Political Science: The State of the Discipline II

Edited by Ada W. Finifter

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This book is dedicated in honor of all political science scholars who, from a multitude of perspectives and using diverse methodologies, strive to increase knowledge and understanding of politics and political life.

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Preface

This project has its origin in the Spring of 1981, shortly after Seymour Martin Lipset, then president of the American Political Science Association, asked me to be program chair for the 1982 Annual Meeting. In undertaking that job, I made a fateful decision -- that it might be interesting and useful to encourage a substantial number of political scientists to turn their attention to one particular issue by having a "theme" for the meeting. The theme I chose was "The State of the Discipline." The idea was well received; interest in serious evaluation of research and knowledge in the discipline was very high, and President Lipset and Thomas Mann, then executive director of the Association, asked me to collect the theme papers that had been presented at the meeting into what became the first edition of this book. This second edition retains the same overall goal of reviewing important recent research in various subfields of political science. All of us connected with the project share the hope that this edition will be as useful as was the first for students, teachers, researchers, and others who are interested in the cumulation of knowledge in our discipline.

In the years since the first edition was published, we were pleased to learn that teachers throughout the profession used that volume in a variety of different courses, and that many researchers were using it to get an overview of both their own and other areas of research. About two years ago, therefore, the current executive director, Catherine Rudder, and deputy director Robert J-P. Hauck, thought it must surely be time to do an update. This edition, however, did not begin as a series of papers for an annual meeting but, rather, "from scratch." To provide the opportunity for different perspectives on research to be expressed, we assembled a new set of chapter authors. In addition, these authors were asked to provide a perspective on the research literature that was as international and comparative as their expertise and the research literature itself permitted. The story of how this transition occurred may be of some interest to members of the profession.

At the time we began to plan for the second edition, Theodore Lowi was the president of the Association. Since we did not have an existing group of papers already prepared as we had had for the first edition, President Lowi and the Publications Committee asked me to consult with colleagues in the profession before deciding on the final group of authors for the new edition. As I recall those early conversations with Cathy, Rob, and Ted, I think we all assumed that we were going to ask the scholars who had written for the first edition to update and revise their chapters, and that the consultation I was to do would be primarily about the new chapters that would be added to cover some major areas that had not appeared in the first edition. I think we all envisioned a project that would be completed fairly quickly and with few of the coordination complexities that usually accompany volumes involving large numbers of authors -- certainly that was my understanding!

As with any project that involves consultation, however, one does not always anticipate the ideas of those one consults. And when the ideas that come forth are good ones, one may wind up with a project that is considerably different from that envisioned by its initiators. That is certainly the history of this edition.

To comply with the request for consultation, I wrote to perhaps a hundred colleagues who had recently served in positions in which they would have had the need and the opportunity to think about the state of the discipline, either in general or in their own particular research areas. My correspondence list included current and past section chairs for the Annual Meetings, officials of the "organized sections," officers of the Association, and other colleagues and friends whom I knew to be especially concerned about the state of the discipline. I expected few replies. Instead, I received numerous thoughtful, concerned, and extraordinarily helpful letters from colleagues of every disciplinary persuasion. Most often, people were generous with kind remarks about the first edition and numerous suggestions for coverage and authors for the new one. There were also, of course, those who were less happy and wanted important changes to be made.

These letters had a profound effect on the further course of planning and, ultimately, the book itself. In the end, it is in large part this input that was responsible for the direction the new book has taken, and the ways in which it departs from the first edition. The many specific suggestions about coverage and content were all interesting and helpful and very many resulted in specific charges to authors about materials that should be covered in their chapters. Many of the authors whose chapters are included were suggested to me initially by these correspondents.

My informal content analysis of the letters also yielded several more general themes that came up with some frequency.

First, both as researchers and as teachers, colleagues did want updates on what was going on in

their own areas and also for areas that they did not have the time or the responsibility to follow on a regular basis. The advice here was yes, do another volume, but then be sure to do a third before another ten years goes by. The literature advances too rapidly to have such long intervals between editions.

Second, many writers pointed out that the research literature is so specialized, and so many new areas of research have achieved critical mass status, that many more chapters were needed. Surely the new edition would have to have a chapter on (insert your favorite research area here; for sure, someone mentioned it).

