviction of many political scientists that there was a definite need for more work in the "field of scientific method" that would yield "a body of testable propositions concerning political nature and activities of man that are applicable throughout the world" and "at all times" and that in the end would make possible a science of "human political behavior" (Anderson, 1949, pp. 309, 314-315). This summed up the governing motif of the forthcoming behavioral movement, but it was also a reaffirmation of a basic faith. The rekindling of the scientific mood was still tied to concerns about the realization of liberal values; this connection would become, if not more attenuated, at least more submerged. The growing sense that political science in the post-World War II period must be more than a science of American politics and that liberalism was more than an American mission moved the discipline further toward articulating a vision of a universal conception of political science.

THE BEHAVIORAL REVOLUTION: 1950-1959

The behavioral revolution was a theoretical revolution in several senses. First, it was a revolution in the theory of science. It introduced an unprecedented metatheoretical consciousness about scientific theory and scientific explanation. Second, much of the energy of behavioralists went into calling for, creating, and applying, what they took to be theories. Third, there was a distinct emphasis on pure or theoretical science and a turning away from the idea of liberal reform and social control as the rationale of social science. Finally, many of the individuals who were centrally involved in effecting the behavioral revolution were by training political theorists of the historical and normative kind; they were what they sought to replace. This group, who at least wrote dissertations in traditional political theory, included David Easton, Robert Dahl, Heinz Eulau, John Wahlke, Karl Deutsch, Herbert McClosky, Albert Somit, Ithiel de Sola Pool, Alfred and Sebastian de Grazia, and Austin Ranney, to name just a few.

The 1950s were the crucial decade in the development of both *PT* and *pt*. Although there were echoes of the move toward scientism in the 1920s, there were some important differences. First, the image of science embraced by behavioralism had a significantly greater impact on the research programs within the discipline. For the most part, historical, legal, and descriptive institutional analyses actually did dominate the field well into and beyond the 1940s; in the early 1950s, the belief that the practice of political science had not lived up to its scientific promise was vehemently expressed. Samuel Eldersveld (1951) noted that it was not necessary to change a syllable of Merriam's 1925 statement that there are "signs of hope that genuine advance may be made in the not distant future toward the discovery of scientific relations in the domain of political phenomena" (p. 87). Second, although some still linked the need for a "profound understanding of political behavior" to practical issues and the advance of democracy (White, 1950, p. 18), few explicitly held fast to the old alliance. Lasswell (1950, 1951) continued to remind the discipline that the basic purpose of political science was "to decrease the indeterminacy of important political judgments" and enhance the chances for a rational democratic society (1950, p. 425), but for most behavioralists the internal scientific conversion of the discipline was the first order of business. Third, the discussions focused in a much more specific way on the concept of theory and the place of theory in science. The view was that any change in the direction of scientific progress would require a change in the nature of theory in the discipline. Fourth, the claims of behavioralists about science and theory fractured both the discipline and the sub-field of Political Theory. Fifth, the arguments in defense of and opposition to behavioralism engaged political theorists in a wide range of philosophical and metatheoretical arguments. Finally, a distinct difference between *PT* and *pt*, and the problem of the relationship between them, became apparent.

Lippincott, in 1950, attempted a critical analysis of the field of Political Theory. He took it to be the "most scientific branch" of political science but an area in which political scientists had "produced little" if theory were defined, as he believed it should be, as "the systematic analysis of political relations" (p. 208). The methods in Political Theory had been basically historical, and the "emphasis placed upon the history of political ideas has meant very largely the abandonment of the aim of science" (p. 214). Some of the shortcomings of Political Theory could also be attributed to the "inadequacy of empiricism," or the crude inductivist misunderstanding of empiricism which eschewed both evaluation and generalization (p. 218). Political theorists had not contributed much to understanding the great political issues and events of the age.

The call for scientific theory was often accompanied by an attack on current practices in the discipline and particularly on Political Theory. Theory was characterized as teleological, moralistic, historical, ethical, and, in general, in about the same state that Aristotle left it. Herbert Simon (1950) was one of the first to suggest that much of the previous work in the field did not deserve the name of theory and that it was "time that we maintained a consistent distinction between political theory (i.e., scientific statements about the phenomena of politics) and the history of political thought (i.e., statements about what people have said about political theory and political ethics)" (p. 411). He emphasized the need for interdisciplinary model construction that would yield predictive generalizations and observably testable propositions.

Exactly what was involved in this commitment, or recommitment, to science and method was not very clearly defined. Most of the claims about science were in terms of very abstract demands about the need for observation and generalization and reflected versions of arguments about the logic and epistemology of science that came, in a secondary or tertiary manner, from the philosophy of science. The new scientific outlook was informed by—or perhaps, more accurately, justified by—"a thorough-going empiricist philosophy of the sciences" based on "logical positivism, operationalism, instrumentalism" (Lasswell & Kaplan, 1950, pp. xii, xiv). But no one was directly coming to grips with the question of exactly how these philosophical claims related to social scientific practice.

When viewed in historical perspective, Easton's now classic analysis of the "decline of political theory" (1951; 1953) does not seem to startling. His indictment of Political Theory for falling into historicism and failing to engage in either creative and relevant evaluative theorizing or developing causal scientific theory reflected, as well as gave impetus to, an increasingly concretely articulated sentiment. Easton's initial essay appeared as part of a symposium on the relationship of political theory to political research. All the essays advocated a sharp break with past modes of theorizing; "the integration of theory, methodology and research; interdisciplinary co-operation; and a treatment of theory as much more than utopian goodness" (de Grazia, 1951, p. 35). Although it is possible to recognize some attributes of the past literature in these critiques of the early 1950s, it is equally clear that these characterizations did not accurately represent either the motives or practice of most past political theorists. In part, the critique aimed at creating an image for justifying a different direction in concrete research programs in the discipline. But the problem also seems to have been what political theory was feared to be becoming; it was charged with moralism and antiquarianism more on this basis than on the basis of what it had been.

I have stressed this "conservative" aspect of the behavioral critique because it has been neglected, but it is necessary to recognize that actual research in political science had never corresponded to the scientific vision of the discipline, and the critics were determined to change the practice. To do this required an image of the past as well as the future. Although it would be a mistake to attempt an explanation exclusively in terms of the sociology of knowledge (response to funding sources, the need to clear political science of ideological bias, etc.), it is useful to note that it was a matter of concern to political scientists that "the National Roster of Scientific and Specialized Personnel, set up to find useful talent during World War II, classified Political

Theory on the advice of the APSA as concerned with political ethics and the history of political ideas" (Smithburg, 1951, pp. 61, 68). The notion that political theory was at the time basically the history of ideas was probably true in terms of the portion of the university curriculum that was designated as Political Theory, and, apart from some scope and methods courses, this would remain generally the case through the mid-1960s at most institutions.

Two developments in the 1950s are quite clear. First, there was the establishment of a basic dispute between "new" and "old" ways of theorizing—based on less than accurate images albeit accepted by protagonists on both sides. Second, *PT* was slowly but consistently differentiated from, but involved with, the wider realm of *pt* that was in the process of formulation.

This attention to the concept of theory during the 1950s began to have the effect of changing it from a category and a subject matter designation to a term that was assumed to refer to a definite element in both science and politics. Furthermore, the focus on theory created a whole realm of discourse and controversy about the "theory of theory" that became increasingly difficult to separate from theory itself and that had a considerable impact on theoretical practice. By the early 1950s, the debate about science and theory was already being transformed into a debate about philosophical positivism. In a symposium on "Recent American Political Theory," the comments by Simon on an essay by Dwight Waldo had less to do with the substantive issues than with Simon's perception of Waldo as a political theorist attached to a pre-scientific conception of that role. Simon charged that the essay was "characteristic of the writings of those who call themselves 'political theorists' and who are ever ready to raise the battle cry against positivism and empiricism" as a threat to democracy but continue to write in a "loose, literary, metaphysical style" (1952, pp. 494, 496).

One of the earliest attempts to specify the content of the behavioral vision of political inquiry was the 1952 report of the Social Science Research Council seminar on political behavior. Here the approach was "distinguished by its attempt to describe government as a process made up of the actions and interactions of men and groups of men" and to "discover the extent and nature of uniformities" (p. 1004). These goals were to be accomplished by the formulation of systematic concepts and hypotheses; the development of explanatory generalizations that would raise inquiry beyond mere factual empiricism; interdisciplinary borrowing; empirical methods of research; direct observation; and a distinct separation from concerns about "how men *ought* to act" (p. 1004). This would all be codified in the behavioral credo by the mid-1960s (Easton, 1965a, p. 9).

A significant factor in this behavioral affirmation was the problem posed by the emerging field of comparative politics that seemed more than any other aspect of the discipline to require a theoretical advance in order to deal with new and complex data. An SSRC report in 1952 stated that "the problem of comparative method revolves around the discovery of uniformities" and the "need for taking some kind of methodological position prior to or along with the collection and descriptive enumeration of facts" (1953, p. 643). This sense of being overwhelmed by facts was hardly novel in the history of the discipline. Although the answer to the problem that was given—political theory—was not novel either, the commitment to the answer in the form of developing a "scheme of inquiry" or "analytical scheme" that would give direction to inquiry and form to the data was unprecedented. The possession of a model or conceptual framework would be the badge of the empirical political scientists in the 1960s.

The poles of the argument within the discipline were increasingly represented in terms of normative concerns and the study of the history of political theory versus theory as part of an empirical science of politics (Easton, 1953; Hacker, 1954; Driscoll & Hyneman, 1955; Glaser, 1955). At the same time, "theoretical" controversies were fast becoming more metatheoretical debates. By the mid-1950s, political theory could be considered a subject of analysis with a whole range of types and categories that would have been difficult to conceive in the 1930s (e.g., Jenkin, 1955). For behavioral political scientists, the problem was to find out what theories were in science and what role they played and to build or construct them. Thus, methodology involved not only techniques of research but reading in the "fields of epistemology, logic, philosophy of science" (Driscoll & Hyneman, 1955, pp. 192-193). To turn to this literature, or more commonly to secondary accounts of it, was to turn to a field almost entirely dominated by logical positivism and logical empiricism; this then, was reflected in the discipline of political science and in the image of science accepted by both the advocates and opponents of behavioralism.

Even a conference on political theory in the study of politics that was by no means dominated by proponents of the behavioral persuasion was reported as reaching a consensus on the view that "all types of inquiry involve the construction of theory" and "that the title of 'political theory' has been unjustifiably appropriated by historians of political thought" (Eckstein, 1956, p. 476). The spirit of the conference seemed to be in favor of demonstrating that there was a "false distinction between 'behaviorists' and 'theorists,' " but the conferees seemed unable to end the distinction. There was a major split between the "behaviorists"—they were not quite united on the term—who wanted "to transform the field of political studies into a genuine scientific *discipline*" and "anti-behaviorists" (or political philosophers) who believed that "the end of the study of politics was something called political *wisdom*" (pp. 476-477).

By the late 1950s, it was generally agreed that political theory had "entered upon a time of troubles." On one side were those who saw theory as a "historical, reflective, and 'literary' discipline more akin to moral philosophy" than science (Smith, 1957, pp. 734, 743), and on the other side were those who saw it as a set of systematic generalizations for dealing with empirical data. For some, the theoretical enterprise was grounded in a search for political wisdom, and for others it was based on a "greater clarification of the epistemological foundations of science" and "training in theory construction" (Apter, 1957, p. 761). V. O. Key, Jr.'s analysis of the state of the discipline in 1958 focused on this dilemma. Key suggested that most of the "worries about the state of our discipline relate in one way or another to the place of political theory in our studies" (p. 967). While theory had been understood largely as the history of political thought and had possessed a relatively autonomous place within the discipline, the behavioral unification raised the question of "what relevance has political theory for other branches of political science." Many believed that a radical reconstruction of the sub-field was necessary. Key claimed that it was clear that there was an "odd relation" between "theoretical and empirical work" that tended to be "one of antagonism, if not hostility" and which had important implications for the future of the discipline (pp. 967-968). For some, such as Norman Jacobson, the situation by the late 1950s was such that the autonomy of political theory was threatened. There was a danger of its absorption into the poles of scientism and moralism as a consequence of the growing distance between "scientific" and "ethical political theory" (1958).

Another view of the fate of Political Theory was offered by Robert Dahl. In an extended review of Bertrand de Jouvenel's book on *Sovereignty,* Dahl treated it as a "serious" but, in many ways, vestigial "effort to do political theory in the grand style. In the English-speaking world, where so many of the interesting political problems have been solved (at least superficially) political theory is dead. In the Communist countries it is imprisoned. Elsewhere it is moribund" (1958, p. 89). Dahl believed that political theory had been reduced, in political science, to a kind of parasitic form of "textual criticism and historic analysis," and that although attempts to return to political theory in the grand manner in the modern age were to be applauded, they were faced with the inherent "impossibility of satisfying the scientific function of political theory" (p. 95).

Many of those individuals who worked within the traditional sub-field of Political Theory were, both through inclination and coercion, pushed further away from the mainstream of the discipline. In the late 1950s, however, political theorists who felt alienated were able to find little support in a wider literature of political theory. Today we are accustomed to the notion that political theory is a general field drawing from, and constituted by, work in a number of disciplines including political science, history, and philosophy; such a field simply did not exist or, at least, was seldom consciously perceived as existing in the 1950s. To the extent that it was perceived, it was seen as declining and it offered little support to political theorists in political science.

For individuals such as Strauss, the very notion of theory in modern social science, as exemplified in the behavioral movement, signified the "decline of political philosophy," while for Easton the identification of theory with the history of political theory was symptomatic of the "decline." But the argument about decline was also coming from other quarters. Arnold Brecht (1959) elaborated his earlier views about relativism and the crisis of political theory; Judith Shklar (1957) found decline to be part of the syndrome of post-enlightenment thought; Alfred Cobban (1953; 1959) believed it was manifest in the alienation of political theory from politics and its transformation into an academic discipline as well as the tendencies of historicism. Finally, the notion of the decline of political philosophy and/or theory was beginning to appear outside political science. It was a product of perceptions of the implications of positivist ethical theory for the status of moral reasoning and notions of rationality in normative discourse. Philosophers such as T.D. Weldon (1953; 1956) in effect suggested that much of traditional political philosophy rested on a mistaken belief that moral and political principles were, like empirical scientific claims, in some substantive sense demonstrable. In the early 1950s, philosophers did not really have much basis for questioning the positivist image of either science or ethics, and to the extent that the possibility of political philosophy was tied to the belief in grounded value judgments, Peter Laslett (1956) concluded that the "tradition has been broken" and "political philosophy is dead."

RECOUPING AND REGROUPING: 1960-1969

Although the tension between the images of traditional and scientific theory was a central motif in the literature of the 1960s, other things were happening in Political Theory. As a result, any general characterization of the state of the field is more difficult than it was in the 1950s.

The writings associated with the Straussian persuasion in political philosophy (e.g., Strauss, 1959; Jaffa, 1960; Storing, Ed., 1962; Strauss & Cropsey, Eds., 1963) and to a lesser extent the work of Voegelin (1952; 1956) and Hannah Arendt (1958; 1961) represented a distinct counterpoint to behavioralism both in that they dominated work in the history of political theory and in that they constituted an alternative to the behavioral approach and its philosophical assumptions. For some, this indicated a "revival" of political theory (Germino, 1963). Sheldon Wolin's *Politics and Vision,* published in 1960, was in many respects a significant departure from the type of historical analysis associated with Sabine and at the same time represented a position that was not altogether compatible with the direction of arguments such as those of Strauss (e.g., Schaar & Wolin, 1963). Wolin indicated that the great tradition was the basic object of the study of political theory and stressed its relevance for understanding and dealing with the present. He offered a general view about the decline or "sublimination" of political theory in the modern age that reinforced the general mood in political philosophy. More than any other work of the period, his book focused on the very idea of theory, the theorist, and the activity of theorizing in a manner that constituted, at least implicitly, a challenge to the behavioral notion of theory.

The presidential address to the APSA in 1961 suggested that behavioralism and "the move toward scientism has made it more essential to define the role of political philosophy in the study of political science" (Redford, p. 758). But there were growing indications that the tensions would not abate and that much of political theory would seek its reference point outside the discipline of political science. There were pleas for reintegration, or at least complementarity (e.g., Thorson, 1961; Bluhm, 1965), but these often pleased neither the behavioralists nor their opponents. It was, however, not the case, as it is sometimes believed or suggested, that political theory, as history and philosophy, was ostracized. The pages of professional journals during this period contained more political theory of the non-behavioral genre than ever. Although political scientists as a whole apparently did not believe that, as a field, Political Theory was producing very significant work, it was the only field between 1953 and 1961 that indicated any "startling" increase in interest (Somit & Tanenhaus, 1980). It is probably accurate to say that what was taking place was less the rejection of traditional political theory than its differentiation.

By the late 1960s, the profession had officially divided Political Theory into three parts—"Political Theory and Philosophy: historical, normative, and empirical" (APSA Biographical Directory, 1968). Dissertation listings in the *APSR* changed to "Political Philosophy, Theory, and Methodology" and the latter category (which included methods as well as metatheoretical material in the philosophy of science and philosophy of social science) was also part of the listing for Book Notes and Bibliography. Annual meetings of the APSA, by the late 1960s, began to make a definite distinction between empirical political theory and political philosophy (or "traditional political theory"). By this time, the Foundations of Political Theory, the Caucus for a New Political Science, and various other unaffiliated groups that represented the diverse concerns of political theory in both *PT* and *pt* had begun to offer panels at the annual meetings. But whether the basic distinctions within political theory were bipartite or tripartite or even more complex, they did not adequately capture the diversity that was becoming characteristic of the field. The problem is to disentangle some of these elements and reconstruct the relationship between them.

The 1960s were a decade of optimism about the advance of scientific theory and the achievement of the behavioral goal of "a science of politics modeled after the methodological assumptions of the natural sciences" (Easton, 1965a, p. 8). Although, today, some of the more extravagant claims about the development of predictive causal theory that would rival modern physics (advanced at that time) seem less than credible, these claims were not simply rhetorical. The vision was one of a "general theory" that would consist of "a deductive system of thought so that a limited number of postulates, as assumptions and axioms, a whole body of empirically valid generalizations might be deduced in descending order of specificity" and provide predictive causal explanations of political behavior (Easton, 1965a, p. 9). Behavioralists believed that theories were arising, or would arise, inductively from a wide variety of empirical studies and data gathering (e.g., Berelson & Steiner, 1964); but more than anywhere else, the heartland of political theory for political science in the 1960s was located in the development of "models," "orientations," "approaches," "strategies" of inquiry, and various conceptual and analytical frameworks. This was the product of theory "construction" and theory "building" in the discipline. These activities were seldom accepted as entirely fulfilling, in themselves, the scientific vision, but they were seen as definite steps in that path or as prototypes of fully scientific theory. Whether it was a form of systems theory, structural functionalism, decision theory, game theory, or some other such construct, these "varieties" (Easton, Ed., 1966; Pool, Ed., 1967) were directed toward the same general goal of empirical theory.

Paramount to all of these efforts was some notion of politics as a "system." Karl Deutsch's *Nerves of Government* was published in 1963, and Easton's *A Framework for Political Analysis* and *A Systems Analysis of Political Life* in 1965. Sociologists such as Talcott Parsons, Marion Levy, and Robert Merton had a significant impact on analytical theory in political science during this period. By 1965, David Truman, drawing upon Thomas Kuhn's model of paradigmatic scientific change, revised Merriam's history and cast the story of American political science in terms of a movement from an early period (1880s-1930s) of "non-theoretical empiricism" or "almost total neglect of theory in any meaningful sense of that term" to "a new disciplinary consensus" based on the "emergence of an explicit interest in the political system" as well as a "revival of interest in theory" and a "self-conscious and fruitful awareness of the necessary conjunction of theory and empirical investigation" (pp. 867, 870, 871).

Truman noted that "the theory chorus" was in some respects "less polyphonus than cacophonus" and that "in practice" it might not be possible in the "predictable future" to develop "general models" that would constitute "hypothetico-deductive theories," but it was clear that contemporary political science had made "a recommitment to the goal of science" and that this recommitment was manifest in its theoretical program (pp. 871, 872). Truman allowed that political scientists still had a certain obligation to address normative issues and that the new consensus would, "for an indefinite future," include "a less or non-scientific component" which could best be filled by a study of the "classics" of political thought which the earlier non-theoretical consensus had "thrust . . . out of the mainstream" but which could again constitute part of a fruitful dialogue within the field (p. 874).

A survey of the field of political theory in the mid-1960s argued that "theory" in general should be understood as a concern with "what 'is' or exists in politics" and specifically as a "search for a coherent image of the political system" (Deutsch & Rieselbach, 1965, p. 139). Concerns about "ought" questions should be considered as part of "political philosophy" or some such category. Political scientists were still concerned that in Political Theory the "preoccupation has been with history, exegesis, and methodological conservatism" in "contrast to other fields in both natural and social science, where theoretical physics and economic theory are clearly distinguished from the history of past theories." The authors argued that a systems analysis of the field of Political Theory itself in terms of structural-functional categories might be useful. They concluded that, from this perspective, "at present political theory is not a well-integrated field, nor does it seem well-oriented toward a prominent goal," but they did see a healthy tendency toward "adaption" which they defined as becoming more scientific and less concerned with "pattern maintenance" or traditional political theory (p. 162). In 1966, Gabriel Almond, once more drawing upon Kuhn's scheme as a basis for a revisionist history of the discipline, reaffirmed Truman's views and claimed that "in the last decade or two the elements of a new, more surely scientific paradigm seem to be manifesting themselves rapidly" and that it could be summed up by the concept of the "political system" (p. 869).

By the mid-1960s the "heterodoxy" of behavioralism had become the "new orthodoxy" (Pool, 1967), and it was attacked as both politically and methodologically conservative. Critics were concerned with the substantive political messages both in what political scientists said and did not say. Many believed that whether it was the commitment to pure science or an implicit ideological bias, political science seldom spoke to, or about, significant current political issues. Despite the problems in Vietnam, the crises of the American cities, the civil rights movement, international tensions, uprisings on college campuses and the like, political issues were largely absent from the literature of political science. In most instances, it was political theorists who took up this critique.

As late as 1965 and 1966, individuals such as Robert Lane were speaking about the "decline of politics in a knowledgeable society" and about how the "age of affluence," beginning in 1950, had produced "a growing state of confidence between men and government" that in many respects was making both ideology and conventional politics obsolete (1965, p. 895; 1966). The notion of the "end of ideology" advanced by Daniel Bell (1960) and similar themes in the work of individuals such as Seymour Martin Lipset (1960) and the revision of democratic theory in the voting studies to accommodate empirical findings (Campbell, *et al.*, 1960) were giving support, or otherwise linked, to various claims about the decline or death of political theory (see Partridge, 1961; Rousseas & Farganis, 1963; LaPalombara, 1966; Waxman, Ed., 1969). In 1968, the introduction to a symposium on the "advance of the discipline," noted that "the discontinuity between classic Political Theory and modern political theory is obvious." Political theory with a normative emphasis "has had much less appeal to the post-war generation of political scientists," and this was a consequence of the fact that the "official ideologies of the 1950s became increasingly unsupportable in empirical theory and untenable in normative philosophy" (Irish, 1968, pp. 298, 299).

During these years, the liberal-pluralist vision of political reality, which was embedded in much of American political science and reflected in both its methodology and substantive concerns, was criticized both as a descriptive and prescriptive claim. Although behavioralism, mainstream political science, and advocates of pluralist democracy were not entirely congruent categories, they often coincided in the work of individuals such as V.O. Key, Jr., David Easton, Robert Dahl, Gabriel Almond, and David Truman. Dwight Waldo (1956) had already noted, in the middle of the previous decade, that the basic structure and values of the American political order had been accepted and endorsed by political science and that the very idea of science in the discipline assumed a fundamental agreement on ends that allowed political theory to be transformed into methodology. This theme was more fully developed by Bernard Crick in 1959. Individuals such as C.W. Mills (1956; Horowitz, Ed., 1963) and Barrington Moore, Jr. (1958) had raised questions about both the conservative stance of social science and its account of the political world; arguments such as these became the property of dissident political theorists who attacked the theory and practice of American politics in both domestic and foreign affairs (e.g., Green & Levinson, Eds., 1970).

From the beginning to the end of the decade, the health of pluralist liberalism was questioned by individuals such as Henry Kariel (1961; 1969) and Theodore Lowi (1969). Its philosophical assumptions were criticized (Wolff, 1969), and the ideological biases of political science hidden behind claims to scientific objectivity were examined (Connolly, 1967; Connolly, Ed., 1969). There was, it was claimed, substantive political theory embedded in the disciplinary matrix of political science and in its various methodologies and conceptual frameworks, and political theorists turned to a critique of those assumptions. Arguments such as those of Dahl (1961) and Nelson Polsby (1963) about the character and structure of political power were countered by individuals such as Bachrach and Baratz (1962a; 1967) and Jack L. Walker (1966), and revisionist notions of democracy advanced by political sociologists and political scientists were attacked also (Duncan & Lukes, 1963; Davis, 1964). It was argued that there was a "behavioral syndrome" consisting of "*conservatism, fear of popular democracy,* and *avoidance of vital political issues*" (McCoy & Playford, Eds., 1967, p. 10); by the end of the decade the Caucus for a New Political Science claimed that there was, or should be, an "end to political science" as it was practiced (Wolfe & Surkin, Eds., 1970). For the most part, there was, however, little direct joining of issues. While some voices, such as that of Morgenthau, pointed to the "complacency" of Political Theory and the lack of social purpose in political science, the discipline continued to emphasize methodology and technique (Charlesworth, Ed., 1966).

Some of the material published in the late 1950s and 1960s that would significantly contribute to shaping the concerns of political theory in the 1970s was still invisible, or barely visible, in both the literature and the curriculum. This included such works as Marcuse's *One Dimensional Man* (1964) and Arendt's *Human Condition* (1958). A category such as Critical Theory was simply not part of the discourse of the 1960s. Although some of this material, as well as Continental literature reflecting neo-Marxist thought, existentialism, and phenomenology, was discussed by political theorists in courses and professional meetings, the discussions rarely surfaced in the professional publications. For example, not one article in either the *APSR* or *JOP* in the 1960s dealt in any direct way with such themes. The material that would constitute the substance of *pt* in the 1970s was appearing, but the form was not yet evident.

More prominent in the literature of the period was the influence of British analytic philosophy and linguistic analysis. By the early 1960s, the pessimism about the condition and future of ethics and political philosophy that had marked the 1950s had given way to a guarded optimism based on the appearance of a number of works in post-positivist philosophy and particularly in the area of moral reasoning (e.g., Toulmin, 1950; Hare, 1952; 1963). Certain thinkers were coming to the conclusion that social science and political theory were not mutually exclusive but rather complementary enterprises. All empirical work involved value assumptions, and empirical evidence was relevant in making and sustaining evaluative and prescriptive arguments (Runciman, 1963; Taylor, 1967). Political philosophy entailed both conceptual and substantive claims and, like normative reason in general, constituted a certain form of discourse that, while not scientific, was rationally autonomous. To the extent that political philosophy was a type of normative reasoning or involved producing knowledge in the form of conceptual clarification that guided action as well as understanding, it seemed to many that "political philosophy in the English-speaking world is alive again" and that it would "not wholly perish from the earth" (Laslett, 1967; Berlin, 1962).

The influence of this literature on *PT* was not very great in the 1960s, even though there were some important examples (e.g., Thorson, 1962; Flathman, 1966; Pitkin, 1967). What this approach promised was a way of actually "doing" political theory—rather than merely talking about its history and condition—and of doing it in a manner that was intellectually secure from the kinds of criticism leveled by positivists such as Simon in early years. It was rigorous yet normative.