Third, political science is a worldwide endeavor. Not only do we need more comparative chapters than there were in the first edition, but *all* the chapters should be comparative. (The people who made *this* suggestion were a particularly intense minority, who tended to express themselves with somewhat more passion than many others...)

Fourth, the chapter by (insert your favorite first edition chapter author here) was truly outstanding but what you must do now is give (insert a new name here) a chance to write on this field so that other perspectives, other approaches, and other bodies of literature have a chance to be highlighted.

In short order, I realized that I had gotten much more advice than I had bargained for, and that all of these common themes represented worthwhile ideas that I wanted to take seriously. It soon became clear that this would not be the relatively expeditious undertaking that had been envisioned. But it would be an opportunity for the resulting book to respond to at least some of these ideas and perhaps to have an even broader impact than we had imagined.

There was not much I could do about the first two of these themes. It was not my task at the moment to plan for future volumes. And, for the present, the Publications Committee and the Council had approved funding for one volume whose size would limit the number of chapters that could be included. But despite the fact that they departed from our original informal planning, I decided that the third and fourth themes were within my purview as editor, and that I would try to implement them.

More comparative approaches and coverage appealed to me for two principal reasons: first, I thought that chapters that made a serious effort to cast our research in an international, cross-national, or cross-cultural perspective could be very useful in encouraging the development of more general theories of politics. And, second, I hoped that a comparative perspective would make the book more useful in other countries and thereby serve to broaden the scope of shared ideas and perspectives, and stimulate additional cross-national discussion and research.

If the comparative theme were to be taken seriously, however, I realized that many of the chapters would have to be substantially different than the versions that had appeared in the original volume, in which this was not a special consideration. At the same time, many writers had commented on how useful the original chapters had been. Ultimately, I saw that if I were to encourage a comparative perspective, and invite at least some new authors, that the original chapters would not, in fact, be superseded by the new ones. The new chapters would of course include a more recent set of materials, but to the extent that they also provided different approaches, including the possibility of a comparative perspective, they would not necessarily supplant or revise the perspectives and materials discussed in the original chapters. And if having a new author with a different perspective was a good idea for one or two areas, why not for all?

Thus began the "second," rather than the "revised," edition of this book, a two year project that would involve an entirely new set of authors whose charge would be to review, discuss, and assess the literature in each area, from a world-wide, cross-national, or cross-cultural perspective, in whatever ways and to the extent that the research in each field lent itself to this goal. I hope that others will share the view that the new goals of this current edition were worthwhile ones, even if their accomplishment has been uneven or incomplete. And because the papers in the original edition really are different in focus and perspective, they remain valuable research and teaching tools. For this reason, the Table of Contents to the first edition of *Political Science: The State of the Discipline* is presented as an appendix to the present volume.

Telling the story of how this book came to be will make clear that while the Association is publishing it, it is in no way an "official" or "authorized" guidebook to our discipline. If a field is omitted, it is because of space problems rather than any decision as to its importance or relevance to the discipline, or the quality of research it produces. Nor, however, do I want to take all the responsibility for fields that do not appear. There were several cases where the person who was invited to write a particular chapter never did so and by the time it became clear that the chapter in question would not materialize, it was too late to invite someone else. And in a couple of cases, authors who did submit first drafts were unable in the short time available to revise their chapters in time for publication. On the positive side, I am pleased that we were able to bring to completion as many of the chapters that were originally planned as do appear in the book.

I hope researchers and teachers in fields that are not covered in this edition will find useful material in the other political science research reviews that have been published in recent years, and that together all of these efforts will contribute to the nourishment and flowering of all of the diverse branches of our discipline. But there may be more that we can do to encourage this process, which brings me back to the first two themes that emerged from the letters of which I spoke above: more frequent updates in a larger number of areas.

Both the letters I received and the chapters that appear in this book suggest to me that political science is in a period of great vitality and increasingly rapid research development. But to turn a proliferation of ideas, information, and research findings into knowledge and theory requires intellectual synthesis and integration. Literature reviews can contribute in an important way to this process.

As I noted above, a few other handbooks and volumes of literature reviews for our discipline have appeared in recent years. Each of these has been a large undertaking that took years to complete. Moreover, because virtually all have been self-contained efforts, the need to draw all of the chapters together for publication at a fixed time in a pre-defined amount of space means that coverage will almost surely be incomplete. Certainly that is the case with this volume. Limited size prevents the inclusion of chapters in each area in which vigorous research is occurring. Fixed and infrequent publication dates mean that chapters that are commissioned but not finished in time do not appear, and that fields growing rapidly may not be reviewed as often as the literature warrants. As knowledge proliferates and research becomes more specialized, such omissions may leave unrepresented in any one collection important fields of research and growing numbers of researchers.