Despite all the things that political theorists had said about science, few, if any, of these individuals had any concrete knowledge of, or association with, the practices of natural science. For the behavioralist as well as the anti-behavioralist, science was an image, and it was an image gained by and large from various philosophical sources. Both parties embraced basically the same image. As the issues revolving around the possibility and desirability of the scientific study of politics sharpened, the articulation of this image became a matter of great importance; increasingly it was inseparable from concrete theoretical claims and actual techniques of analysis. Before the 1960s, there were relatively few books available, outside technical literature in the philosophy of science, that spoke to the question of the nature of science and scientific method, let alone its application to social science (Cohen & Nagel, 1934; Kaufman, 1944). Arnold Brecht's *Political Theory* (1959) expanded the scope of the discussion about science, but few political theorists, let alone political scientists in general, were at that time interested in submerging themselves in the intricacies of European thought (positivism, natural law, phenomenology, etc.). Brecht was not happy with the implications of what he termed "scientific value relativism" for the future of normative political theory, but he was forced to confirm the positivist notion of scientific rationality and its impact on Political Theory as well as the positivist image of scientific explanation associated with the "hypothetico-deductive" model which nearly everyone equated with "science."

By the beginning of the 1960's, this genre of mediational literature standing between philosophy and social science began to bloom. One of the most influential works used in scope and methods courses of the period was Abraham Kaplan's *Conduct of Inquiry* (1964). Although Kaplan stressed the autonomy of inquiry, or the relativity of the logic of explanation with regard to the subject matter, and distinguished between philosophical idealizations of scientific explanation and the practice of science, he nevertheless presented a synthetic but in the end largely Americanized logical empiricist account of science that stressed the underlying unity of science and its philosophy. The book also suggested that it could provide a methodological foundation for the practice of inquiry in social science.

Much of the debate about science (e.g., Charlesworth, Ed., 1962), through the mid-1960s, had been about the possibility and desirability of applying scientific methods to the study of politics with both sides generally accepting the idea of the unity of scientific method and the account of natural science that had become standard in the literature of the philosophy of science, that is, logical positivism and the emendations of logical empiricism (Nagel, 1961; Hempel, 1965; Popper, 1961). However, by the late 1950s and early 1960s, both the approach and claims of this school were being challenged in the philosophy of science by authors such as Kuhn (1962) (see also, Hanson, 1958). The implications for the image and demands of science in the social sciences, as well as for the question of the relationship between science and the philosophy of science, were considerable, but Kaplan, for example, did not mention Kuhn.

Although Kuhn's impact on the literature of Political Theory was eventually of singular importance, there was a general growing awareness that more was involved than merely the question of accepting or rejecting the methods of the natural science. There were numerous instances of mediational works both in social science and the philosophy of social sciene (e.g., Gibson, 1960; Van Dyke, 1960; Brown, 1963; Taylor, 1964; Meehan, 1965; Braybrooke, 1965; Rudner, 1966; Frohock, 1967; Brodbeck, 1968; Greer, 1969; Isaak, 1969). Although much of it may have tended to confirm, and give aid to, the long standing assumption or claim of most behavioralists that there was a basic identity among the philosophical reconstructions of science produced in the literature of logical positivism and empiricism, the practice of natural science, and the demands of scientific inquiry in social and political science, the consciousness of political scientists regarding these matters was being raised to a significant degree.

Another closely related body of literature that appeared in the 1960s, which to some degree overlapped the material discussed above, was a series of works in the philosophy of social science that challenged philosophical and social scientific assumptions about the applicability of scientific methods to the study of social phenomena (Winch, 1958; Natanson, 1963; Schutz, 1967; Appel, 1967; Louch, 1966; MacIntyre, 1962). Much of this work was grounded in post-Wittgensteinian philosophy and in Continental phenomenology, with certain strong affinities with, or parallels to, notions of the explanation of social action in the work of Max Weber. There was, however, a fundamental ambiguity running through much of this material with regard to whether it was challenging the idea of the unity of science, and claiming that the subject matter of the social sciences demanded another or autonomous method of inquiry and notion of explanation, or whether it was claiming that scientific methods of explanation were inappropriate for understanding social phenomena. Although this literature in the philosophy of social science was beginning to enter the discourse of political theory, there were few specific attempts in the 1960s to apply these ideas to issues in *PT* or to relate them to challenges to positivism in the philosophy of science (Gunnell, 1968). By the early 1970s, however, the concern with these arguments and their application would constitute a relatively distinct body of literature in Political Theory.

In the 1968 symposium on the state ("advance") of the discipline, the analysis of Political Theory made a point of the fact that political theory as a field and political theory as an intellectual activity were far from coterminous. The authors claimed that while the activity of theorizing within the discipline had increased, dissatisfaction with the field was on the rise. In their judgment, the field was broad, diverse, fragmented, and replete with paradoxes and perplexities. Despite the fact that most major departments still treated Political Theory as a basic sub-field, it had little to do with the discipline as a whole. While most social scientists viewed theory as "a systematic and self-conscious attempt . . . to explain diverse phenomena that have been or can be observed," theory in political science had traditionally involved much more than this "explanatory" type (McDonald & Rosenau, pp. 317, 318). It had included, and continued to include, various dimensions of political philosophy, or normative theory and ideology, that were inconsistent with the concerns of the "behavioral revolution" and the conception of theory in contemporary political science as a whole. The result was "sustained and unrestrained argumentation" (pp. 320, 321).

In the authors' view, the future of this sub-field, with its current "ungainly structure," might proceed in three possible directions. First, empirical political theory and theorizing might spread throughout the discipline, while the "philosophical" component was retained as a separate sub-field. Second, the behavioral approach to theory might take over the sub-field while Political Philosophy became an additional sub-field. Third, political science as a whole might become an entirely empirical science while the normative and philosophical components would find a place as appendages or elements of other departments.

The authors were relatively optimistic about what they saw as the two principal directions of political theory as an intellectual activity. First, although the many conceptual frameworks and methodological strategies such as systems analysis that had appeared in recent years were, at best, pre-theoretical and of doubtful durability, there were indications that "theory at all levels in all fields will soon experience previously unimaginable breakthroughs because of technological advances" (p. 334). Second, there was a "growing appreciation of the theoretical relevance of the great works of political thought" and, "perhaps as a reaction to the behavioral revolution, the inclination to treat them as historical artifacts, philosophical formulations, and ideological tracts has given way to a growing concern for probing their theoretical content" (p. 337). Although none of these predictions would prove to be precisely accurate and although the hopes for theory might not be realized, the tendency toward dispersion was perceived, and the differentiation between *PT* and *pt* was recognized.

Another development was subtle and incremental; it would be more obvious in discussions of political theory in the 1970s but less consciously recognized as a transformation. It has already been noted that in the beginning of the century, "political theory" was understood more as a functional category, a sub-field name, or a literature classification than a distinct entity or activity. This gradually changed through the 1950s, but it was primarily the debates about theory in the 1960s that gave rise to the contemporary notion that theory is actually an element in the practice of science, and more than an analytical specifiable element; that political theory is a concrete activity with a past and future; and that theorizing is a distinct endeavor. There is a definite sense in which the contemporary notion of political theory was invented in the context of the debates about behavioralism and the decline, and revival, of political theory. The concept became reified to a degree that would have been unintelligible to earlier generations, and the growth of *pt* lent credence to this idea. This can be concretely illustrated.

In the 1937 edition of the *International Encyclopedia of the Social Sciences,* not only was Political Theory not a separate heading, it was not even treated as a distinct sub-division in the discipline of political science. In the 1968 edition, Political Theory was not only given prominence and separate status in the section on Political Science, where it was treated as an activity, product, and sub-field, but it became a separate and equal topic where it was discussed almost as if it were an autonomous discipline with its various divisions, problems, dimensions, and history. Although the views of theory, as presented by Easton in a discussion of theory in "Political Science," and by Wolin and Brecht, in their analysis of "Political Theory," were in many respects quite antithetical, they both exemplified this reification of theory as well as the tension between notions of theory in *PT* and the growing distinction between *PT* and *pt*.

Easton argued that political science, by mid-century, was still "a discipline solving its "identity crisis" and emerging with a "systematic theoretical structure of its own" that was largely the consequence of "the reception and integration of the methods of science into the core of the discipline" (p. 282). The basic thrust of this "theoretical revolution" was a turn toward functional and systems analysis as opposed to a focus on institutions such as the state. According to Easton, the core of the behavioral movement was a "shift from an institutional and practical problem orientation" to pure science, and this shift was most "sharply revealed" in "the sub-field of political theory." The changes were distinctly reflected here, and at the same time the work in theory was crucial in moving the discipline in an analytical direction (p. 243). Easton claimed that behavioralism had dispensed with "the last remnants of the classical heritage in political science." It produced "a profound transformation in conceptions about the role that theory plays as a tool in political research"—a role that was distinguished by a separation of fact and value and an attempt "to drive political science away from a prescriptive problem-directed discipline to one in which research depends increasingly upon empirically oriented theoretical criteria" (p. 296).

Although Wolin took issue with Easton's notion of science and the role that theory played in science, he disagreed less with Easton's description of what theory had come to mean and to be in political science and with the basic "traditional"/"scientific" distinction than with the implications of this development. For Wolin, what was happening in political science was a serious deviation from the historical role of theory and theorizing and its relationship to politics. According to Wolin, "the quest for a scientific theory of politics has altered the character of theorizing in several significant ways" and what it has produced on the whole has been the "sterilization of political theory" (pp. 325, 328). In an important sense, theory for both sides was something even bigger than political science and politics but something that might be manifest or realized in either.

Those who valued the study of the history of political theory were seeking to articulate exactly what it was all about in order to defend it against the behavioral challenge. George Kateb (1968), for example, attempted to specify the principal characteristics of "traditional political theory" and to justify its uses in an age in which it had "fallen on hard times." For the behavioralist, the realization of theory was in the near future in political science, and for the historian the future of theory depended on the recovery of its past. Claims such as those of Easton and Wolin capture much of the flavor of the debates about theory that characterized the 1960s, but the last year of the decade indicated some new directions that would permeate discourse in and about political theory during the 1970s.

First of all, in 1969 Easton announced, or called for, "a new revolution in political science." This was to be a "post-behavioral revolution" which was not "theoretical" or a change in the methods of inquiry but a change in orientation that grew out of a "deep discontent with the direction of contemporary political research" and which advocated, at least in the short run, more attention to the public responsibilities of the discipline and to relevant research on contempoary political problems and issues (p. 1052). The shift in distribution of emphasis from Easton's stress on the priority of "pure science" and the separation of fact and value in the "behavioral credo" (1965, p. 17) as well as his statements about political science and political theory the previous year was, at least on its face, dramatic. This, in some ways, was the official birth announcement of the public policy enterprise which would become the basis of the self-image of orthodox political science in the 1970s, and in terms of which it would attempt to establish its identity. With this shift would come a distinct de-emphasis of the concern with general unified theory as the core of the discipline as well as a retreat from any pointed confrontation with the history of political theory.

In the same issue of *APSR* in which Easton announced the new revolution, Wolin presented his defense of "political theory as a vocation." This was in a very real sense the quintessential statement, as well as, in many respects, the effective terminus of the critique of behavioralism from the standpoint of the history of political theory. Although Wolin's position might not spring from the same intellectual and ideological source as that of Strauss and others, he spoke for a generation of political theorists when he defended the role of the historian of political theory as conservator and transmitter of "political wisdom" against the "methodism" of political science and its "historyless" posture that neglected the "tacit political knowledge" that should inform both scientific and political judgment. He spoke also for the preservation of that "vocation by which political theories are created" in the midst of a world that is governed by "giant, routinized structures" and which is "impervious to theory" and in which political science is marked by "complacency" in the face of political crisis and chaos (pp. 1070-1071; 1077; 1080-1081). Although Wolin's arguments articulated the sentiments of many political theorists, his statement was shouted in a desert, and it was clear that he as well as other political theorists would have to seek a forum outside political science. Behavioralism, which in the immediate sense at least, had begun the war with the history of political theory, was disengaging. On the other hand, there was a distinct sense in Wolin's essay that the history of political theory no longer had a home in political science.

Although it would be several years before developments had progressed sufficiently for a discussion of the "new history of political theory" as a positive enterprise, Quentin Skinner—who would be prominently associated with this position in the 1970s—offered a comprehensive and influential critique of past research in the history of Political Theory in 1969. It was an indication of the anomalous situation of much of this literature, caught between political science and *pt*, that Skinner's methodological critique, in terms of understanding in the history of ideas, tended to neglect the philosophical, political, and disciplinary contexts in which many of the arguments such as those of Strauss developed. But the arguments of individuals such as Skinner were not simply critical but legislative. In the 1970s much of the literature dealing with the history of political theory would revolve around matters of the advocacy and criticism of the "new history". The shift was decidedly outside the context of political science in both a methodological and substantive sense.

Finally, by the end of the 1960s, there were indications of a distinct transformation in the theoretical or philosophical critique of political science. It is easy to forget how little had been published in criticism of the behavioral image of science by the late 1960s that went beyond general concerns about treating politics scientifically and practicing science politically. Although there were growing concerns about the integrity and validity of the behavioral image of science, the critique of behavioralism had in many respects been an external one that was relatively easy to defuse by drawing lines between those who were committed to science and those committed to some other approach to the study of politics. In the same issue of the *APSR* in which Wolin and Easton's articles appeared, there was a critique of the philosophical assumptions of the basic model of scientific explanation that had been adopted by political scientists and of the relationship between philosophical claims about science and the practice of social scientific inquiry (Gunnell, 1969). This critique was published as part of a symposium that included two editorially solicited rebuttals.

There had been a great educational, financial, intellectual, and emotional investment in the behavioral image of science and theory, and despite all the talk about policy, that image was at the core of disciplinary practice. The battle over this issue was, in many respects, yet to come. The question of the philosophical status of this image of science and related ideas would occupy a central place in the literature of political theory (both *PT* and *pt*) during the next decade and would, as a matter of fact, contribute significantly to the dispersion of political theory.

THE DIASPORA OF POLITICAL THEORY: 1970-1979

In the 1970s, there was the disappearance of an issue center within *PT*, and the sub-field was progressively transformed into *pt* writ small as the autonomy of the latter became more firmly established. Journals such as *Philosophy and Public Affairs* (1971) and *Political Theory* (1973) had begun publication. Political theory was no longer centered in the discipline of political science, and political science no longer defined the issues in the literature of Political Theory. New influences, such as the translation of the work of Habermas, came onto the scene the same year as the publication of

Rawls' book; commentaries on, and tributaries of, this material occupied much of the space in Political Theory during the decade. Throughout the late 1960s, graduate education in Political Theory had continued to be traditional with an emphasis on the history of political theory. Although there was some increased attention given to scope and method courses, and although a few thinkers such as Camus, Sartre, Arendt or Marcuse were featured fairly prominently in some limited contexts, few graduate students were exposed, in any systematic or extensive manner, to much more than the traditional canon. This was true even at institutions where theory was prominent. But during the 1970s, graduate education changed significantly in response to developments in *pt*. Names such as Rawls, Nozick, Habermas, Heidegger, Foucault, Gadamer, Lukacs, Gramsci, Althusser, Kuch, and Popper, became common currency, and students were asked to come to grips with such exotic fields such as sociobiology, hermeneutics, and structuralism. It is easy to forget just how radical the transformation was that began in the 1970s in both education and the scholarly literature. Assessing its significance is a contentious matter.

Many would see the developments of the 1970s as growth in the independence of political theory, and as a break from its less than happy home in political science. But although *pt* became a recognizable body of literature, and even a partially institutionalized interdisciplinary field with journals and organizations that reflected its concerns, it was largely a collection of intellectual enclaves with limited communication between them. However one views the implications of the events of the 1970s, the result was the dispersion of political theory both between and within *PT* and *pt*. To disentangle the various themes and threads is not an easy task, but a natural starting point is an examination of the remnant of political theory that was distinctly attached to the discipline of political science.

It was clear from the beginning of the decade that, in practice at least, the discipline's emphasis on universal theory was waning. The disjoining of the debate between historians and scientists led to a more pluralistic view of theory; behavioralism was quite willing to be tolerant of diversity once it had captured the center; the "new revolution" also placed less emphasis on theory; diversity and tolerance were forced on the discipline by the critics in the late 1960s; and specialization in research was leading to more emphasis on intellectually localized research strategies. By the early 1970s, it was not easy to find coherence among the trends or to bring the discipline together into a common notion of theoretical endeavor.

In attempting to understand this situation, Karl Deutsch's 1970 presidential address to the APSA is instructive as an attempt to confront the problem (1971). Several things were quite clear in Deutsch's eclectic view of theory. First, it was a very sharp departure from the conscious attempt by himself and others in the mid-1960s to define theory in a narrow manner and to make a sharp distinction between empirical political theory and political philosophy. Second, there was a good deal of at least implicit reproof as well as conciliation directed toward both academic and political critics of the discipline. And third, there was a distinct call for political relevance which echoed the statement of Easton. Deutsch was determined to find a middle ground between scientism and political radicalism, between theory as an instrument for knowing reality and as ideology, and to present various analytically distinguishable aspects of theory as "an integrative process" involving "stages in a single production cycle of political knowledge and political action" (p. 17). All this required many different types of people and diversity in distribution of emphasis. In the end, Deutsch wanted to give all sides a place while to some degree maintaining the priority of the behavioral vision of theory, but as with most attempts at intellectual synthesis after the fact of differentiation, it did little to change the situation and involved such an amorphous notion of theory that the very thing that was to be identified became still more elusive.

The resonant note in Deutsch's essay was the idea of political science as a policy science; this would also be sounded in Heinz Eulau's analysis of the "Skill Revolution and the Consultative Commonwealth" (1973), Avery Leiserson's call (1975) for a synthesis of science and politics and a reaffirmation of the traditional American faith in the complementarity of "scientific truth and democratic decision making," and Austin Ranney's (1976) celebration of the vision of a "divine science" that had traditionally informed political science in the United States from the time of the Founders and that could provide a basis for the contemporary need to recognize the possibility of "political engineering in American culture." This was the tone of the textbooks and other less official statements about the purpose and direction of the discipline during the 1970s, and although "post-behavioralism" in political science was a concept that meant many things (Graham, Ed., 1972), it certainly included the policy turn in the mainstream of the discipline. But it also represented the ideological critique of the field as it was in the late 1960s and the philosophical critique of the behavioral image of science. "Post-behavioralism" was more a condition or a name for less than coherent congeries of interests than an intelligible intellectual movement or even mood.

The old controversy about political science as a science was, by the early 1970s, largely focused on the critique and defense of the behavioral image of science in terms of the philosophy of science and philosophy of social science. A second symposium in the *APSR,* in which a battery of political scientists and philosophers was enlisted by the editor to exorcize dissident philosophical claims about scientific inquiry, appeared in 1972. In this case, the article (Miller) at the center of the symposium was, ironically, far from fully supportive of, or concerned with, the new philosophy of science represented by individuals such as Kuhn. It was written from a Straussian perspective and focused on matters such as the historicism implied in Kuhn's work as well as the positivism of behavioralism. The critiques, which indicated where the primary sensitivities of the discipline resided at this point, fastened, however, on the alien images in the philosophy of science.

By the mid-1970s, there were philosophically sophisticated critiques of behavioral assumptions about political theory (Spragens, 1973); books defending the behavioral image of science in terms of both the basic logical empiricist philosophy of science and the responses of this school to criticisms mounted by individuals such as Kuhn and Feyerabend (Gregor, 1971); and books which aimed at demonstrating, in light of the contemporary work in philosophy, both the dubious character of logical positivism and logical empiricism and their impact on the conduct and understanding of inquiry in political science (Gunnell, 1975). This kind of controversy was inevitable, but it had the effect of drawing political theory into a realm of metatheoretical debates that in many respects had decreasing relevance to both politics and political inquiry. Somewhat the same problem is apparent in the literature that attempted to apply new ideas in the philosophy of social science (Dallmayr & McCarthy, Eds., 1977) to a critique of positivism in social and political science and to a reconstruction of social scientific inquiry. Such synthetic accounts drawn from linguistic analysis, phenomenology, and critical and interpretative theory (e.g., Bernstein, 1976) were largely summaries and restatements of material in philosophy, and, while useful and interesting in many respects, this material transformed political theory into a less than autonomous mode of discourse.

Compared to the 1960s, with the fixation on theory as conceptual frameworks, it is difficult to discern exactly what orthodox political scientists understood theory to be. The 1973 Supplement to the Biographical Directory for the APSA separated out Methodology (which included epistemology and the philosophy of science) and divided Political Theory into systems of ideas in history, ideology systems, political philosophy (general), and methodological and analytical systems. The (unofficial) *Handbook of Political Science* published in 1975, devoted Volume One to the *Scope and Theory of Political Science* which included chapters on: the history of the discipline; an analysis and synthesis of opposing philosophical views about the nature of social scientific explanation; an essay on the contemporary relevance of the classics; a neo-positivist philosophical analysis of the language of political inquiry and political concepts; and a philosophical discussion of the language of political evaluation. Volumes Two and Three were devoted respectively to what was designated as Micro- and Macro-political Theory, but these were largely categories for encompassing various subjects and research programs rather than specifications of particular theoretical formulations (Greenstein & Polsby, Eds.).

The official panels at the APSA meetings obviously had increasing difficulty deciding exactly what the structure of political theory was or should be. Constantly changing configurations attempted both to capture the diversity within the field and to reflect topical issues. "Formal" or "positive" theory emerged as a distinct category in 1970, and diverse remaining panels were listed under "Philosophical Analysis and Politics." In 1971, the panels were divided between Formal Theory, Ethical Theory, and the Philosophical Analysis of the Science of Politics. By 1972, the attempt to make any general divisions in the sub-field was abandoned in favor of a potpourri including various substantive, conceptual, and methodological issues. In 1973, there was an attempt—without much apparent success since it included panels as diverse as Statistical Problems and Research on Electoral College Reform—to carve out an area designated as Methodological and Analytical Theory. The 1974 panels were divided between Macro Theory and Micro Analysis on the one hand and Political Theory and Ideological Conflict on the other hand with a large number of issues (post-behavioralism, epistemological alternatives to behavioralism, etc.) relegated to the unaffiliated panels. An attempt was made in 1975 and 1976 to hold on to a basic division between Political Theory and Epistemology and Methodology, but it gave way in 1977 to Political Theory, Methods and Empirical Theory, and American Political Thought.

From 1978 through 1983 there has been some basic division between Analytical (and/or Empirical) Theory and Political Philosophy (and/or Political Theory or Thought) with the continued representation of many issues in a wide variety of unaffiliated panels. But the criteria of demarcation has not been very clear or consistent. By 1982, the APSA Directory and the Guide to Graduate Study in Political Science divided the field into Political Theory and Philosophy, Formal or Positive Theory, and Methodology, but these categories are not particularly descriptive of the field or the ten percent of the members of the APSA who designate themselves as primarily political theorists.

Although a general neo-positivist or logical empiricist view of science, scientific explanation, and theory continued to be quite pervasive in the discipline at large, it usually found expression, in pro forma terms, in textbook introductions and methodological prefaces. There were few criteria for connecting this persistent philosophical image of science with what political scientists actually did. The criticism and defense of this image did not peak until the mid-1970s; thereafter, even some of those who had done the most to propagate it began to disavow it. The irony was, however, that this image, in the face of heavy criticism, had only recently received its most thorough explication and defense. At the very point at which many dedicated younger centrist scholars in political science found and accepted it at the core of their education, the older generation began to deny it.

In 1977, Almond, for example, was ready to concede—or "discover," since he did not acknowledge or mention any of the criticism that had been mounted within the discipline during the past few years—most of the points that had been advanced against the philosophical foundations of behavioralism. He suggested that the commitment to these methodological doctrines had caused political science to lose touch with its "ontological base" and mistakenly attempt, with the encouragement of neo-positivist philosophers of science and their partisans, to treat political phenomena as natural events when they should instead have developed approaches "appropriate to human and social reality" (p. 522). He argued that such constructs as the deductive model advanced by logical empiricists as well as the general "explanatory strategy of hard science had only a limited application to the social sciences" and that the "search for regularities and lawful relationships" and the "enshrining of the notion of generalization" represented the wrong direction for the discipline to have taken (p. 493).

Few orthodox political scientists so openly rejected the essence of the behavioral faith, but few attempted to defend it. Some, such as William Riker, continued to describe and justify their work and define theory in terms of notions of the unity of science based on "positivist or objectivist philosophy," as set forth by individuals such as Hempel and Nagel, and to pass off the criticism that by this time had come close to a new orthodoxy in the philosophy of science as "idealist interpretations of science" (1977, p. 16). But such attempts to restrict the idea of theory in this manner were not common.

For both the critics and defenders of behavioralism, the deep but inevitable involvement of political theory in the issues of the philosophy of science and epistemology had become a problem. Theorists were being drawn off into a realm of metatheoretical concerns that alienated them from substantive political issues and a concern with political phenomenon in general (Kress, 1979). Heinz Eulau noted that the cycle of justification, criticism, and defense of behavioral claims in philosophical terms had produced an "exaggerated, almost pathological, concern" that culminated in "the curious notion that the philosophy of science is the high road to scientific knowledge, as if the business of the philosophy of science were science rather than philosophy" (1977, p. 6). This precise point had been made by critics of behavioralism over a decade earlier. Although one might not agree with the particular biopolitical concerns of John Wahlke, his critique of behavioralism (1979) also indicated a certain basic agreement with many of the critics who viewed behavioralism as an empty commitment to methodism. Wahlke argued that despite all the talk about theory and despite the technical methodological advances, political science was still characterized by a "paucity of theoretical concerns."

By the end of the 1970s what had happened to the history of political theory? It was alive and living at least a large portion of its life in *pt*. Although some reverberations from the active discussions in *pt* might surface in the literature of political science, most of the controversy that was generated was not felt in the discipline in general. There was no question that the history of political theory was accepted as part of the field of political science, but it was not at the center of concerns about theory in the discipline. Its new found autonomy guaranteed its existence, but it was increasingly divorced from the issues that had dominated the field and the claims of both its advocates and critics for so many years. Autonomy meant a new self-consciousness which issued in the call for a "new history" (e.g., Skinner, 1979; Pocock, 1971) that would reflect "truly" historical methods and recover the actual meaning of classic texts as well as trace "actual" historical traditions. In the view of the new historians, this required turning away from the philosophical concerns and assumptions which they believed had governed past scholarship. While a considerable body of literature voicing and reflecting this position emerged during this period and added significantly to the understanding of classic authors such as Locke (e.g., Ashcraft, 1980; Tarlton, 1979), the purpose of this activity, its relationship to political science, and a range of similar issues were not only unresolved but seldom directly discussed.

To adequately convey the structure of political theory, whether in *PT* or *pt*, during the 1970s and into the early 1980s would require listing many different and discrete enterprises. Different interests and concerns would yield different distributions of emphasis. The important point is that the field was becoming too dispersed for there to be any core set of issues that would be readily agreed upon as establishing its identity.

Significant work in conceptual analysis was apparent (e.g., Pitkin, 1972, 1978; Flathman, 1972, 1976), and analytical political philosophy was well represented in works such as the continuing series of *Philosophy, Politics and Society*. Although the Straussian project remained active after the death of Leo Strauss, it was dispersed within itself and increasingly insulated from the discipline of political science. There was a legacy of arguments about scientific method and the nature of explanation, but these reflected continually finer drawn points in the philosophy of science and philosophy of social science. The large body of literature that surrounded the work of Rawls continued to grow as well as other quasi-transcendental arguments in normative theory (e.g., Dworkin, 1977). One of the trends that characterized the 1970s was a growing concern with political economy and renewed attention to the "state" as an object of inquiry. Much of this material was not in political science proper (O'Connor, 1973; Wolfe, 1977; Skocpol, 1979), but it influenced the discipline (Lindblom, 1977). There were continuing attempts to bring the world of Continental philosophy closer to the concerns of American political theorists. The problem of summarizing Habermas and keeping up with his dicta was supplemented by problems of understanding individuals such as Foucault, Gadamer, and Althusser. The world seemed accessible only through what somebody else said about it, and authors more than their subjects became the object of analysis.

This somewhat impressionistic account could be expanded considerably and still not adequately encompass the varieties of political theory. The problem is less one of inclusiveness than of determining how to assess the state of the field. The proliferation signified for many that there was vital energy in political theory, but it was difficult to identify any basic coherence. One might, at a certain level of abstraction, find underlying connections or similarities in the literature, such as a concern with language and interpretation or the problem of achieving some transcendental ground for political values. But such characterizations tell us less about the field per se than about what someone might find interesting. Although one could say that the field was vital, it could just as easily be said that it was without definite direction or focus.

PROSPECTS: THE 1980s

Since it is only in retrospect that we can know exactly where we were, what I have termed the dispersion of political theory may, in future years, appear to be something quite different. But an external vantage point—temporal or spatial—often imposes a restricted perspective. Even now it is interesting to note how the situation in political theory is described by an outside observer. The results of one such investigation, while not intuitively counterfactual, would probably be unsatisfactory to most participants in the field. A report on the state of political theory by the American Association for the Advancement of the Humanities described it as a "discipline" that had "died and been reborn several times." The conclusion was that in its present "incarnation" it consisted principally of a debate between Straussians, analytical philosophers, and contextualist historians and that these debates, marked by "intellectual irreconcilability," structured both academic and professional life (Herbert, 1981, p. 8). A much more extensive study of political theory in the United States has been in progress in Germany by the late Peter Lutz and now by the executors of his academic estate, but the difficulty facing such anthropologists of political theory is in part a matter of where they happen to cut into the culture and the categories that they bring to their analysis. They also probably fail to grasp the complexity of the society with which they are dealing. One might not want to say that their results are wrong, but their conclusions are too partial and narrowly focused. The principal difficulty of all these analyses is that they are seeking the identity of political theory.

The dispersion of political theory that took place in the 1970s has resulted in a *loss* of identity (both in *PT* and *pt*). The idea of political theory advanced by behavioralists, the counter arguments of historians of political theory, and the controversy in which they were joined may all be rightfully interred. But these arguments gave a shape to the field that is lacking today. The loss may not be at all lamentable, and there is no obvious reason why a discussion of prospects should assume a need to reconstitute such a form. But it is necessary to ask questions about the elements of the dispersed residue of that form and the relationship between them as well as their relationship to politics.

Although there are some individuals in political theory who believe that the field would be best served by finalizing the divorce from mainstream political science, such views must assume that there is a space for the activity of political theory outside political science. Often, however, such ideas are based upon abstract world-historical views of political theory when in fact the very idea of political theory has been largely a product of the evolution of political science. Its future outside of the discipline may be something of an illusion. There is, for example, little reason to think that *pt* is likely to become anything more than the loose collection of endeavors that now comprise it. But the question of whether there is a life for political theory in political science cannot be avoided.

Political theorists are often not only alienated from the concerns of most research programs in political science, but not even engaged in a critical assessment of these programs. Their principal reference point is the world of *pt*. On the other hand, within political science, there are the views of those who seek to narrow the meaning of political theory not only to orthodox political science but to a particular position within the discipline. For example, William Riker (1983) claims that "the main line of development during the last generation" has been in the direction of social choice theory, and he defines theory in terms of the logical empiricist deductive nomological model which he in turn equates with the practice of natural science. These positions have the unfortunate effect of artificially limiting critical and interesting discussion in or about political theory.

It is probably too optimistic to believe, however, that contemporary trends reflect an end to the conflict between "humanists" and the "science establishment" and signal a "paradigm synthesis" (Bluhm, 1982, p. 3). Nor is it likely that an answer lies in a synthesis of philosophical perspectives about the nature of social scientific explanation (Moon, 1975; 1982). Although it would be nice to believe that the problem is basically one of "communication among all scholars committed to understanding and explaining political phenomena" (Graham, 1982, p. 206), the problems run deep, are substantive as well as methodological in nature, and have to do with the very idea of social science and the relationship between social science and politics.

It is probably true, as the advocates of synthesis believe, that there are "no *necessary* barriers that currently prevent discourse across the methodologies and paradigms of political science" (Graham, p. 222). We are not in the situation of the mid-1960s when there were severe obstacles to discussion both within political science and between political science and other disciplines such as economics and philosophy. Clearly there are, in both theory and practice, synthetic exercises that have brought together philosophy, economics, and political science in areas as diverse as public policy analysis and critical theory. And the general ability of political scientists to move between these areas is much more apparent than ever before. Furthermore, the general mood of the early 1980s is not along the lines of the focused intransigent confrontational discourse that marked the 1960s and 1970s. The problems may be more those of pure tolerance.

Brian Barry, whose own work is part of what many believe to be the renaissance in political philosophy, sees, in comparison with the lack of political philosophy in the early 1960s, a positive movement. But he also admits to an occasional "nightmarish feeling that 'the literature' has taken off on an independent life and now carries on like the broomstick bewitched by the sorcerer's apprentice" (1980, p. 283). The question is not whether there is a great deal of political theory and philosophy being written but whether we have a wealth or a "glut" (1980, p. 284) and whether there is any vital dialogue. Many believe that there is not only a need for more "organizational boundary crossing" between political science and political philosophy (Barry, 1977, p. 299) but "a need to improve the quantity and quality of discussion across the different schools that have gradually established enclaves in political theory" (Nelson, 1983, p. 24).

One prediction that seems totally safe is that *PT* and *pt* will never again be one, and it is unlikely that either will become a more concentrated field. The latter is an interdisciplinary body of literature that shows no signs of consolidation, and most of the sentiment seems clearly in favor of pluralism. Everybody on record is hoping for mutual interaction and cross fertilization, but few seem to care if it is actually effected. Since much of *PT* is now a reflection of the structure, or lack of structure, in *pt*, a more central focus there seems just as unlikely. If we look within *PT* for a core of theory directed toward the empirical study of politics in the mode of the behavioral vision, it seems to be very close to disappearing altogether. But there are some who still hold out hope for a unified center.

Most commentators recognize that in one way or another post-behavioralism also means post-positivism. The question is whether there is a life for scientific theory "beyond positivism." Elinor Ostrom suggests that "we are coming to the end of an era in political science, a slow, whimpering end" which indicates that "the hoped for cumulation of knowledge into a coherent body of theory has not occurred" (pp. 11, 13). Individuals such as Ostrom, however, continue to stress "the need for the development of theory as the basis of our discipline." This, however, would be a more eclectic notion of theory that would transcend both "the naive acceptance" of positivism and some of the narrow approaches to empirical analysis that it was used to justify and which are relevant to modern political issues. For Ostrom, as for a number of others who have celebrated the policy turn in contemporary political science, the demands of both science and relevance seem to be met best in the application of economic models involving rational choice and collective choice and the metatheoretical explication of such models. But, again, there is little clear evidence at this point that a unifying vision of political theory will or should emerge from this material (Moon, 1983).

Unity is still the message of most presidential addresses. Warren Miller predicts unification based on the research methods developed since World War II and claims that only problems of inadequate personnel and funding now stand in the way of bringing "political science into a new age of intellectual ferment and maturity as a discipline" which can at once "make a massive contribution to the welfare of the nation while evolving into a conceptually coherent scholarly enterprise" (1981, pp. 14, 15). A basic fact of intellectual continuity in the field is the belief that we belong to an unfinished discipline in which an imminent theoretical breakthrough will not only verify the discipline as a science but demonstrate its relevance to public policy. The question is *exactly* what is to be the form of interaction between public and academic discourse and the vehicle of that interaction. We can see what Merriam, Wilson, Beard, Bentley, Lasswell and others gave for answers to these questions, and we might well ask what, precisely, is the answer of the modern political scientist?

A survey of the presidential addresses to the APSA, as well as various other intellectual signals in the literature, seems to suggest a liaison between political scientists and policy elites that would take place through consultation, the movement of political scientists in and out of the actual policy process, and the influence of journals such as the *Public Interest*. Although the role is not perceived as necessarily uncritical, the principal thrust is more in the direction of service than reform, let alone anything resembling a more basic critique of political institutions. The mood, for the most part, is conservative.

There are, however, some other directions in public policy analysis and the relationship between political science and politics. First of all, it is not merely in Critical Theory that a vision of a critical social science is present (Connolly, 1981). There are even some obvious shifts within what might be taken as the political science establishment. Charles Lindblom, in his 1981 Presidential Address to the APSA, recognized and endorsed the claims of the dissenting academy. He claimed that the discipline as a whole and most of the major figures in the behavioral movement had largely accepted and reinforced the initial premises, research programs, and methods of "a complacent view of the liberal democratic process, government, and state" (1982, p. 9). Even many erstwhile critics, he suggested, are actually "committed to the conventional view and give little sustained analytical attention to the radical model" which is manifest in contemporary neo-Marxist thought and other elements of contemporary political theory such as "phenomenology, hermeneutics, interpretative theory, and critical theory" (pp. 12, 20).

If the history of political science provides any clue, the argument will be noted, but the direction of the discipline will not be fundamentally altered. Whatever the impact of critical notions of political theory and political science on the discipline, the very fact that they remain in the discipline will probably ensure that such discourse remains ultimately academic. Through the medium of education and osmosis, it may have some practical effect but not the form of engagement often implied. There are some things happening in the world of political theory that obviously suggest a more definite idea about how to make political theory political or at least make contact between political theory and politics. A venture such as that represented by Sheldon Wolin's involvement with the journal *Democracy* deserves consideration.

There is an assumption that political theory must, or should, reach a wider public audience and that there must be a mediating mode of discourse. Exactly what this means, or can mean, is more difficult to say. It does not seem that the basic purpose is to lead or support what might be termed movement politics. Neither is it principally an attempt to enter directly the activity of what might be termed conventional politics. It might be more accurately understood as an attempt to evoke in a wide range of educated individuals, in a variety of contexts, a sense of public consciousness and virtue that transcends the mundanely political but which is not confined to the academic world. Yet the project remains bound within the ambiguities of the relationship between political and academic discourse.

The extent to which practical concerns, and the attempt to make such concerns effective, will have an impact on the discipline of political science and the direction of political theory, and, in turn on politics, is a definite question for the 1980s. Archaeological analysis tends to produce skepticism, since it demonstrates the inevitability of mortality and the demise of the present. Digging into the past of American political science is no exception. Furthermore, the history of the sub-field of Political Theory does not demonstrate any great ability to transcend American political culture. As Norman Jacobson has noted, the idea of a social science as the embodiment of a provocative vision has not been indigenous to American political science. Today political theory is bringing many new ideas into the discipline, but a study of the past indicates "that the genius of American social and political science has been less the cultivation of novel ideas than their domesticator" (1982, p. 6). This has been the case in the sub-field of Political Theory.

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2
Political Theory and the Art of Heresthetics

William H. Riker

My assignment for this essay is to review the present state of the field of political theory. This I will do by examining, first, the main line of its development during the last generation. Then I will look to the future, not only at the continuation of this main line of scholarship, but also at some possibly fruitful new directions, as, for example, the art of heresthetics.

It is necessary in the beginning, because of a usage peculiar to political science, to specify exactly what political theory is. In most scientific disciplines, the word "theory" refers to a set of deductively related sentences that together describe the portion of the world studied with a particular discipline. Etymologically this is correct; "theory" weds observation and contemplation. In political science, however, the word "theory" has come to refer primarily to moral philosophy. While I recognize the necessity and importance of moral philosophy for political studies, in this paper I intend to use the word "theory" as it is used in other sciences. Given our tradition, this is rather difficult because, until recently, there has been no set of sentences about politics to which "theory" could be applied with etymological justification. Beginning, however, with the publication of Duncan Black's essay, "On the Rationale of Group Decision Making" (*Journal of Political Economy*, 1948, although the main ideas were, as noted in Coase, 1981, worked out six years earlier), there has existed a deductive and testable theory about political events. Political theory developed gradually through the next two decades, and during the 1970s it came of age. It was the subject of books, essays, conferences, and convention panels, and now even a summary essay in a book on the state of the art.

I

The content of political theory is the authoritative allocation of values, following Easton's (1954) definition of politics, in that it involves the amalgamation of individual preferences into a social choice and the subsequent enforcement of the result. At this quite general level, the goal of political theory is to identify the conditions for an equilibrium of preferences. Such an equilibrium is a social choice that the members of every sub-group in the society that are capable of bringing about a social decision prefer to any other alternative. This equilibrium is one the society will arrive at for certain, regardless of its particular institutions; and, if by reason of some obstruction the society is deflected from it or forced to abandon it, the society nevertheless will return to it if the obstruction is removed. Equilibrium of this sort is therefore a generally accepted outcome which is, moreover, self-enforcing because of the desire of individuals to arrive at it.

The goal of identifying the conditions for equilibrium is closely parallel to the goals of other scientific disciplines, both social and physical, in which the specification of conditions for equilibria is a central theme. The history of political theory over the last forty years has consisted largely of increasingly precise specification and increasingly deeper analysis of equilibrium conditions.[1]

As previously noted, Duncan Black (1948, 1958) began the construction of theory by looking for an equilibrium of majority rule. He soon rediscovered the paradox of voting (initially discovered by Condorcet, 1785, and then subsequently overlooked), the significance of which is that, when it occurs in a set of individual preference orders, it guarantees that there will be no majority winner (i.e., disequilibrium) within that set. Looking for a way around the paradox, Black devised the condition of the single-peakedness of a set of individual preference orders as a sufficient—but not necessary—condition for the avoidance of the paradox and thus for the guarantee of an equilibrium. This was an excellent beginning. Among its other virtues, the condition of single-peakedness is highly practical because it allows for ready specification of the substantive content of an equilibrium outcome as the median voter's most preferred alternative.[2] Since it is often easy to identify the median voter, it is also easy in many analyses of public policy and political conflict to spell out the concrete details of the equilibrium social choice. As might be expected, this is the feature of Black's work most widely used in practical political and economic studies.

For the development of political theory, however, a more significant virtue of the condition of single-peakedness is that it has an obvious political interpretation. It reveals that an equilibrium among disputing voters rests on an underlying agreement among them about the dimension of political conflict. The fact that equilibrium rests, in this case, on some kind of partial agreement has led to the specification of a variety of other conditions of equilibrium, all based on some kind of similarity of preferences, for example, value restrictedness, extremal restriction, limited agreement, separability, etc. (See Fishburn, 1970, for a method of classifying and generating all possible conditions for equilibrium.)

A third and, for theory-building, perhaps most valuable feature of the condition of single-peakedness is that, once constructed out of the arrangement of preferences on a dimension of judgment, it may appropriately be used to inquire into the fragility of the condition. This Black and Newing (1951) did by analyzing the effect of introducing a second standard of judgment into a committee of three voters. In this case, they managed to find some special equilibria by imposing particular agendas (e.g., sequential voting on first one dimension, then another, then back to the first, *et seq.*). But, in the general case, the similarity of tastes required for equilibrium almost entirely disappeared, even with as few as three voters. The net effect of Black's work was, therefore, to identify conditions for equilibria in special cases, such as single-peakedness, while at the same time generating considerable doubt about the possibility of discovering a general equilibrium.

Arrow (1951) confirmed these doubts by demonstrating the possibility of outcomes analogous to the paradox of voting in any (unspecified) method of amalgamating individual preference. He proved that any such method, satisfying—as does majority rule—some elementary conditions of fairness, suffers from the same kind of defect as majority rule, namely the admission of some cases in which individually transitive preference orders amalgamate into an intransitive social outcome. Many writers have modified or explicated Arrow's theorem in order to soften its impact—for a survey, see Riker (1982a, pp. 123-136)—but his main point remains valid: There is an inherent conflict between voter-dominated procedures and logically consistent outcomes. For political applications, where it is necessary to choose a unique winner, it may be that consistency does not greatly matter. But as Arrow (1963) himself recognized and as Plott (1973) emphasized, failures of consistency mean that the outcome is not independent of the procedure by which it is reached. Hence random variations in procedure imply random variations in outcomes, which is just exactly what is meant by a disequilibrium of tastes. Hence Arrow's theorem may be regarded as a generalization—to all methods of amalgamating tastes—of the fundamental disequilibrium that Black perceived in majority rule. The various elaborations, refinements, and modifications of Arrow's theorem serve to emphasize the ineradicability of the potential for disequilibrium; Gibbard's (1968) theorem—that all methods of voting to amalgamate tastes or values can be manipulated by sophisticated voting—reveals one concrete way in which the disequilibrium may be effected.

The fact that the potential for disequilibrium is ineradicable implies nothing, however, about the frequency of disequilibrium. Though ineradicable, it might—or might not—be so rare as to be insignificant. One line of investigation on this theme is to calculate the expected frequency of nonequilibrium outcomes, given that voters pick preference orders at random and equiprobably (Niemi & Weisberg, 1968; Garman & Kamien, 1968; DeMeyer & Plott, 1970; Gehrlein & Fishburn, 1976). The conclusion is that, as the numbers of alternatives and participants increase, the expectation of disequilibrium very quickly approaches infinity, although just a bit of prior agreement on values lowers the expectation of disequilibrium remarkably (Niemi, 1969; Fishburn, 1973b). The usefulness of this kind of inquiry is severely limited, however, because the assumption of equiprobability or of any particular degree of agreement in picking preference orders is arbitrary and unrealistic, especially when it may be worthwhile for participants to manipulate the distribution of preference orders.

A more fruitful line of investigation about the frequency of disequilibrium is to establish the conditions for equilibrium under a variety of assumptions. In one sense this is an examination of the robustness of conditions like single-peakedness, which seem initially to guarantee a considerable amount of equilibrium in one dimension of choice. However, Kramer (1973) examined these conditions for cases of more than one dimension, when voters' tastes can be represented by quasi-concave differentiable utility functions, and showed that, with even a modest amount of heterogeneity of tastes, majority rule breaks down completely. Kramer's work in effect demolished various special guarantees of equilibrium.

Another possibility is to define what is required for equilibrium. This direction of inquiry has led to an even more dramatic conclusion: equilibrium is nearly impossible. Black and Newing (1951) began the inquiry, as already noted above, by studying the condition for majority rule when three voters used two standards of judgment. They showed that even in quite constrained circumstances a sufficient condition for equilibrium was difficult to satisfy. Plott (1967) elaborated on this result by defining a sufficient condition for an equilibrium of majority rule when $m(\geqslant 3)$ voters use $n(\geqslant 2)$ standards of judgment to select among continuous alternatives. Plott's condition (see also Cohen, 1979) turns out to be so restrictive that even as few as five voters and two standards render disequilibrium almost certain. Schofield (1978b), taking a superficially different tack, defined a condition for an alternative to be undefeatable by any other alternative in its neighborhood. This condition is also extraordinarily restrictive, revealing that equilibrium is highly unlikely, unless tastes are extremely similar, and, indeed, is impossible when $n \geqslant \frac{m+3}{2}$.

The coup de grace for the expectation of equilibrium was supplied by McKelvey (1976) when he looked into the possibility of a small set of alternatives which together might be in equilibrium. The motivation for this research was the idea that, in a case with no majority-preferred alternative (i.e., disequilibrium), a small set—call it T—of similar alternatives might, even though themselves in a cycle, each beat all alternatives not in T. If so, there would be a kind of equilibrium outcome, not unique, of course, but nevertheless stable and predictable in the sense that it would always be a member of the set T, the "top cycle." McKelvey showed, however, that, given continuous alternatives and at least two standards of judgment, the set T is either a Plott equilibrium (which is very rare) or *all* points in the space. This means that any possible outcome is a feasible outcome, which is complete disequilibrium.[3]

This is where the matter now stands. (See Fiorina & Shepsle, 1982; Aldrich & Rohde, 1982; and Schofield, 1982b.) In the abstract case, with no institution assumed and the fewest possible restrictions on individuals' tastes or values, there is no reason to expect equilibrium. The generality and significance of this conclusion is sometimes misunderstood because it is believed to apply only to situations in which outcomes are chosen by voting. But on the contrary, the application is universal. If there is a majority preferred social policy, then, however it may be prescribed, it is welcomed and supported by that majority. Suppose it is prescribed by a dictator. That fact in itself does not make the majority that wants it unwilling to accept it, although perhaps—and this is a matter of local custom—the members of that majority might be even happier to have prescribed it for themselves. Furthermore, this contented majority is willing to retain the preferred policy, perhaps even to defend it; this is precisely what is meant by equilibrium. Obviously equilibrium is based primarily on the actual distribution of tastes and only incidentally, if at all, on the process of amalgamation. On the other hand, if there is no majority preferred alternative, then *any* alternative chosen as the socially enforceable outcome is, by definition, disliked by a majority. The dislike has nothing to do with the way the outcome is chosen—though commonly the majority dissidents blame the process that has frustrated them. And this irrelevance is revealed by the fact that, were another outcome to be chosen by a "better" process, there would still be another dissident majority denouncing the outcome and probably blaming the process. Thus, even if there is no institution (such as voting) by which participants can discover that a majority is dissatisfied, there is still a motive for every one of the individual members of that majority to search for a "better" outcome. This is the essence of disequilibrium and it derives, not from particular methods of amalgamating tastes (e.g., monarchy, oligarchy, democracy) but rather from the distribution of tastes in society. Hence, the theory developed by Black, Arrow, Plott, Schofield, and McKelvey is portentously relevant to all politics, regardless of whether or not particular forms of government involve decision by voting under majority rule.

II

However general the disequilibrium of tastes may be in theory, it is obvious that in the real world there do exist many local or partial equilibria which take the form of recurrent and often reaffirmed outcomes of particular categories of events. It is the task of political theory to explain the sources of these local equilibria. I turn now to an assessment of how well theorists have performed this task.

Inasmuch as something of the same situation (i.e., a fragile or nonexistent general equilibrium, along with strong partial equilibria) prevails in the science of economics, it is possible to begin the assessment by establishing a standard of comparison for the accomplishments of political theory. Economic theorists have long sought to identify a general equilibrium, quite consciously ever since the speculations of Walras and Edgeworth and at least implicitly since the days of Adam Smith. Although the economists' specified general equilibrium is confined to one institutional setting (i.e., the competitive market) that, by its structure, contains restraints (e.g., concave indifference curves) not found in the majority rule setting for a political equilibrium, the goal of a fully identified general equilibrium, divorced from institutions, has eluded them. All the equilibria defined and elaborated have stipulated special institutions (such as *tâtonnement* or coalition-formation of any possible sort, etc.) which, because they involve stochastic variation in costs of organization and information, fail to admit unqualified prediction or description of equilibrium outcomes. Certain tentative equilibria can, however, be identified and theorists can infer certain features of markets from them. Unfortunately the equilibria identified are extremely fragile, and probably therefore meaningless, because of the presence of these stochastically variable elements of preference (Weintraub, 1979; Fiorina & Shepsle, 1982). Consequently, the main bulk of economic science deals with partial equilibria, especially equilibrium prices for particular commodities as determined by quantities demanded and supplied. This kind of analysis has been extremely enlightening because it has, for example, led to non-intuitive revelations about the counter-productive effects of such policies as tariffs, price controls, subsidies, and monopolies. In a sense, however, it has been an easy kind of analysis because it has been based on highly constrained preferences (e.g., negatively sloping demand curves). The moral is that economics, clearly the most scientifically successful of the social sciences, has benefitted from some powerful simplifications not readily available in other social scientific work.

Political scientists, lacking some principle as widely applicable as the law of demand, have not been as successful as economists in identifying partial equilibria. Nevertheless, they have identified some, even though it is necessary for them to overcome more obstacles to theorizing than it is for economists. To illustrate this success and to point out one extremely important common feature of successful theorizing, I shall list three instances of fairly well described partial equilibria, chosen because they have been the subject of recent work.

1. *Duverger's Law:* This is the proposition that plurality voting is associated with two-party systems, or, to phrase it in terms of partial equilibria, the equilibrium number of political parties is, given the use of the plurality rule for deciding on the winner, exactly two. This is a nineteenth century observation that was eventually provided with a theoretical base in the form of a rational choice theory of individual behavior, namely that voters and political supporters choose the candidate with the highest expected value. In the context of plurality voting, this kind of individual choice eliminates third parties and thus stabilizes the two-party system. A number of recent studies have offered convincing evidence of the existence of rational individual choice in precisely the kind of voting involved in Duverger's Law (Black, 1978; Cain, 1978; Lemieux, 1977; Benzel and Sanders, 1979). I have recently surveyed the history of this law to illustrate: (1) its reformulation into progressively more precise and defensible sentences; and (2) its support with increasingly persuasive evidence of the validity of the underlying theory of rational choice (Riker, 1982b).

2. *The initiation of war:* One main intention of students of politics since ancient times has been to identify the cause of war. To do so would be equivalent to specifying a partial equilibrium, because a partial equilibrium is simply the situation that occurs when a cause (i.e., a necessary and sufficient condition) is present. Until recently, no one had been able to state and support either a necessary or a sufficient condition and the study of the cause of war had been largely anecdotal. Bueno de Mesquita (1981), however, formulates a precise statement of a necessary condition for warfare as well as convincing evidence that this condition applies across cultures and through a fairly wide space of time. The condition is that rulers who initiate war have a positive expected value for the anticipated outcome. This condition goes far beyond the conventional notion that rulers of more powerful nations start wars against less powerful ones. Since it is expressed in terms of expected utility rather than power, it allows for the inclusion of ideology and thus admits

application to situations in which, for example, weaker nations attack stronger ones or a ruler attacks allies. The expected utility analysis of international politics is just beginning (Gilpin, 1981) and one can hope that Bueno de Mesquita's necessary condition may ultimately be elaborated into a necessary and sufficient one.

3. *The size of parliamentary coalitions for the formation of cabinets:* Some years ago I enunciated the size principle (Riker, 1963) that, in situations with a constant sum character, participants form minimally winning coalitions. Although my derivation of this principle from the characteristic function of an n-person game has been (mistakenly) disputed (Butterworth, 1971; Hardin, 1976), it has now also been derived from theories about the solution of n-person games (Shepsle, 1974; Schofield, 1978a). It can, therefore, be regarded as well established theoretically. Its empirical validity is somewhat less certain, although it has been subjected to a large amount of testing in analyses of coalition-formation in the real world and in the laboratory. Some of this testing has been irrelevant because the situations studied have been non-constant sum, but cabinet formation is one kind of situation that, at least in the long run, should be constant sum. (That is, what is won or lost by cabinet coalitions is the chance to run the country and this should have about the same value over a period of years.) The evidence has, however, turned out to be ambiguous. Many coalitions have been larger (or smaller) than minimal and so a number of writers have sought to modify the principle with considerations of ideological similarity (DeSwaan, 1973; Axelrod, 1970). Interestingly enough, these modifications have turned out to be unnecessary when cabinets have been analyzed on a long-run basis. While oversized and undersized coalitions are often formed, they have not lasted very long; minimal winning coalitions last (Dodd, 1976). Recently Schofield (1982a) has gone over a set of data similar to, but larger than, Dodd's in order to study the stability of cabinets. He has found that the size principle predicts the duration of governments quite well. (This suggests that cabinet formation and support is indeed a constant sum game, though there may be ideological variations of a non-constant sum nature in the initial formation of cabinets.) In any event, the work of Dodd and Schofield establishes a theory of equilibrium about cabinet formation, provides good evidence of its credibility, and tends to verify a broader political law, namely the size principle.

III

These three examples of partial equilibrium are not intended to be representative of all branches of political science, although it does happen that they come from different traditions of empirical work. Indeed, in order to collect a reasonable sample, it is necessary to look into several such traditions; to date no one tradition could, in itself, yield as many as three examples. This is, of course, quite different from economic science, where many sub-fields would yield several examples of theoretically justified and empirically validated partial equilibria.

One appropriate question from this survey is, then: Why are the examples so few? I will undertake to offer some answers, in part as a guide to the improvement of the productivity of political theorizing.

One answer is, as I have already indicated, the fact that political science lacks a widely applicable, fundamental, and well-verified law of political behavior comparable to the law of demand. As a result, we political scientists have had no compass to guide us through the thickets of facts and we have, therefore, been prone to build theories around regularities we have happened, more or less randomly, to observe.