Therefore, in addition to stimulating research and discussion in the areas that are covered, I hope the publication of this volume will also encourage more attention to the utility of having literature reviews published on a more regular basis and in a wider variety of areas. The most efficient way to do this is through serial publication. Our journal editors might consider whether they can do more of this than they have in the past, although the increasing quantity of the primary research itself creates space pressures that may make this solution improbable. A new journal devoted to literature review also suggests itself. Another possibility might be for the Association to develop a monograph series of literature reviews.

An ongoing series in paperback format would mean that there would be no artificial (i.e., space and time) limitations on the subfields and areas that could be covered, and that fields could be reviewed as frequently as the growth of research or diversity of perspectives in each area warranted. Each of us contributes only a very small part to the enterprise of developing a science of politics and political life. Keeping abreast of what others are doing and getting perspective on how our own work fits into or departs from the larger effort can help in important ways to make that part as productive as possible. For teaching purposes, smaller and relatively inexpensive monographs could be useful in an even larger number of courses. I therefore hope serious discussion of these possibilities will take place in the near future.

The story of how this book came to be would be entirely incomplete if I did not say how important a role was played by the reviewers of all of these chapters. It is understandable that an editor goes first to friends and acquaintances for such a task because it tends to be unheralded and anonymous and one hesitates to impose on those one doesn't know personally. But how many people in so many different fields can one editor know personally? Thus, I found myself at the door, the telephone, the FAX machine or the E-mail port of friends and colleagues, both personally known and known only by name and reputation, at work, on vacation and about to leave for field research. Not only were these colleagues generous in their advice the first time around but many of them graciously assisted again when I called to ask for a second reading of a revised manuscript. The journal editors in our profession probably already know that so many of our colleagues are such good citizens; for me, on a once in every ten-years basis, it was a pleasure to be reminded that I had found this out once before.

My own colleagues at Michigan State, being handier than any others, are the largest contingent of reviewers from any one University and they are due a special note of thanks for their generous counsel, numerous suggestions, and willingness to talk on a moment's notice. Special thanks are due to the department chair, Brian Silver, for marshalling substantial material support for the project.

The American Political Science Association, of course, has borne the major part of the expenses of the project. More than this, however, we are all fortunate to have an Association whose staff is truly dedicated to serving the needs of the discipline, as well as the profession. I want to thank Cathy Rudder for her overall support and encouragement of the project. Joanne Dunkelman managed to take computer disks from twenty different contributors and through the marvel of desktop publishing, and a great deal of personal skill, turn them into the pages of this book. Pat Spellman's careful proofreading was a great help to all. Most of all I want to thank Rob Hauck, who facilitated the project in myriad ways and always with great good humor. As well as supervising the physical production of the book, Rob was always there with phone calls reminding me about deadlines and press dates. Had it not been for his

support and assistance, the book might have been a twenty-year rather than a ten-year update.

Without the assistance and encouragement of many other colleagues throughout the discipline the project would have been both more difficult and less rewarding. I want especially to thank the following individuals who either served as reviewers of the individual chapters or who wrote me with advice and suggestions for the book:

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Ada W. Finifter
East Lansing, Michigan
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Theory and Method

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Texts and Canons: The Status of the "Great Books" in Political Theory

Arlene W. Saxonhouse

The arguments have flown fast and free over the last several years, rocking the academic world: canon or no canon? Is it Western imperialism, misogyny, and racism, or is it the core of civilized discourse over the ages? Are the texts we study and assign to our students culture and time bound, reproducing the white, patriarchal past in a multicultural present or are they to be preserved as the focus of common discourse lest we and our students drown in a sea of relativism? The rhetoric on both sides of the debate has been powerful; moderation has been a forgotten virtue. And the subdiscipline of political theory as the study of great texts written by earlier generations has not been immune to the swirling debates. Indeed, it was a political theorist, Allan Bloom, in his best-selling *Closing of the American Mind* (1987), who did much to bring this issue to public attention and to set the tone for the debate. While this debate has addressed the broader field of the role of education in contemporary American society, challenges from within the discipline of political theory have been launched as well, questioning the claimed apotheosis of certain classic readings: the works of authors such as Plato, Aristotle, Machiavelli, Hobbes, Locke, Mill, and Marx. Others have defended this core as being at the foundation of our ability to theorize about the value, purpose, and meaning of political life, and the courses in which students read Plato et al. continue to be included in the curricula of most political science departments.