But this situation need not continue. We still have no widely applicable empirical law, but we do have in rational choice theory a widely applicable method of analysis. The proposition that, in political affairs, people choose the action (from among several) that yields the largest expected utility is assumed in the justification of Duverger's law; it is the starting point for the statement of Bueno de Mesquita's necessary condition for war; and, in Schofield's formulation at least, it is the basis for an equilibrium in the size of cabinet coalitions. It is no accident, I believe, that these three more or less validated theories of equilibrium share the rational choice assumption. As Ordeshook and I have argued elsewhere (Riker & Ordeshook, 1973, ch. 2), this assumption assures that actors in the political model behave regularly. It plays, indeed, the same role in contemporary social science that the principle of no action at a distance played in eighteenth-century physics: It banishes witchery and other inexplicable variations from the model. If there are regularities to be observed in the world—as indeed there are—then a theory in which regularities are anticipated can conceivably describe them, while approaches postulating actions based on emotional vagaries and variable intentions have no place for regularities and cannot possibly be used to explain them.

Although there are good reasons to believe that rational choice analysis is the traditional paradigm for politics, just as it has been for economics, political scientists have on the whole never quite adopted the rational choice model as a generally accepted paradigm. Now that our understanding of the paradigm has become conscious and explicit, we have begun to exploit the technique of rational choice analysis and it seems reasonable to expect that, as we learn how to use it better, we will be more successful, perhaps even as successful as economists.

A tool is not enough, however. We also need some understanding of how we should use it. It is the absence of this understanding that serves as a second answer to the question of why examples of partial equilibria are so few in political science. As many philosophers of science have pointed out, the selection of facts to be gathered in a science is largely dictated by the scientists' conception of what they are doing. In the initial stages of any science the professionals accept lay and common sensical categories for inquiry. Thus, for example, in the beginnings of biology the main lines of endeavor were anatomy, medicine, and taxonomy. But as the questions for research came to be posed by biologists themselves, the main fields became genetics, evolution and biochemistry, subjects of which the laity knows little and to which common sense has nothing to contribute. Similarly, in political science until now, subjects of research have typically been determined by the interests of rulers or reformers who want to use scholars' discoveries to keep or get control of society. This resulted in a science that consisted of the categorization of institutions and the compilation of rules of thumb about techniques of political management.

One great contribution of political theory, with its emphasis on the amalgamation and manipulation of tastes, has been to redirect the questions of political science away from the interests of rulers and reformers into the description of what both kinds of manipulators do. The goal of a general equilibrium theory is the identification of the consequences of distributions of tastes. This leads to questions about the actions dictated by particular distributions of taste, to questions about the origin, manipulation, and modification of tastes, and to questions about the processes by which tastes are transformed into social decisions. Unfortunately, when political theorists pointed political science in these directions there was little data in the conventional categories on which to base political analysis. Hence, along with the tool of rational choice theory, political science needs the accumulation of data appropriate for the application of that theory.

We need, I believe, a tradition of what I call *heresthetics,* which should have been—but was not—invented by the Greeks as the counterpart in the study of politics to rhetoric in the study of literature. It is perhaps odd that in a survey of political theory one should advocate the accumulation of another categorization of events. But theorizing must begin somewhere and it must use appropriate data. It is to the initiation of that task that I dedicate the rest of this paper.

IV

Heresthetics, in my coinage of the word, has to do with the manipulation of the structure of tastes and alternatives within which decisions are made, both the objective structure and the structure as it appears to participants. It is a study of the *strategy* of decision. Its source is the Greek αἱρεῖσθετοι, which has to do with choosing and electing.[4] I describe it with a Greek-like word because it is a branch of knowledge that should have been, but was not, invented by Athenian philosophers and sophists. The modern impetus to heresthetics comes from game theory and social choice theory, both of which are methods of describing social decisions abstractly. The concrete decisions themselves take many forms, however, and heresthetics is the categorization of the circumstances of these decisions in a way that makes them more amenable to abstract theoretical description. Concrete grammars are necessary for the development of an abstract linguistics and heresthetics serves in an analogous role in the development of the theory of social decision.[5]

Two examples of what would have properly been called heresthetics, if the word had existed when they were written, are:

(1) Pliny the Younger's letter 14 of book 7, to Titus Aristo, as used by Robin Farquharson for his running example in the *Theory of Voting* (1958 and 1969). It is not clear whether Pliny intended this letter as a commentary on social decision or merely as a cry to Titus for help in defending the con-

stitutionality of Pliny's ruling from the chair of the Senate. About Farquharson's intent, however, there is no doubt for he offered the letter as an example of alternative methods of voting. Quite unbeknownst to him—for he had a bad translation—the letter also serves as a commentary on manipulation of the agenda (by Pliny) and on sophisticated voting (by Pliny's unnamed opponent) (Pliny, Ed. by Radice, 1969). In the incident described in the letter to Titus, Pliny ruled for plurality choice among three alternatives on a single division, while the customary procedure would have been a sequence of pairwise decisions. His motive was to ensure the victory of his preferred alternative over the Condorcet winner, which he placed second. In the voting, however, one group of his opposition voted sophisticatedly so that the Condorcet winner actually won, doubtless to Pliny's chagrin.

(2) Schelling, *The Strategy of Conflict* (1960). Several chapters of this volume concern manipulative techniques. When originally published (in 1956 and 1957) as scientific papers, Schelling interpreted them as contributions to a theory of bargaining, but later (in 1960) he believed they were contributions to the theory of games. He was entirely justified in his uncertainty about the classification of these chapters which concern threats, promises, and commitments generally. They are not a theory of bargaining because they have nothing to say about the compromise of tastes (and thus are quite unlike the work of, say, Harsanyi, 1980). Nor are they about game theory because they are not concerned with the way the interaction of players' choices determine an outcome. Rather, they are about the strategems that participants use in selecting their own actions and in limiting the choices of others. They are about the strategies to create situations in which game theory and bargaining theory apply. As such, they are squarely in the field of heresthetics.

Both Schelling's and Farquharson's work are regarded as path-breaking, and properly so, because they deal with a kind of data no one had thought to collect before and furthermore with data that is highly relevant to theories of social decision. To think about the subject matter of these theories—which is what both writers wanted to do—it helps to be able to refer to concrete detail of the sort they collected. In that sense, the content of heresthetics can be said to come out of the demands of science, not out of lay language and observation; it ought, therefore, to be regarded as a significant and new kind of data in the study of society.

It is hard to define heresthetics other than by examples because the purpose of the concept is to provide an open-ended set of categories for events that have not heretofore been systematically categorized.[6] One more example may be helpful, so I offer one that was brought to my attention by Richard A. Smith who used it as an introductory example of agenda-setting in his superb dissertation (1980) on agenda-setting by lobbyists. This example, as related in Redman (1973), concerns Senator Warren Magnuson, who was trying to stop the transport of nerve gas across his state of Washington, enroute from Okinawa to Colorado. Because relatively few other Senators shared his provincial concern for impressing the voters of the state of Washington, he expected to lose. But he won, mainly, so Redman reports, because of clever heresthetical tactics. Redman tells the story well (p. 207):

> . . . during the Senate debate on his [Magnuson's] amendment to block shipment, . . . I prepared another memorandum cataloguing the arguments he had marshaled against the shipment and took it to him at his desk on the Senate Floor. He surveyed the memo cursorily, then handed it back with an annoyed "No, no, no!" Bewildered, I retreated to the staff couch and waited to hear what argument he intended to use instead of the familiar ones of possible sabotage, dangerous sections of track along the proposed route, and populations that would have to be evacuated as a precaution against leakage. When the time came, he took a wholly novel and ingenious approach. The issue, he told his colleagues, was not one of the people versus the Pentagon, as the news media seemed to assume. Instead, it was another case of the President versus the Senate. The Senator from West Virginia (Robert C. Byrd) had recently offered a resolution, which the Senate had passed, stating that the Senate expected the President to keep it informed throughout the treaty negotiations with the Japanese government on the subject of Okinawa. The President's sudden decision to move the nerve gas off Okinawa must reflect some aspect of those treaty negotiations, Magnuson insisted—and the Senate had not yet been informed of, much less consented to, any such agreement. To allow the nerve-gas shipment under such circumstances, he asserted, would be to abandon the Byrd Resolution and to abdicate the Senate's rightful role in treaty-making generally. The President, Magnuson said, might get the idea that he could ignore the Senate and its Constitutional prerogatives whenever he wished. Jolted by this reasoning, the Senator from West Virginia and his Southern colleagues—friends of the Pentagon almost to a man, but vigilant guardians of the Senate's Constitutional responsibilities—voted down the line with Magnuson. The amendment, which had been doomed a few minutes earlier, passed overwhelmingly.

Even though the collection of heresthetic detail is recent, the human behavior involved is easy to observe in vastly different times and places. Heresthetics is in fact universal. One of the settings in which it is easy to observe the manipulation of tastes is the parliamentary assembly, where there are many groups, where individual loyalties are shifting, and where leaders win by assembling ad hoc winning coalitions. Not surprisingly, the Roman Senate is the locale for Farquharson's example (i.e., Pliny's letter) and the American Senate is the locale for Smith's example (i.e., Redman's story about Magnuson). One would expect, therefore, that examples for heresthetical categories could be found in the assemblies of classic Greece, the earliest historical assemblies. And so they can. W. Robert Connor (1971) describes the Athenian assembly as "kaleidoscopic" or "polycentric," composed of several relatively small groups of friends, almost clubs, which come together in support of particular motions and then regrouped on others. This is precisely the kind of setting in which the open manipulation of the agenda and of the salience of dimensions of judgment is likely to be most visible. And indeed, tiny as the historical record is (mostly Thucydides' *History* and in the next century Demosthenes' *Orations*), it contains quite a large number of descriptions of heresthetical devices. Connor discusses, for example, Thucydides' account of the development of the coalition against Alcibiades. That coalition, having failed to halt the disastrous Sicilian expedition, turned to an attack on Alcibiades, its main sponsor. The leaders of the coalition raised the issue of impiety against him—the charge was profaning the mysteries and defacing the statues of Hermes—and, although they failed initially, they ultimately had Alcibiades recalled from his command, though the expedition itself was not stopped, and he deserted to Sparta (Thucydides, VI, 28-29, 61). Connor sees this as an example of the structuring of a coalition, but it is also an example of the introduction of a new issue (Alcibiades' impiety) to transform the coalition against the expedition from a minority to a majority. An even better example of the heresthetical tactic of introducing a new dimension is found a bit earlier (Thucydides, VI, 8-24), in Nicias' argument against the expedition. Initially he opposed it on the ground that it was too large an undertaking and that it was intended only to gratify Alcibiades' tastes for the magnificent. When Alcibiades carried the day by promising to make the Athenians the rulers of all Hellas, Nicias responded in fine heresthetical fashion by introducing a new dimension of judgment. Thucydides wrote (VI, 19-24):

> Nicias, seeing that his old argument would no longer deter them, but that he might possibly change their minds if he insisted on the magnitude of the force which would be required, came forward and spoke as follows: ". . . I say, therefore, that we must take with us as a large heavy armed force both of Athenians and of allies. . . . Our naval superiority must be overwhelming. . . ." These were the words of Nicias. He meant either to deter the Athenians by bringing home to them the vastness of the undertaking or to provide as far as he could for the safety of the expedition if he were compelled to proceed. The result disappointed him. Far from losing their enthusiasm, . . . they [i.e., the Athenians in assembly] were more determined than ever.

This reads just like an event in modern democratic politics: A politician who loses on one platform or set of arguments tries another and, when he loses on the second, his friends try still a third (i.e., the attack on Alcibiades' alleged impiety) on which they finally win, at least in part. I find in Thucydides quite a few other examples of heresthetics: false presentation of an issue (V, 45-47); coalition building around a new issue (IV, 84-88); agenda control (II, 21-22); and the addition of a dimension of judgment (I, 80ff.). Altogether, therefore, it is clear that, given similar settings in different cultures, heresthetical behavior is universal.

Furthermore, since it clearly concerns the dynamics of the approach to important human decisions, one would expect that, even in classic Greek times, men would have begun to collect and classify examples of heresthetic, just as they did for rhetoric, which concerns another feature of the approach to decision. And there was indeed a tiny start in that direction. Demosthenes was a master of heresthetic as well as rhetoric and his orations have enough detail for the modern reader to understand the full scope of his strategy. His modern biographer, Werner Jaeger, commenting on Demosthenes' speech, *On the Symmories,* in which he advocated a larger navy based on a wider assessment of taxes, ostensibly in support of a proposed Persian War, but actually—and successfully—to dampen enthusiasm for that war by emphasizing its cost, observed that this was the same heresthetical technique Nicias had used against Alcibiades. In the *Art of Rhetoric* the pseudo Dionysius of Halicarnassus classified Demosthenes' maneuver with a similar maneuver by the Spartan king Archidamas (as reported by Thucydides, I, 80ff.) to introduce a fiscal dimension to defeat an outcome expected to win if determined only by a military dimension (Jaeger, 1938, pp. 226-227). The false Dionysius, putting the story of Archidamus and the text of Demosthenes together indeed started up a heresthetical tradition. Unfortunately, it never developed further.

I wonder, why not? One possible answer is that, while heresthetic and rhetoric were initially entwined in classical thought, as the democratic assembly—the one place where political strategy could be easily observed—disappeared from classical institutions, only rhetoric in the sense of persuasion was left to be observed and classified. In the classical tradition it was customary to categorize the settings for rhetoric into the deliberative (i.e., in assemblies), the forensic (i.e., in courts), and the epideictic (i.e., in ceremonies). Two historians of Greek rhetoric, Wilcox (1942) and Kennedy (1963), assert that the early rhetoricians were concerned mainly with deliberative persuasion. They argue in support, first, that rich young men of prominent families would hardly have paid the sophists large fees simply to be taught how to be lawyers, and second, that Plato's intense hostility (in *Gorgias* and *Protagoros*) to rhetoricians must have been based on a fear that they would teach people how to distort public policy in general (i.e., in the assembly) rather than in simply private disputes (i.e., in the courts). These arguments seem persuasive to me, especially since the rhetorical tradition from the beginning emphasized persuasion by eloquence rather than persuasion by argument. Jacqueline de Romilly (1974) has argued that rhetoric began in the association of poetry and magic (i.e., spells and charms) with public speech and argument and was developed by the sophists into an art to persuade about the correctness of any preferred alternative. "Plato resented," she wrote, "not the magic [in Gorgias' thought] but the offensive pretense of turning it into science" (de Romilly, 1974, p. 38). Aristotle tried to save rhetoric for philosophy by cleansing it of what Plato despised, by, in fact, turning it into a policy science in which persuasion consists of showing that sentences have a high probability of truth. In particular, he wished to banish eloquence from style so that it "be transparent, not magical" (de Romilly, 1974, p. 73). If Aristotle had succeeded then rhetoric would have been concerned primarily with political decision and would doubtless have continued to subsume political strategy or heresthetics as well. But Aristotle failed and rhetoric became almost entirely a matter of eloquence. It was Cicero who settled the tradition for most of the next two thousand years. In the *Orator* (line 44) he says the rhetor "must consider three things, what to say, in what order, and in what manner or style to say it," but then he goes on to remark that he will treat the first two only briefly because, while important, they are a part of "ordinary intelligence rather than eloquence." Plainly, therefore, eloquence was in Cicero's mind the distinguishing feature of rhetoric and he banished all else, including what might have become heresthetic.

If this historical speculation is correct that rhetoric became simply persuasion—and the least rational kind of persuasion at that—then rhetoric could hardly sustain heresthetic, especially in the absence until modern times of deliberative assemblies where political strategy is easy to observe. While it is thus explicable that the subject of heresthetics has not been studied, it is nevertheless clear that the reason scholars have ignored it has nothing to do with its significance, but rather with the accident that it was excluded from the one tradition in which it might have survived.

V

In the previous section I sought to define heresthetics, to show its universality, and to explain why, despite its universality, it is unstudied. I turn now to a further explication of heresthetics by distinguishing it from rhetoric with which, given the relatedness of subject matter, it might be confused.

Rhetoric and heresthetic are both techniques of winning. But they are different kinds of techniques. Rhetoric is persuasion. It involves confronting a judge or jury in a courtroom, or voters on a committee or in a polity, or buyers in a marketplace, or friends or philosophers in dispute with sentences that may convince them that you are correct or believable. The sentences may convince because they are beautiful or sonorously uttered, or because of their irrefutable arguments, or because of their presentation of the situation in a way the auditors are predisposed to accept. But the essential feature, qua rhetoric, is that they convince. With heresthetic, on the other hand, conviction is at best secondary and often not even involved at all. The point of an heresthetical act is to structure the situation so that the actor wins, regardless of whether or not the other participants are persuaded.

The contrast between rhetoric and heresthetic is nicely seen in the dilemma, a form shared by the two fields. Rhetorically, the dilemma-maker succeeds because he convinces the auditors that, if his opponent cannot resolve the dilemma, then the opponent's position is intellectually weak. Hence the dilemma is a device for persuasion. Heresthetically, the dilemma-maker succeeds because he forces his opponent into a choice of alternatives such that, whichever alternative is chosen, the opponent will alienate some of his supporters.

Consider the best of the textbook examples of the rhetorical dilemma: It is the pair of dilemmas posed by Tisias and Korax. Korax, whose name means "crow," was the founder of the school of rhetoric at Syracuse, and was Gorgias' teacher. Tisias, who was also Korax's student, had undertaken to pay tuition when he won his first case; but he had neither practiced nor paid, so Korax sued. Tisias responded with this dilemma: "If I win the case, then I need not pay because I am freed by the judgment of the court. If I lose the case, then I need not pay because the terms of the contract will not have been satisfied. Since I must either win or lose, I need not pay." To this Korax replied: "If I win the case, then I must be paid because of the judgment of the court. If I lose the case, then I must be paid because the terms of the contract will have been satisfied. Since I must either win or lose, I must be paid." The judge dismissed the case, saying Tisias was a bad egg from a bad crow. Although Korax lost, the advertisement was so good that people still repeat it 2500 years later. Not only did Korax show that he could teach a student to present what appeared to be an absolutely convincing defense, he also showed that he then could himself tear it all down with an exact riposte.

Consider now an heresthetical dilemma, Lincoln's famous question to Douglas in the Freeport debate. During the senatorial campaign of 1858 Lincoln met Douglas, the incumbent, in debates in several Illinois cities, while both were soliciting support for candidates for the state legislature who were in turn pledged to support them for the Senate. Lincoln's question was: "Can the people of a United States Territory, in any lawful way, against the wish of any citizen of the United States, exclude slavery from its limits prior to the formation of a state Constitution?" (Lincoln, 1958, p. 108). To the contemporary reader this seems perhaps innocuous enough; but it is at the very center of the partisan dispute of the era and expresses exactly the raison d'etre of the Republican party.

The parties of the tradition of Federalist-National Republican-Whig-Republican did not do well throughout the first two-thirds of the nineteenth century. Indeed between 1797 and 1867 they elected only one President by a majority, while the Jeffersonian-Jacksonian Democratic parties probably elected ten by a majority. The main problem for the parties of the Federalist-Whig-Republican sequence was that they usually espoused a platform of commercial expansionism which appealed only to a minority; Democrats, on the other hand, espoused agrarian expansionism which often appealed to a majority. Naturally the Whig-Republican leaders searched constantly for a better platform and they finally found one in the combination of commercial expansionism with the limitation of slavery. This platform had the advantage of splitting the Democrats, who were well distributed in North and South, more than it did the Whigs, who were weak in the South. Lincoln's question exactly expressed this Republican strategy.

The Democratic defense was to cover up the slavery issue, to assert it was a local concern that should be banished from national politics. In the 1850s Douglas had been the main agent of this defense by sponsoring the Kansas-Nebraska Act. This Act satisfied the South because it rendered slavery local and it mollified those Northern Democrats who were tempted to oppose slavery because it allowed the territories to eliminate slavery. It failed, however, to localize the issue, especially after the ruling in the Dred Scott case (1857) placed slavery beyond the control of local legislators. But Douglas was still trying in 1858 to patch up the national Democracy by localizing the issue and Lincoln's question was a trap to encourage him to do so, thus:

If Douglas answered "yes" (that territories could exclude), then he would alienate Southern Democrats for whom the Dred Scott decision was the new status quo. Since he hoped and expected to be the Democratic candidate for President in 1860, to answer "yes" would be to reduce his Southern support and thus to jeopardize his chances in that election. At the same time a "yes" answer would placate Illinois Democratic voters with free soil principles and thus enhance his chances for reelection to the Senate.

If he answered "no" there would be converse results. He would alienate Illinois free soil Democrats and possibly lose his Senate seat, but he would win the loyalty of the South for the Presidential election of 1860.

Since he had to answer either "yes" or "no," he jeopardized either his election in 1858 to the Senate or in 1860 to the Presidency.

Douglas answered a kind of "yes." Without disputing the Dred Scott decision, he said "slavery cannot exist a day or an hour anywhere, unless it is supported by local police regulations," which, he argued, an anti-slavery territorial legislature need not adopt (Lincoln, 1858, p. 113). This was the answer Lincoln's advisors feared—they had opposed his asking the question—for it probably helped Douglas to reelection. But, as things turned out, Lincoln won in 1860, helped immeasurably by Douglas' shortsighted "yes." An intriguing question: Did Lincoln in 1858 see ahead to his own candidacy in 1860 or was he sacrificing himself for the greater Republican good?

Clearly the dilemma Lincoln posed was not intended to persuade the audience that Douglas' position was intellectually weak. Indeed, it did not depend for its effect on convincing anybody of anything. It was simply a stratagem to force Douglas to reveal to one of his incompatible groups of supporters that he was faithless to its cause. As such it was strictly a heresthetical device that set up a situation for subsequent decisions in such a way that at least one of the decisions would be to Lincoln's taste.

The essentially heresthetical character of Lincoln's act of posing the dilemma is underlined by the fact that, viewed another way, it is very similar to the heresthetical devices of Nicias or Demosthenes, which I described earlier. They raised a new fiscal dimension of judgment in order that voters would be less enthusiastic about the preparations for war so heartily approved on a military dimension of judgment. Lincoln's stratagem also involved raising a new dimension, slavery, to drive some voters to oppose the agrarian expansion they would otherwise approve. Douglas' response was to try to suppress the new dimension. Thus, although the Lincolnian device has the form of a dilemma, it has the substance of the generation of a new issue.

VI

The purpose of this essay is to recommend the study of heresthetic, not actually to study it. Nevertheless, in the hope of making my recommendation more persuasive, I conclude with a brief statement of what I have so far myself learned from the study of heresthetic.

In the study of politics and public policy we devote most of our attention to the analysis and interpretation of the platforms and policies of the winners of political disputes, elections, wars, and so forth. And this is quite proper because the preferences of the winners are the values that are authoritatively allocated. That is, the tastes of the winners are the actual content of social decisions and thus the content of the immediately subsequent present time. Conversely, we ignore the policies and platforms of the losers because these are the junk heap of history, the might-have-beens that never were. But we should not, I think, entirely overlook the losers and their goals for the losers provide the values of the future. The dynamics of politics is in the hands of the losers. It is they who decide when and how and whether to fight on. Winners have won and do not immediately need to change things. But losers have nothing and can gain nothing unless they continue to try to bring about new political situations. This provides the motivation for change. And the confirmation of this fact comes from the study of heresthetics. Losers are the ones who search out new strategies and stratagems and it is their use of heresthetics that provides the dynamic of politics.[7]

In my recent work, *Liberalism against Populism* (1982) I have discussed, mainly in chapters 6, 7, and 9, the way in which participants in an electoral system can manipulate electoral procedures to their advantage. Strategic manipulation is, of course, one important part of heresthetics. So the categories of manipulation used in that work are also categories of heresthetics. The categories I used were:

(1) strategic voting, which involves voting for a less preferred alternative (in lieu of a more preferred one) at some initial decision point in order that the voter can achieve (or improve the chance of achieving) an outcome at the final decision that the voter favors over the outcome expected from initially voting for the more preferred alternative;

(2) manipulation of the agenda, which is structuring the set of alternatives or the procedure of voting in such a way as to produce an outcome more favored by the manipulator than might otherwise have been achieved.

Within each of these two main categories are a number of kinds of examples, each of which has the property that the user of the particular heresthetical device must be a person who has lost a decision or reasonably anticipates losing one. Then the stratagem itself becomes a means either of winning on a new issue or of transforming the anticipated loss into a victory. Thus, strategic voters in each of the following examples are persons who anticipate losing if they do not adopt the stratagem:

(1) Avoidance of "wasted" votes: In plurality voting over three or more alternatives, a voter who most prefers some alternative other than the two most popular may nevertheless vote for his or her favorite among those two so that in the vote count the favorite (within the top pair) will have a better chance of winning than if the voter had "thrown away" his or her vote on the most preferred alternative. Clearly this heresthetic device is available only to supporters of the third most popular (or, in the case of more than three, the least popular) alternative;

(2) Creation of a voting cycle: When a motion as amended is so distasteful to some of the supporters of the original motion that they reject the amended motion, it is of course open to the opponents of the original motion to attach the amendment to the motion in the hope thereby of creating a cycle in which

(a) the status quo beats the amended motion

(b) the amended motion beats the original motion

(c) and the original motion beats the status quo.

In such a cycle, some opponents of the original motion are, as line (c) indicates, losers; but they can perhaps become winners if they can attach the amendment, as indicated by line (a);

(3) Vote-trading: Potential vote-traders are those who, on divisions on two motions, can expect to win on the motion less valuable to them and to lose on the more valuable one. If they can find enough persons oppositely situated so that they can, by trading, together reverse the outcomes, then the traders will win on their more valuable motions and lose on their less valuable ones. Manifestly, traders must be losers in order to gain by trade.

The same features are exhibited by examples of manipulation of the agenda:

(1) Arrangement of the sequence of decisions: Those directly in control of the agenda can, if they foresee an unfavorable outcome from the normal or customary agenda, rearrange the agenda so that alternatives more popular than their favored one are eliminated prior to the decision on their favorite. Hence they can improve its chances. They need to engage in this maneuver, however, only when they expect the "natural" vote to go against them;

(2) Introduction of new alternatives: Those who expect their preferred alternative to lose initially may introduce new alternatives, even as mere participants not leaders. Assuming the new alternative is at least better for them than the most popular alternative in the initial set and that some of the supporters of the popular alternative will prefer the new alternative, then the introducers improve their chances of defeating the initially most popular alternative. Obviously only those who initially expect to lose are motivated to use this strategy.

It seems clear, therefore, that, in the case of all the heresthetical devices I have examined, the users are persons who are not now winning but hope to become winners. Insofar as these devices involve a fairly wide range of political life, it also seems clear that losers are the instigators of political change and it is they who are thus motivated to exploit heresthetic and who become the agents of political dynamics.

A generalization of the sort just uttered is exactly the kind of theoretical payoff political scientists might expect to obtain from heresthetical categorization. It is in the hope of further such generalizations that I recommend the study of heresthetics.

NOTES

1. See Riker, 1977, 1980, 1982(a) for more detailed versions of this history.
2. This feature of Black's work is closely related to the Hotelling-Downs (1957) model of spatial equilibrium. For a survey of the development of work on that model, see Riker and Ordeshook (1973) and a forthcoming volume by Melvin Hinich and James Enelow.
3. One of my critics believes the statement in the text is too strong because all sorts of institutions impose constraints on outcomes and thereby preclude the possibility that "anything can happen." Of course, he is correct with respect to applications to the real world, as I indicate in section II of this paper. But it is important to note that, in the abstract without considering institutions, there is almost no likelihood of an equilibrium.
4. "Heretics" might be a better Anglicization of the Greek source, but that word is a form of the word "heresy," which has been co-opted by religion. Hence, I prefer the term I have chosen.
5. One of my critics defines my call for a new kind of data as a plea for us "to devote more systematic attention to the *dynamics* of collective decision making. A standard criticism leveled against existing political models is that they are static in nature. Even when the more ambitious among us attempt to model dynamic phenomena, the essentials of the model remain constant. Of course, we must learn to walk before we can learn to run and all that, but at some point a progressive political theory will begin to remove the basis of the criticism."