Political theory as a field managed, though barely, to survive the assaults of the 1950s and 1960s. On the one hand, there was positivism, which denied validity or meaning to the conceptual core of a field that used such inaccessible terms as justice, duty, freedom; on the other hand, there was behavioralism, which dismissed the normative in favor of the empirical. The severest and most powerful assaults of the last decade, though, have come from within. John Gunnell, for instance, claims that the core of authors and readings traditionally included in the study of political theory is simply a "myth," constructed by modern theorists with particular agendas; others argue that those works that have become part of the so-called "myth" survived by chance rather than because of their status as great works, worthy of careful reading, from which we might learn about the nature of politics (Condren 1985). Yet others have turned political theory into a subfield of history, especially intellectual history, embedding the classic texts in an historical setting of language usage on one side or political experiences on the other. To borrow the language of David Miller describing this movement: "[T]exts that we now regard as classic sink into the landscape" (1990, 425).

Such attacks arise since in many ways political theory *is* an artificial construct that lacks an independent identity, caught as it is at the cross-roads of a multitude of disciplines. While political scientists may see political theory as a subfield of their own discipline, political theory draws from (and is claimed by) history, philosophy, sociology, literary studies, linguistics, and women's studies.[1] At home in none, but enriched by all, political theory during the last decade has confronted the new threat of the centrifugal force of interdisciplinary connections which have often led to sharply divided communities of discourse. Beyond the isolation from mainstream political science, the sub-specialties of political theory informed by their various inter-disciplinary connections have become isolated from one another. Thus, at the same time that the very texts we might have defined as the unifying basis of political theory are under assault, the activity of studying those texts has itself been drawn in different directions.

It is the argument of this essay, though, that the texts that have come under such powerful and (usually) articulate attack and that have been appropriated by other disciplines have weathered the assaults and remain a central part of the discipline of political science -- though recent trends suggest that their survival is in no sense assured. In part, these texts have survived the attacks of the last decade because political theorists have not established a simply defensive stance asserting the validity

of the old and established; rather, they have expanded the readings to include a far more varied corpus of works. Further, what I shall call the "instrumental" approach to political theory has allowed for new questions to be addressed to these texts and, precisely because of the inherent richness of these texts, they can help us address such questions. The texts explore what may be called the "perennial questions" that confront all political communities, e.g., how to organize and distribute (i.e., develop principles of justice), what goals to define or deny (i.e., develop justificatory principles). Thus, they have been able to clarify how contemporary questions arising from contemporary events are illuminated by reflection on the fundamental questions of political life with which the classic authors deal.[2]

The survival of texts as the basis of the study of political theory, though, depends on the continued ability of these works to respond to the questions posed by a variety of schools. I emphasize the importance of this response since, of late, the discourse of conflict between the various schools in political thought may have mellowed in tone, but perhaps only because of the absence of a continued engagement; the subfields within political theory seem to suffer from the "separate tables" syndrome diagnosed by Gabriel Almond as plaguing the discipline at large (1988). To argue for the survival of texts at the core of the discipline, though, I must try to integrate the two major, but warring approaches within the subfield and suggest how the newer trends can serve to aid and not undermine one another. The integration may not be possible, but the attempt is essential if political theory is not to lose its status as the normative foundation from which political science gains its significance.

R.G. Collingwood in his sensitive and engaging *Autobiography* introduces his chapter entitled "Question and Answer" with the story of his confrontation with the Albert Memorial in Kensington Gardens. Passing it every day on his way to work, he was struck:

> Everything about it was visibly mis-shapen, corrupt, crawling, verminous; for a time I could not bear to look at it...recovering from this weakness, I forced myself to look, and to face day by day the question: a thing so obviously, so incontrovertibly, so indefensibly bad, why had Scott done it? To say that Scott was a bad architect was to burke the problem with a tautology; to say that there was no accounting for taste was to evade it....What relation was there...between what he had done and what he had tried to do. Had he tried to produce a beautiful thing?...If so, he had of course failed. But had he perhaps been trying to produce something different?...If I found the monument merely loathsome, was that perhaps my fault? Was I looking in it for qualities it did not possess, and either ignoring or despising those it did (1929, 28-29)?