6. One of my critics believes that my implicit definition of heresthetics is too broad. He points out that I seem to be considering as heresthetical both the manipulation of preferences and the manipulation of alternatives. He suggests that I ought to restrict the reference to the latter manipulation (i.e., situations where preferences are constant and only alternatives change) because we already have names for the former manipulation. I hesitate to restrict reference in this way, however, because one of the things that happens in the manipulation of alternatives is that as a consequence the salience (and hence the content) of preferences are also changed.
7. One of my critics suggests that, by emphasizing the creative (perhaps artistic) element of heresthetics, I am, unconsciously or slyly, removing it from the range of science. I certainly do not intend to do so. While it is certainly true that human life involves artistic creations, we would deny the possibility of all social science by asserting that creativity cannot be scientifically described. Just as Von Neumann and Morgenstern discovered how to discuss analytically creative acts of choice among strategies, so I believe we can discover how to discuss analytically creative acts of structuring strategies themselves.

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3

Toward Theories of Data: The State of Political Methodology

*Christopher H. Achen**

In one sense, political methodology in the early 1980s enjoys robust health. Applications of powerful econometric techniques—simultaneous equation estimation, time series methods, analysis of covariance structures—appear in political science journals on a frequent, if not yet routine, basis. *Political Methodology*, less than a decade old, prints a large fraction of the best empirical research done in political science. Perhaps most tellingly, the quantitative method has attained full legitimacy among serious scholars, including those who do not use it. Just a decade ago, a graduate student interviewing for an American politics position at a first-class liberal arts college could be asked whether his interest in mathematical techniques was some youthful fancy from which he might be expected to recover. Today, one trusts, no department with an eye to its reputation would do the same.

Yet if these are the best of times, they are the worst of times as well. Several decades after its beginning, political methodology has so far failed to make serious theoretical progress on any of the major issues facing it. Psychologists invented factor analysis and scaling methods to cope with mental tests; economists created structural equation estimation to deal with their models, especially the economy-wide macroeconomic theories; and even sociologists have contributed latent class analysis to the corpus of social scientific methodology. Political science has done nothing remotely comparable.

Political methodologists have largely occupied themselves with two activities. First, they have continued to develop the major quantitative research

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tool in the discipline, the opinion survey. Most issues of *Political Methodology*, for example, have at least one article exploring the reliability or validity of alternate survey techniques. Political methodologists also contribute regularly to the *American Journal of Political Science*, which often contains articles on survey research, and *Public Opinion Quarterly*, which covers the same topics almost exclusively. Unfortunately, nearly all this work has been atheoretical. Question wordings, interview design, and techniques for reducing nonresponse are invented as needed, with no overarching framework, so that every new topic must be tackled *de novo*. The result is a body of work that is certainly "methodological" in the broad sense of the term, and without it, empirical work in political science would be drastically impaired. But survey research remains a purely applied science. If judged by the standards prevailing elsewhere in the discipline, where good work is recognized for its contribution to theoretical understanding, survey methodology has achieved very little intellectual advance.

Second, political methodologists have expended much of their energies teaching the rest of the discipline new statistical techniques invented in other fields. In a typical publication of this kind, an intuitive exposition is combined with an illustration or two from political science. The new method may be compared favorably with an older, more common technique. The result is an article that can be assigned to graduate students needing both motivation and simplified mathematics to learn the material.

Intellectual middlemen have their uses, of course. Political science remains a field with woefully little methodological capital, and any successful investment scheme deserves praise. But remedial teaching is not scholarship, and popularization does not a methodologist make. Here as in survey research, there is no real intellectual achievement of our own to report.

Political methodologists as a class have largely avoided theoretical thinking. With few exceptions, we do not investigate carefully the properties of the new methodological procedures constantly appearing in the discipline. Certainly we rarely invent a legitimate estimator, prove consistency theorems, or derive confidence intervals. Instead we shop for hand-me-down techniques invented by statisticians, psychologists, and economists—techniques often meant for very different tasks. Empirical researchers then adopt the statistical ideas and methodological standards we have propounded. Is it any wonder that the pages of our journals frequently have the look of living rooms decorated at garage sales?

The general lack of interest in fundamental questions is particularly disquieting because it coexists with deep methodological conundrums specific to political science. One thing political methodologists *have* done is to show that measurement does matter. Apparently trivial differences in question wording can lead to large changes in response patterns. (Among many possible examples, two particularly striking cases appear in Bishop, Tuchfarber, and Oldendick, 1978; and Sullivan, Piereson, and Marcus, 1978.) Nor are the peculiarities confined to survey items. The ghastly results all too common in working with aggregate voting data are also well documented, and the "ecological fallacy" has destroyed the credibility of many well-intentioned projects. Indeed, skepticism about aggregate data is so widespread that the quantitative historical research once so common in the discipline has very nearly disappeared. Thus ignoring our methodological foundations because "they probably don't matter much in practice" is simply naive. Whatever else may be uncertain in methodology, there is ample evidence that the shaky foundation it provides has weakened intellectual structures across the discipline.

These puzzles in our data sets are not work-a-day procedural problems with administrative solutions. Better question writing and more careful data collection will make no more than a marginal difference. Only a better understanding of our processes of measurement can truly help us. Nor can we expect other disciplines to supply the necessary theory. In large part, the statistical questions posed by political data are of relatively little interest outside political science. No one is likely to solve them but ourselves. So far, we have not been much interested.

If political methodologists were to take up their main agenda, the first item of business would be the formulation of our troubles clearly enough that they could be diagnosed and treated. The remainder of this essay is meant to begin this formulation. Of course, remarks of this length cannot hope to cover the entire field. Political scientists are interested in simultaneous equation estimation, time series, discrete choice analysis, and a host of other interesting and important statistical methodologies not reviewed here in detail. None of these statistical fields, however, raises difficulties so much our own as the two long-standing research areas that are the focus of this article. The first is the issue of measurement error, the second the aggregation problem. Each illustrates in striking fashion the dimensions of the task before us.

MEASUREMENT ERROR

Quantitatively-trained political scientists are very much aware that measurement error can bias their findings. Most have at least a vague recollection from introductory econometrics that under some conditions, it can attenuate regression coefficients. Frequently, several other features of measurement error are "remembered" as well, for example:

(A) If some of the independent variables in a multiple regression are measured with error, all coefficients will be attenuated. Thus if one can establish the existence of an effect in spite of measurement error, the true effect must be even larger.

(B) Measurement error can be a serious problem in the bivariate regression case, but the more independent variables without error in the regression equation, the smaller the biases will be.

(C) If the measurement error variance in the independent variables is a small fraction of their total variance (high reliability), the bias in the coefficients will be a small fraction of their true size.

(D) In any case, discussions of all these questions are contained in any standard introductory text on econometrics.

All four statements are false. To take the last first, the surprising fact is that some of the best multiple regression texts ignore the subject of measurement error in the independent variables entirely (Kelejian & Oates, 1981; Dhrymes, 1970). Nearly all the rest discuss the bias only in very general terms (Johnston, 1972; Hanushek & Jackson, 1977; Pindyck & Rubinfeld, 1976; Rao & Miller, 1971; and Theil, 1971). The direction of bias for the coefficients is given solely for random error in the bivariate case—a single independent variable—where, of course, the effect is to drive the estimated coefficient toward zero. (Rao and Miller also reference, but do not discuss, an antique article by Theil that gives a rough approximation for the asymptotic bias when there are exactly *two* independent variables, one of which is observed with random error.) Only Maddala (1977) discusses non-random error (in the bivariate case), or random error in multiple regression (with just two independent variables, each observed with error).

None of these texts gives any explicit results when there are more than two independent variables. In fact, as late as 1973, *Econometrica* was still exploring the case of multiple regression with a single variable observed with random error, and finding the direction of the asymptotic bias only for the variable with error. (It is biased toward zero.) A complicated procedure was given for learning the direction of the biases in the other coefficients, but nothing could be said about their sizes (Levi, 1973). More recently, as part of a sophisticated and fairly lengthy treatment of random measurement error in regression, Dhrymes (1978, pp. 242-266) has shown that R^2 is driven downward in errors-in-variables regression, which implies that the F-test for the overall significance of the regression will also be depressed. But on the question of the direction of the bias in coefficients or t-ratios, apart from the usual bivariate regression result, he throws up his hands.

Apparently the only result in the literature more general than these was published in *Political Methodology* (Greene, 1978). In the ordinary multiple regression setup with k independent variables, each possibly observed with random error, let g_j be the fractional asymptotic bias (inconsistency) in the jth coefficient. For example, $g_j = -0.5$ means 50% attenuation. Then in effect, Greene's main result is that a certain weighted average of these biases must be negative. In particular, let β_j be the true jth coefficient and ω_j^2 the error variance of the corresponding independent variable. Then:

$$\sum_{j=1}^{k} \beta_j^2 \omega_j^2 g_j < 0. \tag{1}$$

Thus in some sense, the "average g_j" is negative, and measurement error drives coefficient estimates toward zero.

Note, however, that this result is limited in several respects. First, as Greene notes, it gives only an average direction of bias that may not apply to most of the coefficients in question. In fact, the theorem guarantees only that at least one of the coefficients will be driven down. The rest might actually be increased in absolute size. Moreover, no limits on the size of the biases are given. Nothing in the result guarantees that coefficients will be attenuated in the usual sense that they will fall somewhere between the truth and zero. We know that they will be driven toward zero, but nothing prevents their being driven right on through it to the other side. Finally and more importantly, independent variables with no error ($\omega_j^2 = 0$) are weighted zero in (1). That is, the inequality tells us nothing about the direction or size of the biases in variables observed without error. Thus the result covers only the variables with error. There appears to be no literature at all on the size of the biases in the other coefficients.

Returning to the other three "facts" researchers remember from econometric theory, one can see that the available literature neither supports nor contradicts them. Answers to them simply are not known, and in their most general form, the complexity of the mathematics will probably prevent much progress. However, a great deal can be done in certain simple cases.

Consider, then, the case of multiple regression with random measurement error in a single independent variable, the other independent variables being measured exactly. Then denote the variable with error by x_1, its error variance by σ_e^2, and its true coefficient by β_1. Finally, let the residual variance when the *true* first variable is regressed on the other independent variables be s^2. Then if $\hat{\beta}_1$ is the estimated coefficient for the variable with error, its "asymptotic value" (i.e., probability limit) is as follows (see the appendix):

$$plim\,(\hat{\beta}_1) = \frac{s^2}{s^2 + \sigma_e^2}\beta_1. \tag{2}$$

For any other estimated coefficient, say $\hat{\beta}_j$, its asymptotic value is:

$$plim\,(\hat{\beta}_j) = \beta_j + \frac{\sigma_e^2}{s^2 + \sigma_e^2}\beta_1 b_j, \tag{3}$$

where b_j is the coefficient on the jth independent variable when the true first independent variable is regressed on all the others.

With this machinery, the questions (A), (B), and (C) raised above can be addressed. Note first that the coefficient on the variable with error is indeed attenuated. Equation (2) shows that asymptotically, the estimated value of the coefficient will fall between zero and the truth. Nothing of the kind is true for the other coefficients, however. As seen in (3), other coefficients will "pick up" part of the explanatory power of the variable with error, with the size of the acquisition varying according to their correlation with it (b_j). Thus variables measured without error can have their effects increased or decreased; there is no net tendency for them to be attenuated.

Next, observe that the size of the bias depends inversely on s^2, the residual variance when the true first independent variable is regressed on the others. Now s^2 can be taken as a measure of collinearity: the more intercorrelated the independent variables, the smaller is s^2, until at perfect collinearity, $s^2 = 0$. It follows that high collinearity will induce relatively large asymptotic biases, *no matter how small the measurement error variance.* The coefficient for the variable with error, for example, can be driven arbitrarily closer to zero just by increasing collinearity, even if the reliability is 99%. Small error variances do not necessarily give much protection against large inconsistencies. And of course, this bias is particularly pernicious because it depends on the unexplained variance in the regression of the true first independent variable on the others, a quantity that cannot be computed due to the measurement error in the first variable.

Finally, note that s^2 is quite likely to become smaller and smaller as additional variables are added to the equation, simply because the more variables there are, the better that x_1 can be forecast and the smaller its residual variance s^2 will be. Thus additional independent variables will tend to reduce s^2 and raise the inconsistencies in (2) and (3), making the biases worse rather than better. Therefore, as promised, we have shown that all four statements (A)-(D) are false in general, since they are false for the simple case of one independent variable with error. Measurement error, then, is more dangerous than commonly believed.

So far only purely random measurement error has been considered. Unfortunately, political scientists can rarely be certain that their errors are uncorrelated with the true values and with other independent variables. In fact, in many applications just the reverse is suspected. For example, Ross and Duvall (1982) have reminded us once again of the treacherous nature of cross-national political information. They note that even the size of standing armies in modern nations is subject to gross errors. In 1965, for instance, one major source reports 132,000 men under arms in Norway, while another gives 36,000 as the correct figure (p. 31). Using sources like the International Institute for Strategic Studies and the U.S. Arms Control and Disarmament Agency, Ross and Duvall commonly find discrepancies of 50% or more in the estimates for the same country in the same year throughout the modern era, and the mean is almost 25%. Some sources correlate rather well with each other, others quite poorly. It seems unlikely that this kind of error is purely random.

Unfortunately, nonrandom error presents even more serious threats to inference than the random kind. Suppose that just one variable in a multiple regression is observed with error, and that the error may be systematic in the sense of being correlated with any of the independent variables. (Correlation with the disturbance term, which can only make matters worse, will be excluded.) Then we have this mathematically trivial but substantively significant result: no matter what the true regression coefficients, if there is systematic error in a single independent variable, ordinary regression can converge to *any* pattern of coefficient estimates.

Proposition. Let β^* be any given vector of dimension k. Then in the multiple regression setup with k independent variables, there exists a pattern of systematic measurement error in the first independent variable such that the OLS coefficients will tend asymptotically to β^*.

Proof. Let the true model be:

$$y = x_1\beta_1 + X_2\beta_2 + u, \tag{4}$$

where y is the n-dimensional column vector of observations on the dependent variable, the column vector x_1 contains the n observations on the first independent variable and the $n \times (k-1)$ matrix X_2 the observations on the other $k-1$ independent variables, u is a disturbance term, and β_1 and β_2 are a constant and a column vector, respectively, of coefficients to be estimated.

This relationship is not observed. Instead, we have:

$$y = (x_1 + e)\beta_1 + X_2\beta_2 + u, \tag{5}$$

where e is the vector of systematic error in x_1. Now partition $\beta^{*\prime} = [\beta_1{}^* \ \beta_2{}^{*\prime}]$, where $\beta_1{}^*$ is the first element of β^* (corresponding to x_1), and $\beta_2{}^*$ is the vector of remaining elements (corresponding to X_2). Suppose that e has the following form:

$$e = (x_1\beta_1 - x_1\beta_1{}^* + X_2\beta_2 - X_2\beta_2{}^*) / \beta_1{}^*. \tag{6}$$

Now e is just a linear combination of the independent variables x_1 and X_2. Hence from standard regression theory, its only effect on the asymptotic values (probability limits) of the coefficients is to transform them accordingly. That is, suppose that the original relationship is:

$$y = X\beta + u.$$

Now suppose that the matrix of independent variables is linearly transformed, which we can represent as postmultiplication of X by the nonsingular square matrix A. Then regression theory tells us that the new coefficients will be given by $A^{-1}\beta$. The new setup can be written as:

$$y = (XA)(A^{-1}\beta) + u.$$

The obvious fact here is that $(XA)(A^{-1}\beta) = X\beta$. Given the transformed data matrix XA, then, we can find the new coefficients simply by solving for constants which, when multiplied by XA, will equal $X\beta$.

In the systematic error problem, the estimated coefficients will be those values of β_1 and β_2 which, when substituted in the transformed regression (5), make (5) the same as the original regression (4). Elementary algebra will show that when e is defined as above, (4) and (5) are equivalent when β_1 and β_2 are replaced by $\beta_1{}^*$ and $\beta_2{}^*$, respectively. Thus, for any arbitrary coefficient vector β^*, there exists a systematic error structure (6) that will generate it. *This ends the proof.*

These developments make it clear just how dangerous to inference measurement error is. Tossing raw attitudinal measures of survey responses into regression equations or cross-tabulations is likely to lead to sensible conclusions only by chance. Correcting for measurement error in independent variables alone guarantees useful results.

A number of correction procedures for measurement error have been proposed. The first, and perhaps the most useful generally, is the set of techniques known as factor analysis or scaling. These methods all assume that the researcher has available several different measures of the same true underlying variables. Strong distributional assumptions are then added (e.g., all measures are normally distributed, the underlying true scores are normally distributed, the measurement errors are normally distributed, or even, quite commonly, all of the above). Estimates of the true variables are then produced.

A wide variety of models are available, suitable for continuous or discrete variables, one underlying dimension or many, and several different theories of how the scores were produced. (The most complete source remains Lord & Novick, 1968. See also Torgerson, 1958; and Rasch, 1980.) These theories constitute an impressive armamentarium, and they are far too little used and understood in political science. But we stand a great distance off from making a theoretically-informed choice among them. Do survey respondents obey the assumptions of any of these models, even approximately? The answer is surely no. If they do not, how much difference does it make? Brady (1981) has begun to explore factor analytic models with no assumptions about either the distributional form of the errors or about the functional form that relates factors to observed variates. A great deal more work of that kind will be necessary before one can have full faith in scaling and factor analytic methods. Intensive, theoretically-informed research on these techniques should have a prominent place among methodologists' priorities.

Scaling methods are not the sole route to correcting regressions for measurement error. Attractive and relatively simple correction procedures also exist, for example, when the error variances are known or when they are estimable (Johnston, 1972, pp. 281-291). Procedures for computing the standard errors of the adjusted coefficients have also been derived (Warren, White, & Fuller, 1974; for a cruder but simpler approach, see Achen, 1978). Sometimes, as with well-known scholastic tests like the SAT or GRE batteries, reliabilities have been published. In other cases, more elaborate estimation procedures are required.

In one case, political scientists have expended considerable effort to learn a set of error variances. Converse (1964) argued brilliantly that most individuals have little conception of political issues. As part of his evidence, he showed that a set of standard political attitude questions from the Michigan Survey Research Center's National Election Studies (NES) correlated quite poorly with themselves over a two-year interval, with Pearson's r's often no more than 0.4. Implicitly assuming that the reliability of the questions was 1.0, he said that responses with so little stability indicated a lack of real understanding. By contrast, individuals showed considerably more stable party identification, demonstrating that party loyalties were genuine in a way that issue preferences were not.

Converse added other evidence as well, such as the higher inter-item correlations for elites than for mass samples, but his study (and virtually all such elite-mass comparisons since) examined groups who were asked quite different questions. The elite questions were more precise, the mass items diffuse. Differences in the correlations would be expected on that basis alone. Similar objections apply to the comparison of elite over-time correlations with the corresponding mass correlations, as in Kinder (1983). For this reason, the stronger part of Converse's argument has always been the mass over-time correlations considered on their own.

Since Converse's time, a series of scholars have tried to learn the reliabilities of the items to correct his correlations for measurement error. Constructing careful measurement models and exploiting the three measurements in the NES 1965-58-60 panel study, they have derived reliabilities for the data Converse considered. The uniform conclusion of them all (Asher, 1974; Achen, 1975; Erikson, 1975; Dean & Moran, 1977; Jackson, 1979) has been that, apart from the party identification question, reliabilities were quite low, on the order of 0.5. Correcting for them typically brought the over-time attitude stabilities to 0.90 or more, meaning that most respondents were quite stable in their true opinions. Moreover, respondents' measurement error variances did not diminish much as they became better educated or more interested in politics, indicating that measurement error was not primarily a matter of misunderstanding the questions.

All of these revisionist studies require that the response uncertainties constitute "measurement error" in the statistical sense. That is, instability must be due to the questions, not to the respondents. Confidence on this point is necessary if the corrected correlations are to be meaningful.

Several arguments blaming the respondents rather than the questions have been developed. For example, it is sometimes said that if the measurement error corrections are accurate, then the populace is very sophisticated about politics—too sophisticated to be believable. Hence the measurement models must be false. This argument misses the point of the revisionist literature. Nothing in the high over-time correlations implies that the citizenry has a good grasp of political life. As noted repeatedly by the revisionists, opinions may be stable for all sorts of unimpressive reasons. H. L. Mencken could accept the measurement models without changing his estimate of American intelligence.

In a similar fashion, some scholars have wondered whether the questions could have been as badly worded as the measurement models seem to imply (Converse & Markus, 1979; Kinder, 1983). But again, nothing in the measurement literature implies anything of the kind. Rather, the low reliabilities may simply reflect the difficulty of eliciting certain kinds of attitudes; difficulties that no opinion researcher can escape. Even in an interview with a foreign policy expert, for example, learning his general orientation toward American interventionism will be no simple matter. Inquiring after his party identification will be far easier. There is no reason to believe that all opinions are equally complex, even for individuals who understand them thoroughly. Thus the fact that most opinion reliabilities are low, while the party identification reliability is not, carries no necessary implication that the opinion questions are badly worded or that one item is better written than another. Objections based on those assumptions attribute a theoretical content to the models which they do not contain.

Other objections to the measurement literature have been based on collateral evidence. Thus Converse and Markus (1979) have argued that in two-wave panel studies, attitudes toward a political candidate correlate over time in proportion to the candidate's visibility. Feelings about Edward Kennedy are more stable than those about Henry Jackson. Since the question wordings are essentially identical, differences in stability must be due to differences in comprehension. Thus low correlations show low knowledge. Converse and Markus conclude that something similar must be going on with the original Converse items, so that the measurement models were mistaken in finding that true policy opinions were relatively stable. In their view, "measurement error" consists primarily of misunderstanding.

Converse and Markus's argument is inventive but not convincing. Its logic is this:

1. Low over-time correlations in candidate preference items are due to a lack of understanding on the part of respondents.
2. The revisionists' models imply that candidate preference items correlate poorly over time solely because of random measurement error.
3. Therefore the measurement models are mistaken.

The conclusion here certainly follows if the two premises are correct, and Converse and Markus make a good case for the first premise. Unfortunately, they present no evidence at all for the second one. They apparently assume that the measurement models would give the same result for candidate preferences as for policy views, so that the low correlations would be due to random error in both cases. But it is by no means clear that this supposition is true. Why should the rather precise notion of candidate preference have the same measurement characteristics as the necessarily nebulous policy attitudes? Without the second premise, the issue is not joined. It does no harm to the case for stable policy attitudes to point out that voters sometimes fail to learn much about unsuccessful Presidential candidates.

A serious indictment of the measurement models using the Converse-Markus data must support the second premise above: that the candidate preferences behave as policy preferences do. Two pieces of evidence are needed. First, the measurement models would have to be shown to yield high over-time stabilities for both visible and obscure candidates. Secondly, the measurement variances would have to be demonstrated to vary little with differences in education, political interest, or other relevant proxies for sophistication. Absent either one of these results, the measurement models would imply precisely what Converse and Markus say is true, namely that the differences in stabilities for candidates reflect genuine differences in understanding. Without these two findings, then, Converse and his critics agree on the analysis of candidate preferences, and those data are simply irrelevant. Since Converse and Markus provide no evidence on either point, the measurement models escape unscathed.

One is left, then, with the conclusion that the revisionists' case has yet to be seriously challenged. No one has been able to formulate a measurement model for Converse's data which supports his analysis. This consensus might seem to give hope to political scientists facing error-laden data. Unfortunately, matters are not so simple. All the available measurement models are closely related to a model of D. Wiley and J. Wiley (1970), in which attitude change over time is supposed to follow a first-order autoregressive law with purely random disturbances. In the Wiley-Wiley (1970) model, let observed survey responses (standardized to mean zero) at times 1, 2, and 3 be denoted by x_1, x_2, and x_3, and let u_1 be the true attitude at time 1. Let v_1 and v_2 be random disturbances in true attitude, and e_1, e_2, and e_3 be measurement errors at times 1, 2, and 3, both sets of which are assumed to have mean zero and to be distributed independently of the u's. In addition, the errors are assumed to have constant variance over time. Finally, let r_1 and r_2 be constants. Then the equations for the three time periods in the panel study are:

$$\begin{aligned} x_1 &= u_1 + e_1 \\ x_2 &= r_1 u_1 + v_1 + e_2 \\ x_3 &= r_2(r_1 u_1 + v_1) + v_2 + e_3 \end{aligned} \tag{7}$$

The six variances and covariances may be used to solve for the six parameters, namely, r_1, r_2, and the variances of u_1, v_1, and v_2, plus the error variance.

Other models can be imagined. For example, one might want to make the measurement errors slightly systematic, so that they were correlated over time. This corresponds to the notion that people may answer the same question the same way on two different occasions at least partly because the same extraneous forces impinge on them at both periods. For instance, the same interviewer may have come around again, the same election-time propaganda may have appeared on the doorstep, and so on. In short, individuals may be stable from one election to the next for reasons having little to do with the persistence of attitudes in their own minds.

A model of this kind has been proposed by J. Wiley and M. Wiley (1974). It differs from the D. Wiley-J. Wiley model in just two respects: first, the autoregressive parameter is fixed equal over the second and third time periods ($r_1 = r_2$); and second, the errors of measurement are assumed to obey a first-order autoregressive scheme with lag coefficient s. Thus the equations are as follows:

$$\begin{aligned} x_1 &= u_1 + e_1 \\ x_2 &= r_1 u_1 + v_1 + s e_1 + e_2 \\ x_3 &= r_1(r_1 u_1 + v_1) + v_2 + s e_2 + e_3. \end{aligned} \tag{8}$$

The six parameters here are the same as before, except that s replaces r_2, and they may be estimated in the same way.

The results of applying these two models to two of the questions used by Converse are given in Table 1. Both models fit the data exactly, in the sense of reproducing the variance matrix with perfect accuracy. Yet their conclusions are astonishingly different. If the first model, which is standard in the literature, is correct, the over-time stabilities are very nearly 1.0, more than twice as large as the raw correlations, making Converse dead wrong. On the other hand, if the second model is the truth, most of the over-time correlation is due to correlations between error terms, and the true stabilities are about 0.2, just half the size of the raw coefficients. In that case, attitudes at one time period statistically explain almost nothing in attitudes two years later (in no case more than 7% of the variance), so that Converse was even more right than he imagined.

With present knowledge, no grounds exist for choosing between these competing models. The rather dreary conclusion follows that even in the most heavily studied case, no confidence about the error variances is possible. In less trodden fields, the situation is inevitably more desperate. Bluntly put, political scientists do not know what survey responses are measuring.

The fundamental problem here is lack of a mathematical theory of the survey response. As a recent report of the National Academy of Science (Turner & Martin, 1981) has emphasized, we have little or no rigorous understanding of the effects of question wording, question order, and response error. And if these difficulties are severe in the case of ordinary surveys, they become crippling in the case of important international studies like Almond and Verba (1965), as was emphasized by Scheuch (1968). Real progress waits on basic theoretical research by political methodologists interested in surveys.

Even without a theory, a great deal could be learned by extending our current techniques to lengthier panel studies. For example, with just four waves of interviews, the two Wiley-Wiley models could be combined and tested. The new model would subsume (7) and (8) and extend them to a fourth wave in the obvious way, allowing for first-order autoregressive struc-

TABLE 1
Overtime Correlations of Political Attitudes Corrected for Measurement Error Under Two Different Models
(Original Items Are from Converse, 1964)

	Guaranteed Jobs			Housing and Power		
	56-58	58-60	56-60	56-58	58-60	56-60
D. Wiley–J. Wiley Model (1970)						
raw r	.45	.47	.43	.37	.36	.37
corrected r	.95	.99	.94	.99	.99	.98
	56	58	60	56	58	60
reliability	.46	.49	.45	.38	.36	.38
	56-58	58-60	56-60	56-58	58-60	56-60
J. Wiley–M. Wiley Model (1974)						
raw r	.45	.47	.43	.37	.36	.37
corrected r	.25	.27	.07	.18	.18	.03
	56	58	60	56	58	60
reliability	.84	.85	.84	.88	.87	.88

tures in both true opinions and measurement errors, with the autoregressive parameters fixed equal over time for the errors. Thus the lag coefficients for true opinions would be r_1, r_2, and r_3, and the lag parameters for the errors would be s. Along with the measurement error variance and the variances of u_1, v_1, v_2, and v_3, there would be nine quantities to estimate, but ten variances and covariances in the data. Thus one could distinguish these two models (and many others) statistically and evaluate how well they fit. A better understanding of the Converse problem would result, and more importantly, clues would be provided as to how mathematical modeling of survey responses might proceed.

The urgency of theoretical research on the survey response bears emphasizing. Correcting for measurement error is the heart of any use of survey data. As yet, we simply do not know how to do that.

AGGREGATION BIAS

When political scientists doing empirical work are not using survey data, they are most often working with aggregate data of one sort or another: census information, voting returns by county or electoral district, crime statistics by state, and so on. These data are often cheaper to collect than survey information, and they extend back considerably further in time than do surveys. If one wants to know who voted for the Nazis, for instance, there is little alternative to working with aggregate elections returns. Moreover, even when surveys are feasible and affordable, aggregate information may be preferable. If one wants to study the effect of capital punishment on the murder rate, murder statistics are likely to yield more trustworthy conclusions than any possible survey. The answers to questions like "Have you ever murdered anyone?" or "Have you ever wanted to murder someone but were deterred by the thought of the electric chair?" would not be of much value.

Since Robinson (1951), social scientists have been painfully aware that ecological data were full of pitfalls. If one has a proposition cast at the individual level (e.g., Catholics were more likely to vote for the Nazis), voting returns from constituencies allow no direct test of it. Catholic constituencies might be more likely to vote Nazi even though individual Catholics are not. Robinson spoke of correlations, but his point is perfectly general. Goodman's (1953) "ecological regression" has the same problems.

Aggregation Bias as Contextual Effect

Two reactions are possible. In the first, one simply ignores the individual level of analysis. All explanations are cast at the constituency level, and the ecological inference problem disappears. In this model, single citizens do not vote; constituencies do. The statistical demand of this approach is that there be a unitary actor at the constituency level. Interactions among individuals do not suffice to create group-level explanations; processes of that kind can be formulated mathematically at the individual level (Erbring & Young, 1979). A legitimate "group mind" is needed to make the group-level explanation coherent.

Theories of group mind have always depended for their credibility on their imprecision. Stated clearly, they lack persuasive power. If the voting decision is truly made at the group level, one must come very near saying that the constituency rises sleepily on election day, rubs its eyes, takes thought, and says, "I imagine I will vote 37% Nazi." Researchers who find this a veridical theory of voting will be able to use logistic transformations of vote percentages —which destroy all chances of talking about the voters themselves (see Hannan, 1971, pp. 23-30)—and every other technique of quantitative analysis without qualm. No *statistical* difficulties occur.

For political scientists who cling to the conventional wisdom that individual citizens do the voting, more complex responses are needed. First, one may derive the conditions under which the ecological inferences will be the same as the individual-level ones. Theil (1954) considered the case of a single aggregate unit observed over time. His conclusions were extremely pessimistic. Roughly, every individual within the aggregate had to obey identical statistical laws or had to respond to changes in aggregate statistics in the same way. In practice, of course, neither is true.

Although no similar results are available for the cross-sectional case more common in political science, practical examples have been just as depressing as Theil's theoretical results. In Goodman's ecological regression model, for example, one might want to find the proportion of Democratic voters at time 1 who continued to vote Democratic at time 2, and the proportion of Republican voters at time 1 who switched to the Democrats at time 2. Call these proportions P and Q, respectively. Then if the fraction of Democratic votes at time 1 is denoted by D_1 and the same fraction at time 2 by D_2, and if everyone votes either Democratic or Republican, we have the simple accounting relationship:

$$D_2 = PD_1 + Q(1 - D_1). \tag{9}$$

If P and Q are constant across constituencies, or at least vary in ways that can be absorbed into a random disturbance term u, then (9) can be applied to a set of constituencies. Thus if i denotes the constituency number, we have from (9) in an obvious notation:

$$D_{2i} = Q + (P - Q)D_{1i} + u_i, \tag{10}$$

which has the form of a bivariate regression with intercept Q and slope $P - Q$. Thus one simply regresses the proportion Democratic at time 2 on the same proportion at time 1, and solves for P and Q from the resulting slope and intercept. It is quite easy to extend the model to multiple parties, abstention, controls for demographics, and so on, just by adding more terms to the regression and estimating additional regressions.

Computational experience with this model has been most unhappy. Commonly, the intercept is too small or even negative, and the slope is so large that the estimate of P is too high or even above one. Thus meaningless probabilities below zero and above one result. In fact, a substantial literature has developed to force the usual implausible estimates into the meaningful range from zero to one (e.g., Irwin & Meeter, 1969; Crewe & Payne, 1975). So far, the corrective treatments merely palliate the biases.

The standard interpretation of aggregation errors is to attribute them to "contextual effects." Ecological results are said to differ from individual findings because people influence each other. For example, heavily Democratic districts will produce Democrats who vote even more heavily Democratic than they otherwise would, simply because the environment drives them in that direction. Thus an individual-level—though, of course, not individualistic—explanation is used to account for the aggregate findings.

Much of this literature is not closely related to data, with predictable results. For example, literally dozens of articles have been written arguing that ecological regressions like Goodman's need a quadratic term. The argument goes that P and Q are not really constant across constituencies but instead depend on D_{1i}. Making P and Q linear functions of D_{1i}, and substituting in the original equation (10) yields a quadratic equation in D_{1i}. Unfortunately for this line of thinking, almost all ecological regressions exhibit strikingly linear relationships. The key to Goodman's woes must lie elsewhere.

The most specific and best grounded contextual argument is Butler and Stokes (1969, pp. 303-312). First, they give examples to show that the well-documented phenomenon of "uniform swing" in Britain is incompatible with Goodman's model. Their point holds generally. Formally, define uniform swing by the condition that for all i and some constant k, $D_{2i} = D_{1i} + k$. Taking expectations in (10) and rearranging:

$$k = Q + (P - Q - 1)D_{1i}. \tag{11}$$

Since k and Q are fixed, if (11) is to hold for all D_{1i}, we must have $P - Q - 1 = 0$, or:

$$P = Q + 1. \tag{12}$$

But then for any $Q > 0$, we have $P > 1$, which is impossible. Hence Goodman's model cannot be correct for any electoral system capable of uniform swing.

Butler and Stokes propose to modify Goodman by distinguishing two groups of voters. The first group attend to national media and thus respond to national forces, obeying Goodman's model. The second group are locally-oriented and take their cues from the partisan context of their local constituency. Butler and Stokes show that combinations of these two forces can produce uniform swing, and they give some evidence that in the 1964-1966 elections, the two groups exhibited the approximate qualitative behavior expected from the model.

Sad-to-say, the great difficulty of producing adequate models of contextual effects suitable to aggregate data can be illustrated even in this case. Formalize Butler and Stokes in the following way. Suppose that, in addition to the usual Goodman effects, a contextual force operates in proportion to the difference in the party strength. Following Butler-Stokes, let us measure this difference at the *previous* election. Then the contextual force is proportional to $D_{1i} - (1 - D_{1i})$, or $2D_{1i} - 1$. Letting its coefficient be G and adding this effect to the original Goodman equation (10), we have[1]:

$$D_{2i} = Q + (P - Q)D_{1i} + G(2D_{1i} - 1) + u_i,$$

or:

$$D_{2i}\ {}^{5}\ (Q - G) + (P - Q + 2G)D_{1i} + u_i. \tag{13}$$

At first glance, Butler and Stokes appear to have broken through to high ground. Goodman's model tends to produce intercept terms too small and slopes too large, and equation (13) seems to explain why. The intercept is depressed by the amount G; the slope is increased by $2G$. The equation is perfectly linear, just as Goodman and the data had assured us, but the coefficients must be interpreted differently than Goodman had supposed.

Unfortunately, the model does not hold up upon closer examination. Return again to an electoral system with uniform swing. Under the Butler-Stokes model, two statements can then be made. First, in a constituency in which the two parties each received 50% of the vote at time 1, no contextual effect operates at time 2. That is, if $D_{1i} = 0.50$, $2D_{1i} - 1 = 0$, then the original Goodman model must fit. Making use of uniform swing gives (11) with $D_1 = 0.5$:

$$k = Q + (P - Q - 1)0.5. \tag{14}$$

Next, we note that (13) must also hold when averaged over constituencies. Letting $\overline{D_1}$ be the mean vote at time 1, exploiting the fact of uniform swing, and taking expectations:

$$k = (Q - G) + (P - Q + 2G - 1)\overline{D_1}. \tag{15}$$

The problem now becomes apparent. In any given election, P, Q, and G are parameters determined by the contests at times 1 and 2, plus the nature of the contextual effect. In addition, $\overline{D_1}$ is fixed. Thus these four factors can be taken as given. Among them, they determine the size of the uniform swing. Unfortunately, they do so in two distinct ways, (14) and (15). In general, the two equations are not consistent.

As an example, consider two elections with $P = 0.8$, $Q = 0.3$, $G = 0.1$, and $\overline{D_1} = 0.4$. (Incidentally, though it is beside the point, these are all quite reasonable values that might be expected to occur in an actual election.) Then (14) gives a swing of 5%, while (15) implies an 8% swing. Thus the model contradicts itself. Butler and Stokes cannot cope with uniform swing any more than Goodman can.

The dilemma cannot be escaped by making contextual effects dependent on the vote at the *second* time period. Though this assumption makes more sense substantively, essentially the same problem occurs. In this case, the Goodman model must fit constituencies where the vote at time 2 is 50%, plus fit a modified version of equation (15). Consistency again occurs only by chance.

As this example makes clear, even our most sophisticated aggregate-level contextual models have important lacunae. Contextual effects simply are not well understood. In spite of an enormous literature extolling their importance, almost no one has suggested explicit models by which they might operate. The key exception is Erbring and Young (1979), an article that summarizes and eviscerates an enormous amount of careless thinking on this topic. Although entirely focused on individual-level data, in principle this article proposes for the first time the explicit statistical models of social interaction that contextual theories assume. Whether its very heavy data demands will be met in the near future is questionable. But success at the individual level might make possible an inference to the aggregate-level model that is so badly needed.

Whether a contextual effect is there to be found remains an open question. As Prysby (1976) notes, even the best arguments in its favor are deeply flawed, and their evidence is certainly consistent with its absence. Weatherford (1982) finds that most people do not talk politics with their neighbors in any case, which eliminates the usual mechanism for communicating contextual effects. Finally, in one of the most direct tests of the contextual hypothesis, Tate (1974) showed that a large number of contextual variables proved to be useless in predicting individual-level British voting behavior. In short, the form of contextual effects, their internal logic, and even whether they exist at all remain unknown. Serious theoretical research on deriving an aggregate-level contextual model from empirically-verified assumptions about individual-level interactions should have a high priority for students of social context.

Aggregate Bias as Specification Error

Since Hanushek, Jackson, and Kain (1974), many researchers have taken up the view that aggregation error occurs because of specification errors. On this view, contextual effects may or may not exist, but aggregation bias would be expected in any case, simply because erroneous equations never yield the right answer. Hence the task is to properly specify the regression equations so that bias disappears. Hanushek *et al.* show that Robinson's original example is very nearly corrected by adding variables to the equation to specify it more accurately.

Needless-to-say, better specifications are always welcome. But one can question whether this formulation resolves the theoretical question. "Specification error" is often taken to mean a deviation from the one true causal law. On this logic, aggregation bias occurs because some of the causally important variables are omitted from the equation. Adding them, it is said, eliminates the bias.

A moment's thought will show that in this form, eliminating specification error is a hopeless task. For we are not very likely ever to have perfect knowledge. Causally important variables are omitted from *every* social science regression, simply because not everything is measurable. If good estimates from aggregate data wait on specifications with all causal factors included, no progress can be hoped for in our lifetimes. More importantly, adding a variable here and there may make marginal improvements, but no guarantee exists that the bias is thereby reduced. Statistical theory demonstrates that the one true model is better than any other, but not much more. An equation missing several key causal factors is not necessarily improved by adding one of them; the biases in fact may get worse.

If on the other hand, one takes "proper specification" of a regression equation to mean that its independent variables are uncorrelated with the disturbance term (the conventional statistical definition), the notion that the absence of specification error implies the absence of aggregation bias is simply false. Take the case of Goodman's model. It is purely descriptive and hence perfectly well specified at the individual level. With individual level data, its independent variables are necessarily uncorrelated with its disturbance, and accurate estimates result. However, with aggregate data, it often gives meaningless results, even with large amounts of data. Thus proper specification in this sense is not the answer either.

We are left then with just one possibility. "Proper specification" might mean that at the aggregate level, the disturbances are uncorrelated with the independent variables. Now of course this does guarantee asymptotically accurate results. But what are we to make of it? What sort of modification of our specifications is called for? Our theoretical knowledge is entirely at the individual level, and we rarely understand much of the grouping procedure that created the geographic districts. The advice to seek proper specification, interpreted in this fashion, is either tautological (Avoid bias!) or else quite difficult to interpret. *What characteristics of the individual-level specifications lead to uncorrelated disturbances at the aggregate level?* That is the central question for this point of view, and its answer is unknown.

What makes the aggregation problem so severe, then, is this dilemma. Aggregate data are often superior to individual-level information, even when no contextual effect or group mind is at work. But they commonly lead to severe biases, of whose solution we understand almost nothing. For serial correlation, selection bias, underidentification, or the other difficulties routinely encountered in non-experimental work, every textbook carries straightforward counsel on eliminating the biases. One may not always be willing or able to take the advice, but there is never any doubt about what should be done. One looks in vain for similar instruction on aggregation. At this stage in our knowledge, the aggregation problem simply is in a class by itself. Clearly, intensive theoretical research on this problem deserves high priority in methodologists' research programs.

CONCLUSION

The state of our knowledge in political methodology gives little cause for self-congratulation. The two principal topics that have concerned us, measurement error and aggregation bias, remain both poorly formulated and even more poorly understood. Enough data has been collected that one could surely recognize a successful solution to either one if it were presented. But the anemic mathematics that has been applied to both tasks is not likely to produce one.

More than anything else, what would help are formal theories with measurement models built into them. These have been common in psychology (see, for example, Atkinson, Bower, & Crothers, 1965) and sociology (see Berger, Cohen, Snell, & Zelditch, 1962) for two decades now. Political science remains dominated by fact-free theory of the verbal or rational-choice variety. In a charming exception, Enelow and Hinich (1983) have recently attempted to link the spatial theory of voting to factor analytic models of public opinion. Work of that kind remains all too rare.

LISREL-type models (Jöreskog & Sörbom, 1979) also constitute a partial exception. These sophisticated structures offer the opportunity to mesh substantive specifications with measurement models. In a very general way, they represent the direction in which empirical work must move, and political scientists have not been slow to exploit them. But here again our theoretical limitations are painfully obvious. First of all, we often have no knowledge of either the substantive or the measurement model, and so assume linearity without further argument. Doing so may be better than ignoring measurement altogther, but it creates additional doubt in what is usually a complicated specification with enough credibility problems to go around. No LISREL model abolishes the theoretical questions discussed earlier.

Even ignoring these first difficulties, LISREL models raise many inferential questions. They assume normal distributions for every variable in the equations, including both measured and unmeasured variables, plus disturbances. Thus one dummy exogenous variable will violate the postulates. No doubt the consistency of the estimates will survive such minor transgressions (though this has not been proved, to my knowledge), but the standard errors of the coefficients will not. Thus even if all the other assumptions held, it would be a safe bet that every LISREL standard error published in a political science journal is erroneous. Perhaps jackknifing the estimates would help; again, no such result is known.

Other limitations of the standard LISREL model have become clear in applications. All equations are assumed linear. No interaction effects can be employed, no quadratic terms, no transformations of variables already used untransformed elsewhere in the model. Moreover, variables must be continuous. Dichotomous or polychotomous endogenous variables are beyond the capacity of the system (except in rather specialized cases, e.g., all exogenous variables normally distributed). Brady (1982) has investigated the extension of LISREL models to discrete data, but very little else has been accomplished in political science. Here as everywhere else, there is much theoretical work remaining.

The progress that might be expected from more explicit attention to methodological theory became clear in the recent debate on how economic conditions affect the vote (Kramer, 1971; Kinder & Kiewiet, 1979; and others). Initially, one side used over-time aggregate data which implied strong economic effects on the vote, and the other studied survey responses which showed none. Since neither side had a measurement model, contradiction and confusion were the inevitable result.

Happily, however, Kramer (1983) has produced formal statistical models of the theories on each side. He assumes that citizens respond politically to *government-induced* changes in their income, while researchers can observe (with either survey or aggregate data) only the *total* income change. Thus a measurement error is generated. As his main finding, Kramer shows that, as one would expect under his assumptions, aggregate-level data largely wash out the errors. Though not unbiased, they are greatly superior to the individual-level survey responses. In this case, then, aggregate data are to be preferred, an iconoclastic finding reminiscent of Grunfeld and Griliches (1960). Similarly, Kramer shows that if voters respond to the true overall state of the economy rather than their own personal situations ("sociotropic voting"), an interpretation seemingly supported by some of the cross-sectional survey data, then in fact those data are hopelessly biased. Because there is no variance in the state of the economy at a single time period, a cross-section contains no statistical information about sociotropic voting. Once again, aggregate data are superior.

Kramer's powerful argument poses a severe challenge to the conventional wisdom about aggregate data. At a minimum, it dramatically raises the level of the debate, suggesting that our lack of understanding of aggregation may not be so debilitating after all. Yet the force of his conclusions can be questioned. As Kramer himself notes, aggregate data are inadequate in principle to disentangle self-interested from sociotropic voting. Regardless of whether citizens respond to their own income changes or to changes in national income, the effect at the aggregate level is the same: national income changes predict the vote. What Kramer shows is that if we must choose between individual-level cross-section data with uncorrectable errors and over-time aggregate data with the same weaknesses, the latter are to be preferred in studying economic effects on the vote. But since the aggregate data cannot answer the central question, and probably have biases beyond those Kramer mentions[2], his result shows only that in the land of the blind, the one-eyed man is king. If depth perception is needed, it may be best to find a new king.

Kramer's findings do leave an alternative open to the devotees of individual-level data: the estimation of a substantively plausible model taking account of measurement error in the survey responses (both Kramer's kind of error and ordinary response error). In theory, truly adequate estimates could be produced from a single cross-section, at least if we relax Kramer's assumption that in sociotropic voting, everyone acts on the same view of the national economy. In practice, however, panel studies seem more likely to be helpful, not only because they generate real variation in the national state of the economy and thus escape Kramer's critique, but also because they contain repeated measures and lagged instrumental variables to control measurement error. A panel study with individual-level survey responses and adjustments for measurement error would be greatly superior to aggregate data, even under Kramer's postulates. Most importantly, it would distinguish sociotropic from individualistic voting, which aggregate data cannot hope to do. Of course, as noted earlier, correcting for measurement error in the survey responses will be no trivial affair. The plausible substantive assumptions and analytic techniques that would guarantee consistent estimates are not yet evident. Certainly, intriguing methodological challenges on both sides of this topic remain to be investigated. Kramer's work demonstrates just how great the rewards of progress on our principal agenda might be.

Mounting a sustained attack on such questions across political science may demand some changes in our work habits. Fundamental research must come to take priority, at least some of the time, over applied work. It should be possible to get funding, publish respectably, and make a career studying the principal agenda of political methodology. And to provide for the future, much better mathematical training will have to be provided at both the graduate and undergraduate level.

Basic methodological research will not be of general interest to the profession. Almost by definition, any work simple enough to be understood widely will not dent the problems. *Political Methodology,* if it is to keep its leadership position in the field, will become more specialized and technical. To some degree this has already occurred, just as it did in economics and psychology in an earlier era. So long as it does not lead to sterile theorizing, the growing separation is a sign of health and progress.

In sum, the tenor of this essay should be taken as optimistic rather than pessimistic. Certainly it provides no excuse to abandon quantitative work. One does not escape logical gaps in an argument by becoming less rigorous, or by abandoning logic entirely. We have no choice but to find out how people answer survey questions, how national and international agencies produce cross-national data, and what aggregation bias amounts to.

A beginning has been made. Political methodologists may still be rewarded more for "substantive" (i.e., merely applied) work than for "technical" (i.e., theoretical) contributions, but their numbers grow and the product improves. *Political Methodology* may look like *samizdat,* but its sophistication steadily distances it from the more prestigious journals. And even the fact that our theoretical failings have led to so many anomalies gives cause for hope. For if we can come to face our weaknesses, then in the manner of the alcoholic standing up for the first time to confess his drunkenness, a major step toward improvement may have been taken.

APPENDIX

This appendix gives a proof of the inconsistency result for multiple regression when a single independent variable is observed with error. The true relationship is assumed to be

$$y = X^*\beta + u, \tag{A1}$$

where y is an n-dimensional vector of observations on the dependent variable, X^* is an n $\times$ k matrix of observations on the fixed true values of the independent variables, β is a coefficient vector to be estimated, and u is a disturbance term whose elements are mutually independent. The usual regression assumptions are made: the disturbances are assumed to have mean zero, constant variance, and zero correlation with the true independent variables. That is, $E(u) = 0$, $E(uu') = \sigma_u^2 I$, and $E(X^{*\prime}u) = 0$.

All but one of the independent variables are observed without error. The first true independent variable, x_1^*, is not observed but is related to an observed variable, x_1, as follows:

$$x_1 = x_1^* + e_1, \tag{A2}$$

where x_1, x_1^*, and e_1 are each n-dimensional vectors, and where it is assumed that the elements of e_1 are mutually independent, and that they have mean zero, constant variance, and zero correlations with the true independent variables and the disturbances. Thus $E(e_1) = 0$, $E(e_1 e_1') = \sigma_e^2 I$, $E(X^{*\prime} e_1) = 0$, and $E(u' e_1) = 0$. Finally, to guarantee that certain probability limits exist, it will also be postulated that $\lim X^{*\prime} X^*/n = \Omega$, a constant positive definite matrix, and that the elements of u and e_1 have uniformly bounded absolute fourth moments.

Letting $E = [e_1\, 0 \ldots 0]$, it follows that $plim\, E'E/n = \Sigma$, a matrix which is zero everywhere except for its upper left corner element, which is σ_e^2. Now let β_1 be the first element of β and let X be the matrix of observed independent variables, so that $X = X^* + E$. Then write (1) as:

$$\begin{aligned} y &= X\beta + u - E\beta \\ &= X\beta + u - \beta_1 e_1. \end{aligned} \tag{A3}$$

Then the OLS estimate is:

$$\begin{aligned} \hat{\beta} &= (X'X)^{-1}(X'y) \\ &= \beta + (X'X/n)^{-1}(X^* + E)'(u - \beta_1 e_1)/n. \end{aligned} \tag{A4}$$

Thus:

$$plim\,(\hat{\beta}) = \beta + (\Omega + \Sigma)^{-1} \begin{bmatrix} -\beta_1 \sigma_e{}^2 \\ 0 \\ \cdot \\ \cdot \\ \cdot \\ 0 \end{bmatrix}. \tag{A5}$$

Now partition the matrix of observed independent variables as $X = [x_1\, X_2]$ and similarly $\beta' = [\beta_1\, \beta_2']$. Thus x_1 is the first independent variable and β_1 is its coefficient, while X_2 is the matrix of all the other independent variables and β_2 is its coefficient vector. Set $b = (X_2'X_2)^{-1} X_2' x_1^*$ and $s^2 = [x_1^{*\prime} x_1^* - x_1^{*\prime} X_2(X_2'X_2)^{-1} X_2' x_1^*]/n$. Hence b is just the coefficient vector when x_1^* is regressed on X_2, and s^2 is the corresponding residual variance. Now using standard results on partitioned inverses (e.g., Theil, 1971, p. 18), we find from (A5):

$$plim\,(\hat{\beta}_1) = \frac{s^2}{s^2 + \sigma_e^2}\, \beta_1. \tag{A6}$$

and

$$plim\,(\hat{\beta}_2) = \beta_2 + \frac{\sigma_e^2}{s^2 + \sigma_e^2}\,\beta_1 b. \tag{A7}$$

The last equation is a relationship between vectors. Taking any single line from it, say the jth, gives the result (3) in the text.

NOTES

1. To avoid unnecessary complexity, the division of the population into locally and nationally oriented groups has been suppressed. If these groups have constant proportions across constituencies, for example, (13) can easily be rewritten to reflect the fact that the contextual effect applies only to the former group and the Goodman model only to the latter. As written, (13) absorbs the proportionality weights for the two groups into the parameters P, Q, and G.
2. Another of Kramer's assumptions is that in expectation, all citizens respond identically to income changes (in dollars), meaning that their regression coefficients are all equal. This postulate allows him to escape aggregation bias (Theil, 1954). However, systematic variation in political responses to income change seems quite likely, even within the rational-choice framework Kramer adopts. Diminishing marginal utility of income would generate it, for example. Thus his aggregate level estimates probably have additional biases beyond those he considers. Some random coefficient models also generate biases in cross-sectional or even panel data, of course, so that the relative magnitudes of the biases need investigation. The advantage of individual-level data is that more powerful statistical techniques can eliminate those biases. With aggregate data, the parameters of coefficient variation are hopelessly underidentified.

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4

Self-Portrait: Profile of Political Scientists*

Naomi B. Lynn

This survey offers a current profile of the political science profession in the United States, drawing on a substantial number of studies that have been made over the years. What emerges can be termed a self-portrait. It is, however, no simple self-portrait of an individual against a neutral background. Rather it is a group picture which shows a profession resting on a somewhat fragmented foundation, grappling with current challenges--both individually and collectively--in order to build a viable future. Thus, the portrait is complex, more in the tradition of Hieronymus Bosch than Rembrandt.

At the 1978 meeting of the American Political Science Association Everett Carll Ladd, Jr. and Seymour Martin Lipset characterized political science as "a discipline in decline" (Ladd & Lipset, 1978, p. 21). This summary statement is an appropriate and proper backdrop for the self-portrait of the discipline. Decline, however, for political science is a new development; the profession has not suffered from any gradual, long-term loss of ideas or energy. On the contrary, the profession experienced recent periods of explosive growth. But the excitement of the intellectual currents and debates led us to ignore the impending demographic and economic forces that have shaped our current problems. The past half dozen years have been difficult ones for political science.

The Discipline's First Century Foundations: Some Broad Currents

Political science, as an organized academic discipline in American universities and colleges, was one of the products of the flowering of higher

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education in this country in the decades following the Civil War. In June of 1880 the School of Political Science was founded at Columbia University, under the leadership of John W. Burgess (Somit & Tannenhaus, 1978, p. 17). Interest in the subject grew at numerous other institutions; by December of 1903 the numbers of and the interactions between political scientists had reached the point that it was useful to establish the American Political Science Association. Thus our discipline is barely into its second century as an established field, while as an organized professional group we are an even younger phenomenon.

A Capsule Look at Some Major Intellectual Issues. The century following 1880 had many turbulent aspects as the field sought to define itself and to select its objectives and methods of reaching them. Lipset (1969, p. vii) has pointed out that the term *political science,* as originally used, meant what would now be called policy science; that is, it initially emphasized the structure and function of government and the concerns of authorities. Only later did political science also stress systematic generalizations based on empirical data and statistical analyses of the sort now familiar in all social sciences. This fluctuation of emphasis between the study of the polity on the one hand and the relationship of the polity to the society in which it operates on the other has been a main feature of the discipline. Such fluctuation can be frustrating for those who feel a need for an integrating model of the field. Greenstein and Polsby (1975) reflect on this when they observe that "Early in his career, the fledgling political scientist learns that his discipline is ill-defined, amorphous, and heterogeneous" (p. v).