Collingwood's tale can be read, as one school of thought does, as only allowing the old questions to be asked of artifacts, whether they be the bowl made by the ancient Corinthian potter or the dialogues written by Plato. Anything more would impose on the texts a meaning never intended. And one finds support for this view in Collingwood's claim that: "Now, the question 'To what question did So-and-so intend this proposition for an answer?' is an historical question, and therefore cannot be settled except by historical methods" (1929, 39).[3] Or, his tale may be read as urging us not to ask our own questions; thus, we may remove ourselves from the specificity of our own time and place, and learn from the insights offered to us by those whose questions may have been different, but who, precisely by their different questions, make us attend to universals rather than the particularities of our own time and place. The latter reading of Collingwood's message would keep the study of texts as political theory within the discipline of political science as a guide to its practice. The former may transfer the reading of such texts to a subfield of history or make it of curious interest to the political scientist, but hardly the stuff out of which a discipline is made. The conflict between these two approaches and their numerous variants have dominated the last decade. Political theory concerned with the classic texts must confront that conflict both substantively and methodologically.

The Construction of a Canon in Political Theory

In the earlier edition of *The State of the Discipline* and in two articles in the *American Political Science Review*, one from 1978 and a more recent one in 1988, John Gunnell analyzed the "reified analytic construct[ion]" of what he called the "chronologically ordered canon of classic texts" (1988, 72) or "a reconstructed tradition [that] has become the principal context for understanding the meaning of past texts, and the significance attributed to a work that has become largely a matter of its role in this historical narrative" (1978, 122). In the earlier essay, Gunnell uses Leo Strauss's writing as an example of many who "presuppose the tradition of datum." "Strauss articulates a distinctive traditional myth; but the myth of the tradition, in one form or another, has been a pervasive

feature of most scholarship in the history of political philosophy" (1978, 131). Gunnell here sets forth an important challenge for those in political science who study texts: not simply, how do we study the texts that we do (the most important methodological controversies within the subfield which over the last decade have threatened civilized discourse among its practitioners), but why study texts. Both questions need to be addressed in an assessment of the state of political theory.

The debate over the "canon" that has in the last several years come to infiltrate the study of political theory may find its origins in the writings of Matthew Arnold, who in his turn drew heavily from the German authors concerned with *Bildung*, the moral and character education of the young. Confronted with the emerging challenge to the Bible as the primary source of moral education, Arnold proposed the turn to literature as the new guide to the moral education of the young in a democratic regime. What had been canonical in terms of Church-related doctrine becomes canonical in terms of a secular literature that teaches the virtues of the new secular age. Reading texts is not merely illuminating, historically enlightening; for Arnold speaking to the world of nineteenth century England, the careful reading of literature is the source of our humanity and identity.

Quite in contrast to such a role for the canon, George Sabine published his volume, *A History of Political Thought* in 1937 and thereby established for American students of political science the sub-field based on the title of his book, distinct from intellectual history and a part of the study of politics. Unconcerned with the history and writing of texts as the source of education for the upright man or citizen, he introduces his volume by noting that "theories of politics are themselves part of politics," and by firmly asserting that "they do not refer to an external reality but are produced as a normal part of the social milieu" (1949, xi), he made it clear that his study had nothing to do with virtue, truth, or moral education. By the time of the second edition in 1949 Sabine asserts that the author (i.e., himself) is "even more convinced than he was in 1937 that neither he nor any man can stand apart from the values and conventions of the culture in which he was read" (1949, ix). Thus, his volume is a selection of authors who have interesting things to say about certain political issues of their times and may have also had an influence on subsequent authors. Sabine's firm assertion that the texts studied were exemplars of political thought of the past granted them no particular status beyond localized expressions and analyses of political circumstances. They were no more than that, not eye-opening documents, not works that might transform the reader as Arnold envisioned in his campaign to create a literary canon. Sabine's selection process, however arbitrary we may consider it, has had a primary role in defining the canon for the discipline. For example, he includes Seneca and Ambrose, John of Salisbury and Dante, John Knox and Thomas More, but not Aeschylus or Tacitus or Shakespeare or Mary Wollstonecraft or Simone Weil or Hannah Arendt. All in the latter group have become in the last decade or are in the process of becoming, part of the ever expanding, though seldom contracting, core of works read and studied by political theorists.