The discipline of political science has problems: political scientists per se are highly differentiated from political philosophers on the one hand and pure mathematicians who may handle political data on the other; the discipline lacks a central paradigm and is unlikely to get one soon; political science relies mainly on conceptual schemes from such disciplines as economics, sociology, and psychology; there is a notable current lack of methodological concerns; political cleavages of the late 1960's and 1970's have left scars on the discipline; and finally, economic and demographic factors have adversely affected the environment within which political scientists work. We may draw, however, some comfort from a brief review of some of our field's past challenges.

One of the pioneer scholars, Charles E. Merriam of the University of Chicago, sought in the early 1920's to move toward a "science of politics" (Somit & Tannenhaus, 1967, p. 87). He wanted more scientific rigor, and his efforts led to the establishment in the American Political Science Association of a Committee on Political Research. (Dwight Waldo, 1975, p. 48 traces to Merriam's "school" many leaders in the behavioral movement of later decades.) In contrast, Thomas H. Reed of Harvard argued that the primary responsibility of political science was to educate citizens and to prepare students for careers in government. As Somit and Tannenhaus (1967) note "Though neither Merriam nor Reed captured the Holy Land, or came very close to it, both crusades left their mark on the discipline" (p. 88).

After the second World War, with the advent of computers, "behavioralism" became the dominant intellectual force in political science. Behavioralists argued for a more scientific approach to the study of politics. They borrowed many of their models from the physical and natural sciences. By the 1960's the behavioral movement made extensive use of the sort of scientific approaches and systematic tools that Merriam had urged. It appeared that if behavioralism delivered on its major promises, it might become the "predominant paradigm" of political science (Somit & Tannenhaus, 1967, p. 210). This did not occur.

A survey of members of the American Political Science Association indicates that while behavioralism made important contributions, it did not establish hegemony; indeed, it appears that there is no consensus of commitment to it, nor great certainty about the meaning of the term (Roettger, 1978, p. 10). In part this reflects the deep doubt of most political scientists that the field has the capacity to become "scientific" in the sense of the natural sciences (Roettger, 1978, p. 14). More seriously behavioralism has been accused of developing "a number of blind and unquestioned ideological biases . . . that followed almost directly from the dominant belief that facts could and should be separated entirely from values, and that followed from the criterion that rigor always comes before relevance in questions of good political science" (Lowi, 1972, p. 14).

By the early 1970's the term behavioralism was often used in contradistinction to post-behavioralism (see Graham & Carey, 1972). The latter embodied the value orientations that were associated with the counterculture and the New Left (Waldo, 1975, pp. 113, 114). Post-behavioralism also incorporated other points, as David Easton observed in his 1969 presidential address to the APSA: Substance must precede technique; behavioral research must not lose touch with reality; knowledge must be implemented (Easton, 1969, p. 1052). In summary, post-behavioralism "supports and extends behavioral methods and techniques by seeking to make their substantive implications more cogent for the problems of our times" (Easton, 1969, p. 1061). Like the counterculture post-behavioralism left a noticeable mark, but it did not produce enduring basic changes in political science's intellectual and methodological underpinnings.

As political science ended its first century, it became increasingly clear that the search for a single paradigm was futile. Rather, each sub-specialty searches for its own unifying model. As it does so, it tries to incorporate appropriate scientific methodology, but it also seeks to discover meanings that "science" alone may miss (Baum, Griffiths, Mathews, & Scherruble, 1976, pp. 915-917). Baum *et al.* points out the need for the latter with a quotation from Charles Darwin's *Autobiography:*

> But for many years I cannot endure to read a line of poetry. My mind seems to have become a kind of machine for grinding laws out of a large collection of facts, but why this should have caused the atrophy of that part of the brain on which tastes depend, I cannot conceive. The loss of these tastes is a loss of happiness, and may possibly be injurious to the intellect, and more probably to the moral character, by enfeebling the emotional part of our nature (p. 917).

Other intellectual issues are also unresolved in the discipline. The dispute between rationalism and anti-rationalism is a notable example. Essentially rationalism contends that people are rational and seek to maximize stated goals in a calculating manner; this is the approach of economics. According to this formulation the bureaucrat seeks to maximize the budget, the voter tries to maximize expected utility, and the analyst attempts to formulate theories of collective choice (Barry, 1970; Frolich & Oppenheimer, 1978; and Goodin, 1976). The anti-rationalist view is more social-psychological in nature. It points out that irrationality is an inescapable feature of social experience and that feelings and needs defy rational examination (see Glass, 1978, pp. 63-92 in Frolich and Oppenheimer).

A parallel dispute arises over whether political scientists should seek to build deductive or inductive theories, or both. Similarly there is disagreement between some who downplay the role of the study of classical political philosophy, and others who consider that these roots are our rich foundation that should not be ignored.

Two decades of intellectual ferment have led to productive and exciting work. They have also exacted a price as David Finley (1982) points out:

> The hiatus of the methodological debates, squabbles and cabals of the 1960's, championing or reviling symbols of behavioralism and then post-behavioralism—essential as the underlying issues have been and remain—did forestall a lot of the substantive progress that the outside world (and governments and private foundations that want a product) had been encouraged to expect from the discipline. That generally undermined the popular respect for political science among important external constituencies on whom we still depend. The historical fact is a part of diagnosing the condition of the profession today. . . . We *do* suffer from the absence of an agreed paradigm, even if that absence is inevitable. (Comment made as panel member, APSA convention, Denver, 1982)

Indicators of Excellence. Political science is taught and learned at many hundreds of American colleges and universities. Political science research and writing have produced a voluminous literature. As in all academic fields, some schools, journals and individuals have achieved special prominence. The work that carries the mark of special excellence forms part of our common foundation.

Over the years there have been many ratings of the "top departments" in virtually all areas. Such ratings are subject to inevitable criticisms, chief among them that the underlying validity of the reputational perceptions on which most are based are doubtful. All measures are necessarily symptomatic, and they should not be taken too seriously. They seek signs which may be reasonable surrogates for excellence, but which are not excellence per se. Somit and Tannenhaus (1967), in their monumental study of our field, recognize that, "Full many a flower is born to blush unseen." They go on, however, to publish a comparative ranking shown here as part of an interesting 40-year comparison study in Table 1. These data show great consistency, even though some schools have gained (e.g. Yale) and some lost (e.g. Illinois) standing over the years. While a few position shifts have occurred, the striking fact that emerges is the stability in these ratings over a 57-year period.

This is especially significant when one observes that no two studies used exactly the same criteria and questions.

John S. Robey (1979) made a "productivity" study of departments that considered, not reputations, but publications by departments' faculty. This is shown in Table 2. Michigan is first in productivity—above its rating in the other studies. Ranked second through fifth are Kentucky, Florida State, Michigan State and Georgia, none of whom ranked in any of the reputational studies. While one would not want to claim that counting journal articles in the *American Political Science Review* and six regional journals is an ultimate criterion, it does suggest that excellence is more widely distributed than some suppose. It must, of course, be noted that many leading scholars publish most of their articles in books because they are under pressure to do so by people with project and/or conference money. (Robey did not count books, even highly important ones; he also did not control his numbers for department size.)

In a 1982 study McCormick and Bernick found results generally consistent with the earlier studies cited above. They controlled, however, for two things: the size of the authors' current departments and the number of recent graduates produced at the Ph.D. granting institutions (see Table 3).

The identification of prestigious journals is shown in Table 4. Data come from surveys from Somit and Tannenhaus with a later update by Roettger. The main change shown is the emergence of the *American Journal of Political Science* to "second tier" status, as it moved from its former regional (Midwest) identification (Roettger, 1978, p. 19).

The identification of the most outstanding contributors to the field of political science can be done both by reputation and by a count of others' citations to their work. Table 5 shows the results of Roettger's 1978 reputational study. Table 6 was compiled by John S. Robey; it is based on 1970-1979 citations. Also shown in this table are the schools where these scholars received their Ph.D.s and where they teach. It is noteworthy that the "elite" universities identified by the early reputational studies again show up prominently.

THE PROFESSION'S JOB MARKET

A full picture of the job market would have to include the observation that there are many political scientists among the tenured faculty at soundly financed universities. But, just as it is unemployment more than employment that reflects the economy's health, so our profession's economic situation is heavily influenced by the problem-ridden segments of the job market.

The Stagnation of Demand. The demand for academic political scientists is related significantly to the number of potential students who are able to pursue higher education. From the middle 1940's to the early 1960's a "baby boom" occurred. This produced a large number of college students from the 1960's to the early 1980's. There was general prosperity (with notable exception periods such as 1973-1975), and government-sponsored financial aid pro-

TABLE 1

Longitudinal Ranking of Graduate Departments of Political Science: 1925, 1957, 1963, 1964,* 1975-1976, 1982

1925 (Hughes)		1957 (Keniston)		1963 (Somit-Tannenhaus)	
1	Harvard	1	Harvard	1	Harvard
2	Chicago	2	Chicago	2	Yale
3	Columbia	3	California (Berkeley)	3	California (Berkeley)
4	Wisconsin	4	Columbia	4	Chicago
5	Illinois	5	Princeton	5	Princeton
6	Michigan	6	Michigan	6	Columbia
7	Princeton	7	Yale	7	Michigan
8	Johns Hopkins	8	Wisconsin	8.5	Stanford
9.5	Iowa	9	Minnesota	8.5	Wisconsin
9.5	Pennsylvania	10	Cornell	10.5	California (Los Angeles)
11	California (Berkeley)	11	Illinois	10.5	Cornell
		12	California (Los Angeles)	12	Johns Hopkins
		13	Stanford	13	Northwestern
		14	Johns Hopkins	14	Indiana
		15	Duke	15	Illinois
				16	Minnesota
				17	North Carolina
				18.5	Duke
				18.5	Syracuse
				20	Pennsylvania

grams multiplied; thus the market demand was good. The birth rate peaked in 1957, so by 1975 the number of potential freshmen started downward. In 1973-1974 there were 291 new positons for political scientists; by 1974-1975 there were only 239 (Mann, 1976, p. 412).

In the 1980's the smaller number of college-age people and the lessened amount of financial aid create a market that can optimistically be called stagnant. A moderate recovery of the birth rate points toward some improvement by the year 2000, especially if prosperity then prevails.

The demand for academic political scientists affects the whole field, including the best paid tenured professors. In the 1960's the strong demand helped attract large numbers of highly talented students into undergraduate and graduate programs. Those with intellectual interest and high ability in political science could afford to pursue their interest, confident that with a Ph.D. they would be eagerly sought in the job market. We will never know exactly how many of the potential top contributors to our field will never enter it because of the present low level of demand, but surveys (e.g. Ladd & Lipset, 1978, pp. 9-12) suggest that it is a substantial number.

Doctoral Output. In most years of the mid-1970's, there were about 1000 firm candidates for academic positions (Mann, 1976, p. 413). This reflected in large part the dramatic increase in doctoral output that took place after 1960.

TABLE 1 (continued)

1964 (Cartter)		1975-76 (Roettger)		1982 (Jones, Lindzey and Coggeshall)	
1	Yale	1	Yale	1	Yale
2	Harvard	2	Harvard	2.5	California (Berkeley)
3	California (Berkeley)	3	California (Berkeley)	2.5	Harvard
4	Chicago	4	Chicago	4.5	Chicago
5	Columbia	5	Michigan	4.5	Michigan
6	Princeton	6	Stanford	6	M.I.T.
7.5	M.I.T.*	7	Princeton	7.5	Stanford
7.5	Wisconsin	8	Wisconsin	7.5	Wisconsin
9	Stanford	9	North Carolina	9	Princeton
10	Michigan	10	Minnesota	10.5	Minnesota
11	Cornell	11.5	California (Los Angeles)	10.5	Cornell
12	Northwestern	11.5	Johns Hopkins	12	Rochester
13	California (Los Angeles)	13	Northwestern	13.33	Columbia
14	Indiana	14	Columbia	13.33	North Carolina
15	North Carolina	15	Cornell	13.33	Northwestern
16	Minnesota			16.5	Indiana
17	Illinois			16.5	California (Los Angeles)
18	Johns Hopkins			18	Duke
19	Duke			19.33	Illinois
20	Syracuse			19.33	Ohio State
				19.33	Washington (St. Louis)

Source: 1925-1964 data taken directly from Albert Somit and Joseph Tannenhaus (1967), *The Development of Political Science: From Burgess to Behavioralism,* Boston: Allyn and Bacon, p. 164. 1975-76 data are from Walter B. Roettger (1978), "The Discipline: What's Wrong, and Who Cares?" Paper presented at the Annual Meeting of the American Political Science Association, New York City. 1982 data are from Lyle V. Jones, Gardner Lindzey and Porter E. Coggeshall (Eds.), *An Assessment of Research-Doctorate Programs in the United States: Social and Behavioral Sciences,* Washington, D.C.: National Academy Press, 1982.

*M.I.T. was not included in the 1925, 1957 and 1963 studies.

Table 7 shows the number of American doctorates produced in political science. The 1960-1970 production was absorbed fairly easily by the "baby-boom" induced demand, but the burgeoning output of the 1970's swamped the market. The post-behavioralists of the 1970's urged relevance and future orientation in their writings; perhaps too little attention was paid to these points in our graduate advising. By the last half of the 1970's the production of Ph.D.s started to slow down. Especially important was the dramatic reduction in the number of new Ph.D. students (see Table 8). Even with these reductions, there were still more new Ph.D.s than the academic market could absorb.

TABLE 2

Political Science Departments:
Top 20 Departments Ranked by Productivity

1. Michigan	11. Minnesota
2. Kentucky	12. Texas
3. Florida State	13. Arizona
4. Michigan State	14. Harvard
5. Georgia	15. California (Berkeley)
6. Iowa	16. Rochester
7. Wisconsin	17. Houston
8. Massachusetts	18. North Carolina
9. Ohio State	19. California (Los Angeles)
10. Indiana	20. Yale

Source: John S. Robey. Political Science Departments: Reputations Versus Productivity, *PS*, 1979, *12*, p. 205.

Law schools apparently attracted many of the stronger students of the late 1970's who might have sought Ph.D.s in political science a few years earlier (Ladd & Lipset, 1978, p. 9). The growth in Masters in Public Administration programs also drained off some of the bright career-oriented students. About a third of political science departments have reported a decline in the quality of new Ph.D.s (Sheilah Mann, 1982, p. 91). Among sub-fields there have been moderate gains in the number of students in public administration and public policy as areas of concentration, and a decline in the students choosing comparative politics and political theory (Sheilah Mann, 1982, p. 91).

The Pay Record and Outlook. Academic folklore has long accepted as an enduring truth that there was low faculty pay in the field of political science. Thus it was surprising when Ladd and Lipset, using 1969 survey data, found that the pay of political scientists was relatively high; only law and medicine did better in the *elite* schools (Ladd & Lipset, 1974, pp. 21, 22). Salaries in political science relative to other fields dropped from 1969 to 1977, although similar drops occurred in other social sciences (Ladd & Lipset, 1978, p. 6).

The most recent expectation has been that incomes, adjusted for inflation, will drop in the future (Walker, 1978, p. 484). Given the imbalance between the number of openings and the number of people seeking academic positions, it could hardly be otherwise. The effects of low starting pay extend to all faculty ranks, as hard-pressed institutions are forced by state budget crises to economize. The outlook, even for those who have tenured jobs, is economically bleak. And since the median age of political science faculty was 37 in 1971 (Baker, 1971, p. 34), and it only rose to 42 by 1980 (Lane, 1982, p. 52), many of us could face decades of economic hardship.

In addition to the economic toll there are side effects which drastically alter professional and interpersonal relations in faculty departments.

TABLE 3

Comparative Rankings of Political Science Departments by Reputation and Alternate Standardized Measures of Productivity

Reputational Rankings[a]	Graduate-Training Rankings I[b]	Graduate Training Rankings II[c]	Affiliation Rankings[d]	Affiliation Rankings (1974-1978 data)[e]
1. Yale	1. Iowa	1. Rochester	1. Florida Atlantic	1. Carnegie-Mellon
2. Harvard	2. North Carolina	2. Washington-	2. Carnegie-Mellon	2. Michigan State
3. Berkeley	3. Vanderbilt	St. Louis	3. Kentucky	3. Kentucky
4. Chicago	4. Michigan State	3. Kentucky	4. Emory	4. Iowa
5. Michigan	5. Syracuse	4. Stanford	5. Rochester	5. Virginia
6.5. MIT	6. Yale	5. Vanderbilt	6. Florida	6. Rochester
6.5. Stanford	7. Rochester	6. Brown	7. Iowa	7. Ohio State
8. Wisconsin	8. Kentucky	7. Michigan State	8. California-Riverside	8. Houston
9. Princeton	9. Minnesota	8. Boston College	9. Michigan State	9. USC
10. North Carolina	10. Duke	9. Yale	10. Georgia	10. Wisconsin-Milwaukee
11. Columbia	11. Stanford	10. North Carolina	11. Cal. Tech.	11. Florida
12.5. UCLA	12. Illinois	11. Duke	12. Ohio State	12. California-Riverside
12.5. Minnesota	13. Princeton	12. Tulane	13. Stanford	13. Wisconsin
14.25. Cornell	14. Wisconsin	13. Georgetown	14. Minnesota	14. Michigan
14.25. Indiana	15. Tulane	14. Minnesota	15. California-Irvine	15. Minnesota
14.25. Northwestern	16. Berkeley	15. Michigan	16.5. Arizona	16. Texas Tech
14.25. Rochester	17. Michigan	16. New School	16.5. Cincinnati	17. Duke
18.5. Iowa	18. Chicago	17. Case Western	18. Vanderbilt	18. Rice
18.5. Oregon	19. Harvard	18. Houston	19. Michigan	19. Tulane
20.33. Illinois	20. Florida	19. Pennsylvania	20. Massachusetts	20. Arizona
20.33. Johns Hopkins		20. Ohio State		
20.33. Washington-				
St. Louis				

TABLE 3 (continued)

Source: Joseph M. McCormick and E. Lee Bernick. Graduate Training and Productivity: A Look at Who Publishes, *Journal of Politics*, 1982, *24*, 212-227.

[a]The reputational rankings are drawn from David R. Morgan and Michael R. Fitzgerald, Recognition and Production Among American Political Science Departments. *Western Political Quarterly*, September 1977, *30:* 348. The numbering for tied ranks has been changed slightly to conform with the convention and in other tied rankings.

[b]To obtain these graduate training ranks, the weighted department scores were standardized by the number of recent graduates. The figure used for each department was the average number reported in the *Guide to Graduate Study in Political Science* for the 1977 through the 1979 editions. Since the figure reported in each edition of the *Guide* is in itself averaged over the past three years, the figure ultimately employed in the analysis tends to cover the years of our study. [After controlling for number of graduates in Ph.D. programs, the authors weighted the journal articles on the basis of the estimated quality of the journal: *APSR* as 1.0, *JP* as .957, *AJPS* as .943, *Polity* as .843 and the *WPQ* as .829.]

[c]To obtain these graduate rankings, the weighted department scores were divided by the number of political scientists in the profession who received their graduate training from that institution (as determined by our systematic sample of the discipline).

[d]These standardized rankings come from Robey, "Political Science Department."

[e]To obtain these rankings, the weighted present affiliation scores were divided by the number of faculty members in a department. The figure used was the average of the number reported for 1976 and 1977 in the *Guide to Graduate Study in Political Science* and the 1978 figures from Robey, "Political Science Departments."

TABLE 4

Journal Ranking 1963 and 1976

Journal	1976 Rank	1976 Index	1963 Rank	1963 Index
American Political Science Review	1	2.75	1	2.78
Journal of Politics	2	2.42	3	2.31
World Politics	3	2.40	2	2.32
American Journal of Political Science	4	2.25	9	1.89
Public Administration Review	5	1.96	7	1.99
Political Science Quarterly	6	1.94	4	2.07
Public Opinion Quarterly	7	1.90	8	1.93
Administrative Science Quarterly	8	1.82	5	2.01
American Behavioral Scientist	9	1.84	10	1.73
Western Political Quarterly	10	1.81	6	2.00

Source: Table compiled by Walter B. Roettger. 1976 data were from a random sample drawn from APSA's Directory of Members; 1963 data from Somit and Tannenhaus, *American Political Science: A Profile of a Discipline.* See Walter B. Roettger, "The Discipline: What's Right, What's Wrong, and Who Cares?" Paper presented at the 1978 Annual Meeting of the American Political Science Association.

Caplow's *The Academic Marketplace* (1958) notes schisms prevailing in higher education in the 1950s: young turks vs. elderstatesmen; teachers vs. researchers, generalists vs. specialists; conservatives vs. liberals; pro-administration vs. anti-administration; humanists vs. scientists; and inbred vs. outbred. To these we would have to add—perhaps at the top of the list—tenured vs. non-tenured. It is not unfair to say that there are three distint classes of political scientists; tenured, those with hope of getting tenured, and those with little or no hope of getting tenure. This situation introduces a new stress on hierarchy within departments. Organizational hierarchy involves at least three modes of unequal allocation of resources: inequality in security, inequality in perceived punishment for deviance, and inequality of authority. In all cases non-tenured persons are at a disadvantage and those "without hope" are uniquely so. The result is an atmosphere scarcely conducive to stimulating the free exchange of ideas which traditionally has characterized the academic arena.

Academic and Non-Academic Prospects. Academics have always been the majority among political scientists. A 1970 National Science Foundation study showed the following occupational breakdown of political science Ph.D.s (quoted in Waldo, 1975, p. 120):

Academics	76.9
Business	1.8
Federal Government	5.4
Military	1.4
State and Local Government	3.5

TABLE 5

Ranking of Significant Contributors: A Longitudinal Perspective[a]

Pre-1945[b]		1945-1960		1960-1970		1970-1976	
Rank	Name	Rank	Name	Rank	Name	Rank	Name
1	Merriam	1	Key (35%)	1	Dahl (40%)	1	Lowi (18%)
2	Lasswell	2	Lasswell (32%)	2	Easton (19%)	2	Wildavsky (10%)
3	White	3	Dahl (20%)	3	SRC Group[c] (18%)	3	Dye (9%)
4	Beard	4	Easton (18%)	4	Deutsch (17%)	4	Dahl (8%)
5	Corwin	5	Morgenthau (18%)	5	Almond (16%)	5	Huntington (7%)
6	Bentley	6	Truman (16%)	6	Wildavsky (7%)	7	SRC Group[c] (6%)
7	Wilson	7	Strauss (8%)	7	Lowi (4%)	7	Verba (6%)
8	Herring	8.5	Deutsch (6%)	9	Lipset (4%)	7	Sharkansky (6%)
9	Wright	8.5	Simon (6%)	9	Wolin (4%)	10.5	Barber, Deutsch, Left Radicals[d], Riker
10	Ogg	10.5	Friedrich (5%)	9	Huntington (4%)		
		10.5	Schattschneider (5%)				

Source: Compiled by Walter B. Roettger, Strata and Stability: Reputations of American Political Scientists. *PS*, 1978, *11:* 9.

[a]Figures in parentheses represent the percentages of respondents designating the contributor. Sample size for 1945-1960 was 181; for 1960-1970, 179; and for 1970-1976, 113. The variation between periods (and the departure from the overall response level) is due to the failure of all respondents to designate significant contributors in each period.

[b]Taken from Somit and Tannenhaus, *American Political Science: A Profile of a Discipline*, New York: Atherton Press, 1964, p. 66.

[c]The "SRC Group" consists of Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes. Mention of one or more of these persons was coded as "SRC Group."

[d]The "Left Radicals" include: Ira Katznelson, Herbert Marcuse, Ralph Miliband, C. Wright Mills, James O'Connor, and Bertell Ollman. Mention of one or more of these persons was coded as "Left Radicals."

TABLE 6

Rank Order of 20 "Most Significant Political Scientist Contributors" by Number of Citations 1970-79, Where They Received Degree and Where They Teach

Name	Number of Citations	Ph.D.	Teaching
1. Seymour Martin Lipset	3425	Columbia	Stanford
2. Herbert Simon	3425	Chicago	Carnegie-Mellon
3. Robert Dahl	2235	Yale	Yale
4. Angus Campbell	2184	Stanford	Michigan
5. Karl Deutsch	1870	Harvard	Harvard
6. Gabriel Almond	1799	Chicago	Stanford
7. David Easton	1644	Harvard	Chicago
8. Samuel Huntington	1511	Harvard	Harvard
9. Harold Lasswell	1410	Chicago	Yale
10. Philip Converse	1282	Michigan	Michigan
11. V. O. Key	1110	Chicago	Harvard
12. Theodore Lowi	913	Yale	Cornell
13. Charles Lindblom	958	Chicago	Harvard
14. Robert Lane	782	Harvard	Yale
15. Aaron Wildavsky	766	Yale	California, Berkeley
16. W. H. Riker	759	Harvard	Rochester
17. Thomas R. Dye	709	Pennsylvania	Florida State
18. Carl J. Friedrich	701	Heidelberg	Harvard
19. Sidney Verba	645	Princeton	Harvard
20. Ira Sharkansky	589	Wisconsin	Wisconsin

Source: Compiled from John S. Robey, Reputation vs. Citations: Who Are the Top Scholars in Political Science. *PS*, 1982, *15:* 200. Biographical data from *Who's Who in America.* Herbert Marcuse and C. Wright Mills were dropped from Robey's list because they were not political scientists.

TABLE 7

Production of Ph.D.s in Political Science

Year	Number of Doctorates Awarded in Political Science
1880-1960	3700
1960-1970	3836
1970-1980	8519

Source: 1880-1970 compiled from Walter B. Roettger. "I Never Promised You a Rose Garden: Career Satisfaction in an Age of Uncertainty." Paper presented at the Iowa Conference of Political Science, 1977. 1970-1980 data compiled from Sheilah Mann. Placement of Political Scientists, 1980-1981. *PS*, 1982, *15:* 85.

TABLE 8

Supply of Political Scientists, 1969-1981

New Students Beginning Ph.D. Study in Political Science		Graduate Student Enrollments in Ph.D. Programs in Political Science		Ph.D.s Awarded
Fall, 1981	1,042	1981-82	5,491	679
Fall, 1980	1,068	1980-81	5,756	729
Fall, 1979	1,100	1979-80	5,888	766
Fall, 1978	1,051	1978-79	5,742	851
Fall, 1977	1,182	1977-78	5,737	881
Fall, 1976	1,064	1976-77	5,462	885
Fall, 1975	1,174	1975-76	6,150	862
Fall, 1974	1,443	1974-75	6,150	907
Fall, 1973	1,414	1973-74	6,450	906
Fall, 1972	1,576	1972-73	*	811
Fall, 1971	1,695	1971-72	*	821
Fall, 1970	2,138	1970-71	*	634
Fall, 1969	2,487	1969-70	*	559

Source: Data obtained from "Political Science Degrees Awarded and Graduate Students Enrolled: 1982 Update." *PS*, 1982, *15:* 459-460.

*Not available.

The remainder were presumably retired, unemployed, or not in the labor force. This suggests that, given a good job market, such as that of 1970, the great majority of political scientists prefer an academic position.

Even in recent job markets most of those who get positions find them in universities and colleges. In 1981 only 18 percent of Ph.D. placements were non-academic (less than in 1979 and 1980). The proportion of 1981 placements to temporary positions was 28 percent, down from all earlier surveys. The placement success percentage--81 percent--was the highest since 1974. The reason for this may well lie in the relatively low number of candidates, 697 (Sheilah Mann, 1982, p. 86).

Success in placement varies by sub-fields. Public administration, public policy and American government Ph.D.s fare better than those in comparative politics, international relations, and political philosophy (Sheilah Mann, 1982, p. 89). A shift in placement opportunity has resulted in the need for the Ph.D. to be completed; ABDs found many placements in the early 1970s, but later in the decade the completion of the degree was required for the majority of new positions (Mann, 1978, p. 27).

Some have suggested that the skills of political scientists fit them well for many non-academic positions. About one fifth of Ph.D. programs—particularly those of smaller departments—have sought to adapt themselves and prepare students for non-academic positions (Sheilah Mann, 1982, p. 85). Also, about 150 to 175 Ph.D.s (2 percent of the total) leave academia annually for outside jobs. The 1981 placement data suggest that non-academic placements are not becoming the solution to our profession's market problem. A few want such positions; others, such as those who do not gain tenure, must seek them. In some fields, such as government and lobbying, the political science degree is, of course, a distinct asset. For most people starting post-baccalaureate study, however, the alternative to being a political science Ph.D. teaching in a university is not being a political science Ph.D. at all. It is setting out for another educational and career objective in the beginning.

VIEWPOINTS

The views of political scientists have been noticeably different on the average from those of other academics. Certainly we are not a representative sample of the nation's population. We are drawn disproportionately from the middle class and almost half of us come from families where the father holds a professional or managerial position. Even when we compare ourselves to others in the academic community we are more advantaged. Only the students and faculty in the medical profession can claim a higher socio-economic background than political scientists (Ladd & Lipset, 1974, pp. 5-6). In terms of ethnocultural background it is worth noting that even though Catholics outnumber Jews by roughly nine to one in the general population, twenty-two percent of political science faculty is Jewish compared to 10 percent Catholic. Jews are also disproportionately represented at elite schools (Ladd & Lipset, 1974, pp. 7-9). In a 1982 survey of 637 political science departments it was found that women made up 11.17 percent of full-time tenure-track faculty. Blacks were 2.89 percent and 1.07 were Spanish surnamed Americans (American Political Science Association, 1982, p. 1). The political science professoriate is indisputably male, white and non-hispanic.

Political Views. Political views are taken with special seriousness by our profession. The major currents of these views have been consistent over a substantial period. A 1977 faculty study conducted by Ladd and Lipset indicates that 58 percent of social scientists identify themselves on the liberal side of the political spectrum (Lipset, 1981, p. 3). Among social scientists, economists are the most conservative; sociologists and anthropologists are the most liberal. Political scientists take a central position in this group, but are still liberal when compared to the average American (Lipset, 1981, p. 5). The *Christian Science Monitor* surveyed political scientists attending the 1981 American Political Science Association Meeting and 73 percent of those responding characterized themselves as "moderately liberal" or "very liberal" (*Christian Science Monitor* Survey, 1981).[1] This liberal stance is partially explained in terms of self-selection. Students with more liberal orientations are attracted to fields with a heavy focus on social problems and to fields where they feel ideologically comfortable (Ladd & Lipset, 1975, pp. 152-157). Ideology and partisan preference are determined more by adult socialization than by family background. Political scientists credit knowledge gained in the profession and the influence of colleagues as major determinants of their partisan preferences and ideologies (Turner & Hetrick, 1972, p. 365; Ladd & Lipset, 1975, pp. 157-159).

Political scientists are strongly Democratic, although there has been an increase in the number of Independents. This is evident in a comparison of the 1970 study of members of the American Political Science Association and those surveyed by the *Christian Science Monitor* in 1981 (See Table 9). It is obvious that the Democratic party's loss is not necessarily the Republican party's gain. Political scientists may become disillusioned and frustrated by the failure of the system to respond adequately to the problems confronting it—32 percent agreed that the American political system was not sound and needed many improvements or fundamental overhauling—but apparently they are not willing to join the Republican party to seek a solution. In the 1981 APSA convention survey respondents reportedly saw a conservative trend in the country, but 67 percent saw it as a brief phenomenon (*Christian Science Monitor* Survey, 1981).

Seymour Martin Lipset contends that the *leadership* of American political science is neoconservative. These neoconservatives support government action to curb social injustice and the welfare state, but they have serious misgivings about what they consider to be misguided efforts to achieve equal opportunity, and they are concerned about policies which fail to meet what they view as communist expansionism (Lipset, 1981, p. 5). It is likely that some variance does exist between these leadership views and those of the membership in general.

Franklin Delano Roosevelt was by far our favorite president. When asked to rank the best overall President from Roosevelt to Carter, 69.3 percent chose Roosevelt. The race for the worst president was won easily by Nixon who received 59.5 percent of the vote. His nearest competitor was Carter with 22 percent.

John F. Kennedy, who is one of the most popular presidents with the general public (Gallup, 1980, p. 27), does not have high standing among

TABLE 9

Party Preference of Political Scientists

Party Preferences	1959	1970	1976	1981
Republican	16.4	11.8	12	10.04
Democrat	73.7	73.4	74	61.19
Independent	8.0	12.5	12	22.39
Other	1.9	2.3	2	6.37

Source: 1959 data from Henry A. Turner, Charles G. McClintock and Charles B. Spaulding. The Political Party Affiliation of American Political Scientists, *The Western Political Quarterly,* 1963, *16:* 652. 1970 data from Henry A. Turner and Carl C. Hetrick. Political Activities and Party Affiliations of American Political Scientists. *The Western Political Quarterly,* 1972, *25:* 363. 1976 data furnished by Walter Roettger, Drake University, from unpublished Ph.D. dissertation, University of Colorado, 1977. 1982 data computed from *Christian Science Monitor*/American Political Science Association Survey, data obtained from the Roper Center, The University of Connecticut.

political scientists. Only 7.6 rank him as the best overall, and he is tied with Eisenhower in that rating. This is consistent with the earlier Ladd-Lipset finding that although Kennedy made a conscious effort to gain their support, he was never popular with intellectuals. Adlai Stevenson and Hubert Humphrey were much preferred for the 1960 Democratic nomination. Intellectuals disdained Kennedy's mediocre congressional record, and his failure to take a strong stand against McCarthy (Ladd & Lipset, 1975, pp. 22, 23). During Kennedy's term of office, James Reston of the *New York Times* discussed Kennedy's lack of support among intellectuals and said that they were describing his presidency as "the third Eisenhower administration" (quoted in Ladd & Lipset, 1975, p. 24). Some twenty years later Kennedy is still equated with Eisenhower among political scientists; for example, as best on foreign policy, 12 percent chose Eisenhower and 11.3 Kennedy. (Eisenhower and Kennedy rank on foreign policy behind Roosevelt, Truman and Nixon, in that order.)

Jimmy Carter's ranking among political scientists is low. Sixty-three percent rate him as the president who was least able to get things done. This is a consistent evaluation regardless of ideology, party preference, academic rank, gender or age. His predecessor, Gerald Ford, comes in second, but far behind with 20.6 percent.

When asked to compare Reagan with the past eight presidents 66 percent rate him as either not as good as most or worse than most. Political scientists do give him a grade of A, however, for his relations with Congress. From then on his grade point average begins to slip. He gets a majority of Ds and Fs on economic policy (58.4 percent), foreign policy (63.1 percent), social issues (73.8 percent), and over-all performance (51.1 percent). On all the scores mentioned above, women political scientists consistently rate Reagan lower than their male counterparts. Reagan's lack of relative popularity with women was manifested in the presidential election, which showed a significant sex difference in preference for Reagan.

As anticipated, political scientists exercise considerable "constraint" in their political ideologies. That is, their political positions and practices demonstrate a logical or systemic order (Ladd & Lipset, 1975, pp. 37-40). This constraint is shown by the fairly consistent positions taken by self-identified liberals and conservatives on major social issues in the *Christian Science Monitor* Survey. Self-identified liberalism/conservatism was the single most accurate predictor of policy position. It also confirms that political scientists are comfortably clustered at the liberal end of the political spectrum (see Table 10).

Educational Views. Clark Kerr has observed that "Few institutions are so conservative as the universities about their own affairs while their members are so liberal about the affairs of others" (quoted in Ladd & Lipset, 1975, p. 33). This has not been true of political scientists. The same consistency of ideas discussed above applied when political scientists were questioned on issues of student activism such as a broadened student role and the demands of blacks in the 1960's. Liberals were significantly more supportive of student positions (Ladd & Lipset, 1971, p. 138).

A major and controversial issue directly affecting higher education has

TABLE 10

Views on Selected Policy Issues of Political Scientists Attending the 1981 Meeting of the American Political Science Association

Issue	Political Scientists (n = 526)		Sample U.S. Population	
	Agree/ Approve	Disagree/ Disapprove	Agree/ Approve	Disagree/ Disapprove
Producing and readying the neutron bomb	34.5	65.4	48	44[a]
Reagan's handling of economic conditions	28.0	72.0	52	26[b]
Reagan's New Federalism	36.0	63.9	67	18[a]
Amendment to the Constitution that would permit prayers to be said in the public schools	7.9	92.03	76	15[c]
Equal Rights Amendment	83.04	16.9	63	32[d]

Source: Political Science data computed from data obtained from the Roper Center, University of Connecticut, *Christian Science Monitor*/American Political Science Association Survey. General population sample from:

[a] *Gallup Report* No. 193, October, 1981.

[b] *Gallup Report* No. 191, August, 1981.

[c] *Gallup Report* No. 177, April-May, 1980.

[d] *Gallup Report* No. 190, July, 1981.

been affirmative action. Among political scientists there has been a loss of support for affirmative action from 1969 to 1975. When asked about relaxing standards for the admission of minority students, 69 percent supported such measures in 1969 while only 43 percent did so in 1975; when asked similarly about minority faculty, support dropped from 40 percent in 1969 to 19 percent in 1975 (Ladd & Lipset, 1978, graph 3). The shift may be the result of a re-confirmation of the university as a meritocracy, but the shift in attitude away from minority faculty may also reflect a protective response to the tight academic market.

Political scientists are not, on the whole, well-satisfied with the product of their educational efforts. Data from 1969 show that 45 percent of the political science faculty at top schools disagreed with the proposition that graduate education in their field did a good job. The only field with a higher percentage of dissatisfaction was sociology (47 percent); by way of contrast, for business administration and chemistry the figures were 18 and 13 percent respectively (Ladd & Lipset, 1974, p. 35).

In a later study the attitudes remained about the same. Only half of the political science faculty said that graduate education did a good job, compared to three-fourths of all academics surveyed (Ladd & Lipset, 1978, p. 14).

This dissatisfaction may have several roots. One is that students must take courses from widely disparate fields, such as political philosophy and statistical applications. The sense of a unified whole may be apparent to only a few. The varieties of intellectual turnings may have caused some graduate students who need structure to lose confidence. Dissatisfaction is also nurtured now by the bleak job market outlook for many political science graduates. Perhaps we should not expect nor desire a high level of satisfaction with graduate education in political science at this time. Indeed, Charles O. Jones contends that, "We should probably be suspicious of a high level of satisfaction, since it could indicate that we have not kept up with the turns, twists and reversals which have characterized our discipline as it continues to undergo painful development" (Jones, 1982).

Views on Professional Satisfaction. This section can be summarized with Walter Roettger's (1978) conclusion that political science is not a very happy profession (p. 48). In all fields a minority of academics have always expressed regret concerning their choice of careers. Ladd and Lipset in 1973 found that 10 percent of political science faculty would not want to be professors, if they could start over again. This was about the same figure that applied to business,. engineering, and the medical professionals (quoted in Roettger, 1978, p. 31). More relevant is the finding of statistically significant deterioration in career satisfaction from 1963 to 1976 among academic political scientists. Asked in a 1976 survey, "If you were able to start over and pick your profession again, would you still pick a career in political science?," academics gave these answers among others (Roettger, 1977, p. 12):

	Definitely yes	Definitely No
1963	41%	1%
1976	29%	7%

The main "pockets of optimism" were found in two seemingly disparate groups: public administration and political philosophy. The one is a growing field, and the other, perhaps, is a field that attracts those who care most for the joys of pure scholarship (Roettger, 1977, p. 33).

THE ASSOCIATION IN AN ERA OF STRESS

The membership of the American Political Science Association enjoyed the sort of post-World War II growth that characterized many fields of endeavor during that boom period. Its numbers went from 3300 in 1945 (Waldo, 1975, p. 54) to 15,758 in 1974 (Mann, 1982a). By 1982 the number had dropped to 11,597. This reflects economic conditions, but it also reflects a breakdown of shared consensus; Roettger (1978) reported many "can't say" answers in a 1978 survey that covered many educational, intellectual and professional issues that might inspire controversy, but hardly lack of opinion. A former executive officer of the American Sociological Association has stated that calling the ASA an "association" may be an overstatement, since not everyone is associating with everyone else (Demerath III, 1981, p. 87). The same description may be appropriate for the APSA.

The Association's stresses have been compounded by the controversies arising from behavioralism and post-behavioralism, by the emergence of policy oriented associations, and the growth of cognate organizations such as the American Society for Public Administration and the International Studies Association (the latter accompanied by a growing conviction that public administration and international relations ought to break off from political science), by the Viet Nam War, by the rise of the Caucus for a New Political Science, and by the emergence of women and minorities as groups with claims on the Association.

Preliminary results from a 1982 APSA survey of political scientists show some of the expressed reasons for discontent with the Association:

> Those who let their membership lapse did so because they thought the dues were too high, they didn't like the *APSR,* and they felt the APSA didn't serve their interests. Lapsed members most frequently agree with the statements that "The dues are unreasonably high" and that there is "too much emphasis on quantitative research." Moreover, lapsed members appear to be slightly less professionally active (in terms of memberships, journal subscriptions and publishing) and earn somewhat lower salaries than current members. (Mann, 1982c)

The Problem of Membership Decline. APSA membership peaked in 1974; by 1982 it was down 25 percent from that peak (Mann, 1982a). Annual meeting registration peaked before membership in 1969 at a level of about 4200 at the New York City meeting. Low points in registration of under 2500 were hit in 1973, 1976, and 1978. Both 1979 and 1980, however, showed growth (American Political Science Association, 1981, p. 610). The 1981 meeting totaled 2,887 which was the highest registration since the 1972 meeting (French, 1981, p. 786). Perhaps the meeting locations (Washington, D.C. and New York City) helped.

One reason for the membership and attendance decline is the emergence of specialized groups. This is related to the lack of a single paradigm discussed earlier. As political scientists have become more specialized, some members have concluded that their interests are better served by other organizations. A comparative government area specialist, for example, may find that he/she has more in common with economists, sociologists and anthropologists working in the area than with other political scientists. This may also decrease the value of the *American Political Science Review,* the most prestigious journal in the field. The journal may not be providing an adequate vehicle for exchanging ideas and concepts among those with similar interests. Scholars may find it more rewarding to publish in journals read by their "significant others" where they are more likely to be cited and where they may establish their national reputation more quickly. Specialization has devalued the two principal reasons for joining the APSA—the annual meeting and the journal.

Another problem is that of communication among members of the discipline, not only between areas of specialization, but also between scholars who have training such that they can comprehend research using quantitative methods or arguments presented in terms of formal logic and those who cannot.

The APSA has been generous in its approach to unaffiliated groups which meet at its annual meeting. The number of unaffiliated groups that got courtesy listings in APSA programs rose from six (with two and a half pages) in 1972 to 31 groups with 30 pages in 1982. Some have contended in the APSA Administrative Committee that unaffiliated groups were prospering at the expense of the APSA itself (Minutes, 1982). It seems unlikely, however, that non-cooperation with sub-groups would be an effective long-run strategy. In 1982 the Association developed guidelines for the establishment of sections. Sections will make it possible for groups of APSA members who share a common interest to meet, coordinate communications and receive help from the APSA national office in collecting dues and maintaining membership lists (American Political Science Association, Fall 1982b, p. 627). It is hoped that the establishment of sections will lead to restored integration of the discipline.

The Caucus for a New Political Science. The emergence and activities of the Caucus for a New Political Science have been among the most noteworthy features of the Association's history over the past two decades. The New Caucus was an organizational manifestation of the intellectual forces that led to the post-behavioral movement, with which it has significant ties. The Caucus for a New Political Science was convinced of the inseparability of politics and intellectual work. Beyond this the New Caucus was what one of its founders calls "an organized insurgency" (Lowi, 1972, p. 12) that believed that the APSA should have a strong stand against the Viet Nam War and racial discrimination and that it should actively support the "war on poverty" programs of the Johnson Administration (Lowi, 1972, p. 13). There were immediate negative reactions among those with the strong conviction that professional academic associations should not take policy positions on issues not directly related to the main function of the organization: promoting the intellectual and professional interests of the discipline and its members. The position of others can be summarized in a statement of a former president of The American Sociological Association reacting to the issue of taking policy positions within his organization, "The most that can be accomplished is to announce a policy position on an issue and thereby provide a catharsis for members who need it" (Hawley, 1981, p. 108).

In 1966 it was discovered that some APSA officials had worked with the CIA; this had a catalytic effect on the New Caucus' emergence. Lowi described the New Caucus' leaders efforts in the 1967 APSA Business Meeting as precipitating "probably the ugliest confrontation in the history of the profession" (Lowi, 1972, p. 13).

The groundwork was thus laid for a major schism in the Association. In the 1970's the New Caucus continued to attempt to take control of the APSA. Probably the high water mark of these efforts occurred in 1973. The New Caucus had a full slate of officers to compete with the slate offered by the APSA Nominating Committee. The final vote (which was taken by mail ballot) gave the New Caucus candidate for President, Peter Bachrach, 3191 votes; Austin Ranney, the choice of the Nominating Committee got 3803 votes (American Political Science Association, 1974, p. 36).

The New Caucus continues to function in the 1980's, although it does not issue the sort of challenges that it did in the 1970's. The appeal of the New Caucus has diminished with the end of the Viet Nam War, the worsening of the job market, and the declining role of the American Political Science Association resulting from the emergence of competing organizations. Partly in reaction to their recent lack of significant success in sponsoring candidates, and because the APSA Nominating Committee, in an effort to be more representative, has regularly nominated some New Caucus members, the New Caucus has stopped nominating its own slate. In 1978 the New Caucus proclaimed itsef a socialist organization; in 1979 it amended this to include socialist feminist. Its avowed objective is to create "a socialist center of gravity in the profession" (Caucus For A New Political Science, 1981). It has developed local chapters and is working with other groups, such as the Marxist Literary Group in sponsoring conferences. Its journal, *New Political Science,* deals with such topics as "The Left and Civil Liberties" and "The Socialist Academic: Between Theory and Practice." It meets annually as an unaffiliated group at the APSA convention. In 1982 it sponsored 25 panels. A major attraction of the New Caucus has been that of providing a forum and a critical means of support for those doing analysis of policy issues relevant to creating the social changes necessary to transcend capitalist society (Caucus For A New Political Science, 1981; Lankowski, 1982).

The Status of Women. Another important and related aspect of the challenge raised by the non-establishment groups in the late 1960's was the situation of women political scientists. In 1969, about 5 percent of APSA's members were women. About 8 percent of the faculty members in the field were women (Gruberg & Sapiro, 1978, p. 318); the lesser amount of membership may have reflected low pay, and temporary appointments.

In 1969 the Association adopted a resolution supporting equal treatment for women (American Political Science Association, 1969a, p. 671) and the Committee on the Status of Women was established (American Political Science Association, 1971, pp. 3, 10). In the same year women's concerns were organized through the establishment of the Women's Caucus for Political Science. In the 1970's the Women's Caucus ran candidates for APSA offices. Efforts to combine slates with the New Caucus were rarely successful because many women who were concerned with feminist issues did not share the New-Left ideology of the New Caucus.

The 1970's saw a dramatic percentage increase in the proportion of women in the field, except at the rank of full professor:

	% Women	
	1972	*1981*
Bachelor's degrees in Pol. Science	19%	37%
Master's degrees in Pol. Science	19%	28%
Ph.D. in Pol. Science	10%	20%
Assistant Professors	10%	20%
Associate Professors	8%	12%
Full Professors	4%	5%

Source: Data from Thomas Mann, Executive Director, American Political Science Association.

The APSA has never had a woman president. In 1979 the Women's Caucus nominated Betty Nesvold for president. The New Caucus also ran a candidate against the nominating committee's choice, Charles Lindblom. Although the Women's Caucus ran a vigorous campaign their candidate came in third and far behind the official nominee (813 to 2,335) (American Political Science Association, 1980, p. 42). Over the past decade Women's Caucus candidates who were also official Nominating Committee choices gained election to office. This has provided them with representation on the Executive Council of the Association.

In 1978 the Women's Caucus and other supportive groups, in an intensely debated motion before the Association's business meeting, passed a resolution calling on the Association not to hold its annual meeting in Chicago because the State of Illinois had not passed the Equal Rights Amendment to the U.S. Constitution. If the resolution had received less than a two-thirds majority it would have had to be ratified by the total membership by mail ballot. Those opposing the action argued that the ERA position violated the APSA constitution's ban on committing its members on questions of public policy. Others believed that the APSA should honor its hotel contract which had been made before the extension of the time period for ratification of ERA. They feared the law suit which ultimately occurred. Those supporting the business meeting action responded that in 1972 APSA had passed a resolution in favor of ERA and the Chicago boycott was simply an implementation of present policy. They also contended that one of the Association's main raisons d'etre is to promote and protect the interests of its members. As long as women members of the Association are victims of professional and employment discrimination, the Association has a responsibility and an obligation to take any and all steps necessary to help alleviate the situation.

The reaction of the Association's leadership was similar to its response in 1969 when it feared the New Caucus would take over the Association at business meetings. The 1969 response was to suggest a change in the Association's constitution so that candidates who were opposed had to be elected by a mailed secret ballot rather than at the business meeting which had been the earlier practice. The argument was that "temporary majorities" should not be making decisions for the vast majority of members. This was countered by the contention that members with sufficient interest to attend the annual meeting can adequately represent the membership (American Political Science Association, 1969b, pp. 269-302). There was thus a sense of deja vu in 1979 as the establishment moved to limit the power of the business meeting to take policy positions. A detached observer of the 1969 meeting made some observations which were equally applicable a decade later. Anthony King of the University of Essex, England commenting on the struggle between the New Caucus and the establishment wrote:

> One is bound to feel sorry for...(New Caucus spokesperson). The poor man claims to value democracy and participation. But he knows very well that the more democracy and participation there is in the Association, the worse the prospects for the Caucus, so he is reduced to extolling the virtues of the Annual Business Meeting which he knows can, with any luck, be controlled by a tiny unrepresentative minority...Meanwhile, the defenders of the status quo profess to be anxious to embody democratic principles in the Association's constitution, whereas they are really worried about the possibility of a Caucus takeover. It is all an act on both sides. The Establishment's act is marginally more enjoyable, if only because it is being played with such a straight face. (American Political Science Association, 1969b, p. 294)

In 1980 the Constitution was amended by a vote of 2400 to 887 so that controversial policy issues receiving one-third vote at the annual meeting must be submitted to the entire membership by mailed secret ballot (American Political Science Association, 1980, p. 42).

The influence of the Women's Caucus, however, has been felt, and women have influenced the Association in many significant ways. In 1981, women held 11 percent of all political science full-time positions (American Political Science Association, 1982a, p. 1). This represents progress, if not great strides. The outlook is that the percentage of women is likely to do little more than hold steady because of the small number of openings, lower Ph.D. output, and the lower attractiveness of academic careers.

Minority Members. The American Political Science Association had a Black President, Ralph Bunche, in 1954. Despite this milestone the inclusion of racial minorities in the discipline remained extremely low, relative to the population at large. In 1969 there were only 65 black American Ph.D.s in Political Science, well under one percent of the total (Prestage, *et al.*, 1977, p. 1). In 1969, however, two developments improved the situation. One was the establishment of a Committee on the Status of Blacks in the Profession; the other was the establishment in the APSA of a fellowship program for black graduate students. Evron Kirkpatrick, then Executive Director of APSA was prominent among those who worked to improve the status of blacks. By 1977 the number of black political science Ph.D.s reached 200 (Woodard, 1982).

In 1977 the Committee on the Status of Chicanos was established for similar reasons. This committee also encourages members of its group to enter the field, seeks aid for graduate students, and stimulates research.

Chun-tu Hsueh argues that Asian and Asian-Americans are underrepresented and discriminated against in political science. To support this contention he observes that no Asian has ever served on the APSA Council and that the Council denied a 1976 request to establish a Committee on the Status of Asians. The Council, however, agreed to publish professional notices of special concern to Asians in *PS* and to make space available at annual meetings for meetings of Asian political scientists. The status of Asians in political science contrasts with their prestige in other disciplines such as physics. More Asian-American political scientists could serve as helpful role models for Asian and Asian American students; they could also offer useful insights into many issues (Hsueh, 1976-77, pp. 11-15).

Had the favorable market conditions of the 1960's persisted, the efforts of the committees established to improve the status of underrepresented groups in the profession would probably have yielded more positive results. As things have evolved, the profession was opened noticeably for minority groups at the time of the market decline. The placement success of blacks was 90 percent in 1978 and 96 percent in 1981; for Spanish surnamed political scientists it was 100 percent in 1980 and 67 percent in 1981. The reader is reminded that we are dealing with small numbers. Blacks were 4 percent of 1981's placement class of 696; Spanish surnamed candidates comprised only 2 percent. By contrast the placement of women in 1981 was 75 of 101 candidates (including women of all races); this was the same percentage as that of men of all races (Sheilah Mann, 1982, p. 88).

Association Responses. The establishment of the committees on women, blacks and Chicanos was in itself a significant response on the part of the APSA and demonstrated the willingness of the Association to open itself more to the full participation of all members. All the recent Executive Councils have had women and minority officers. There are those who believe that the comparatively small number of women and minorities involved in leadership positions represents "tokenism" and object to the tendency to re-appoint the same small group instead of seeking to identify new talent.

One available measure of tangible progress is participation in the annual meetings. Progress by women has been dramatic in this respect. In 1970, for example, 5.6 percent of the papers presented at APSA meetings were by women; by 1980 the figure was 21.9 percent and in 1981 it was 18.8 percent (Gruberg, 1981, p. 725). Women are thus represented more as paper givers than as full-time faculty.

Blacks have had more uneven progress, due perhaps in part to their relatively small numbers. Black program participation rose from 14 items in 1969 to 23 in 1972, but it dropped back to 16 in 1976 (Prestage, 1977, p. 16). Unfortunately full data on minority representation on panels are not available.

STRIVING FOR QUALITY AND VITALITY IN A DECADE OF AUSTERITY: APPROACHES TO MEETING THE CHALLENGES

Political scientists in the 1980's must understand and accept the realities of their situation, but they also must be future-oriented. There are discouraging aspects to the stagnant job market and the low turnover of personnel. We should remember, however, that most academic fields share a similar environment. Non-academic fields—many in the private sector and some in the public—often look at mere stagnation with envy, as they fare worse.

This period of increasing scarcity may not be all negative. Some serendipitous results may come out of the experience. Let me suggest two: First, the cut in research funding may minimize what many consider to be a prostitution of academia. Some observers have seen instances in which new ideas in the area of pure theory were shunted aside because researchers found it more convenient to work on projects for which funds were more readily available. Perhaps more people will be making decisions on the basis of academic value rather than fund availability. Second, we may see a resurgence of an emphasis on teaching. Areas such as political theory which have been devalued because of limited outside funding sources may experience a renaissance. Third, we are witnessing what Thomas Mann has described as the "political mobilization of the social science community" (Mann, 1982a, p. 416). In response to the Reagan Administration's proposed cuts for social science research, the Consortium of Social Science Associations was established. This new organization has the potential for providing a unified and effective voice in representing social science interests in Congress and with the federal bureaucracy.

The prime responsibility for maintaining academic vitality must rest at the individual department level. We must accept the fact that we have relatively young faculties who will not be able to move away. Faculty development must attain a very high priority. Support for attendance at professional meetings and seminars must be augmented, perhaps with outside gifts. As the pattern of student interests inevitably shifts, our nearly "tenured-in" faculties will have to re-tool. We may need to encourage a network of faculty exchanges. We will need to make more productive use than ever of sabbaticals. Department heads and chairpersons must assume an appropriate leadership role in seeking adequate support from their administrations; then they must work together nationally—in large part through APSA and the regional associations—to share their thoughts and to learn what methods work most effectively. More attention needs to be given at national meetings to professional development, and it also merits more research attention.

The Association itself had demonstrated that it is capable of responding to changing conditions and membership concerns. It now must move beyond response toward future-oriented actions.

NOTES

1. The respondents are sufficiently close in overall profile to the membership of the American Political Science Association to warrant the use of the data shown. The results are also generally consistent with other sample surveys of members.

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