Sabine's choice of whom to include depended on Sabine's assessment of who said interesting things about politics, but also the degree to which he could set a particular author within his particular milieu: "The fact that a man exists or that a book was written is, in itself, no part of the history of political theory as here conceived" (1949, xiii).[4] In stark contrast to Sabine's approach to the history of political thought stands the work of Leo Strauss and many of his followers. For Strauss, the texts that we include in our canvass of what has been said about political life in the past have an entirely different status; far from being simply the expression of the political circumstances of a particular time, they have become in his writings works to be plumbed for the truths that they may tell about political life, its goals, its possibilities, and especially its limits. In his concern for natural right, these are not truths that depend on the political circumstances of a particular time. They are not simply "produced as a normal part of the social milieu." The study of texts goes beyond learning what interesting things were said about politics in the past to the search for what these texts, though written long ago, can tell us about politics in a world confronted by the crises of self-doubt, historicism, and relativism. They play the moral, educative role Arnold praised, not so much because they teach what to think, but how to think about the political world. As Strauss phrases it in his essay "What is Liberal Education?," "We cannot be philosophers, but we can love philosophy; we can try to philosophize. This philosophizing consists...in listening to conversations between the great philosophers...the greatest minds, and therefore in studying the great books" (1989, 7).[5] Thomas Pangle and Nathan Tarcov put it this way in the newly added "Epilogue" to the 1987 edition of the Strauss-Cropsey volume *History of Political Philosophy*: "Strauss's study of the history of political philosophy therefore presupposed a radical dissatisfaction with contemporary thought and a resulting interest in that earlier thought which did not start from the premises responsible for the present crisis" (1987, 912). In this sense Strauss is, as Gunnell claims, "thoroughly instrumental...in that he utilizes the history of political philosophy to mount an attack on what he believes to be the philosophic foundation of contemporary politics" (1978, 130). There was nothing instrumental about

Sabine's readings; he simply told us what men thought about politics. There is something profoundly instrumental inherent in Strauss' study of the texts.

To Strauss and Sabine can be attributed what I see as the two dominant approaches to the study of texts of political theory in the last decade, though the variants are many.[6] On the one hand, there are those who turn to political theory for enlightenment about our condition in the contemporary world, for guidance about the normative choices that we as political creatures confront, for a sense of who we as political actors are or can be, for the conversations we must have about the meaning of political life.[7] On the other hand, there are those who deny that the texts of the past can provide us with insights of that sort, who demand that we see the history of political theory as a way of studying the foundations of our political language and concepts, of learning how texts and concepts emerge from political and linguistic practices and how they in their turn come to govern our understandings and analyses of political life.[8] Such differing goals for the study of political theory have generated further (but, I would suggest, subsidiary) debates about how one goes about studying a particular text. These methodological debates may have caused firestorms of controversy a decade or so ago; more recently, the methodological concerns have led to a series of self-exploratory reviews of methods within, rather than across, approaches. This has had the consequence of taming the discourse at the same time as unfortunately dividing the discipline.

Gunnell may be justified in claiming that the canon has been constructed in the case of political theory, but the challenge is to assess how we as scholars are to respond to this construction. Are we to follow what Gunnell has pejoratively called the instrumental approach? Are we to find in this construction of texts an historical challenge, both of origins and of impact? What methodological implications do such approaches entail? Are we to escape the texts whose language constrains us within antiquated and/or oppressive conceptual frameworks? Before I turn to a discussion of the two major approaches to political theory, let me try to frame the discussion in part by addressing what, in answer to this last question, is perhaps one of the most radical challenges to political theory as the study of texts. This challenge finds its origins in Hegelian thought and is carried forward by such continental authors as Nietzsche and Heidegger and on to Foucault and Derrida; it raises questions not about the text insofar as it serves as a source of enlightenment about what was thought in the past or what ought to be thought in the present, but about whether the text as object is capable of study by a subject and whether the text imprisons the reader in the particular language an author (or the tradition) uses, thereby limiting the nature of our discourse about political life. It is to the latter point that I wish to turn first, in particular in its expression in feminist theory, as an aid to understanding the controversy between the two schools that I have identified.[9]

Feminist political theory, often drawing on literary criticism of the last decade and the continental thought emerging from the Hegelian legacy, has asked on occasion whether feminist scholars can even write about the canon of classic texts without themselves being co-opted by the masculine language that has been used in those texts. Does the decision to study the text commit a scholar to a pre-determined masculine discourse? Should the old texts and the study of them, then, simply disappear because of their oppressive analytic frameworks which we must endeavor to escape? Linda Zerilli (1991) in a recent article in *Political Theory* raises issues about the attachment of political theorists to the canon of old texts and sees those texts as restraints upon transformative discourse. Focusing on an earlier debate between two feminist scholars, Jean Bethke Elshtain and Mary Dietz, about the proper reading of Sophocles' *Antigone*, Zerilli castigates each author for speaking in predetermined language. Elshtain (1982) had turned the powerful figure of Antigone into the expression of familial concern within the context of the city. Dietz (1985) preferred to see Antigone in her role as citizen concerned about the public welfare of Thebes. Calling on the language of the French feminist Luce Irigaray (but echoing the concerns I shall discuss below that Quentin Skinner has raised about imposing our own paradigms on the thought of the authors of the past), Zerilli raises the problem: "If Elshtain and Dietz translate, finally, the foreign, dissonant voice of Antigone into the more familiar, reassuring voice of mothers and/or citizens, it is because their readings have been 'framed,' so to speak, by the larger political conversations: a 'perennial' dialogue which requires that the authors articulate feminist politics by situating themselves in relation to the accepted terms of debate (public/private), and by defending a particular reading of canonical texts" (1991, 257). She proposes a new strategy of "mimesis" as a means of transcending "discourse which now admits women as conversants but whose unspoken symbolic terms require that, when women speak, they continue to disguise themselves as men and deny their origins in the house" (1991, 259).

Zerilli, by introducing these challenges to the study of texts, brings not only the deconstructive language of our literary siblings to the discussion; she raises as well the challenge first popularly posed by Carol Gilligan's 1982 book, *In A Different Voice*, the challenge of whether males and females speak differently on moral issues. Though a whole industry has emerged in the social science literature addressing the adequacy or

inadequacy of Gilligan's studies of young men and women, the broader issue that was raised forces readers of texts to question whether the gender of the author -- and perhaps even the reader -- determines the way in which that text can speak and the way in which it will be used. When Zerilli asks whether female scholars working in the field of the history of political thought are forced, as she puts it, to "cross dress," to talk in masculine language filled with "masculine images" which enslaves rather than liberates women, she is engaging in the broader debate about whether political theory is universal or particularized, or in Zerilli's and by extension Gilligan's view gendered, whether women authors reflecting on the principles of political life perceive those principles differently from men and whether women caught up in exploring the ideas of male authors become themselves the exponents of male values and the articulators of male perspectives.

The challenge posed is one that must be answered differently by the two major approaches I identified above and discuss below. If a text is particularized -- located in the gender of the reader or the author or in historical or linguistic space -- it will be lost as the speaker of a universal truth or as the object of commentaries through which we can educate ourselves and others. It will "sink into the landscape," in Zerilli's view, of body or of gender, as well as of language and historical circumstance. If the texts are to be read as universal educators, as the "instrumental approach" suggests, then they must abstract from the peculiar qualities of the author (and the reader) -- be they of historical place, gender, or psychological make-up. Feminist theory, drawing on what has come to be part of the post-modern discourse, has helped to clarify this opposition, making us confront the degree to which we need to see a text as embodied or abstracted from particularity. It is an opposition of underlying epistemological and methodological principles that needs to be remembered for the following discussion.

The "Instrumental" Approach

What I shall call the instrumental approach, without the pejorative overtone implicit in Gunnell's language, is captured in the language of the Preface of the first edition of the Strauss-Cropsey volume and reprinted in the second and third: "The authors and editors have done their best to take political philosophy seriously, assuming throughout that the teachings of the great political philosophers are important not only historically...that the questions raised by the political philosophers of the past are alive in our own society" (1987, xiii). Jacob Klein observed in the preface to his classic commentary on Plato's *Meno*, "In the past, for long stretches of time, writing commentaries was a way of expounding the truth. It may still be that" (1965, 3). This is not so much "hiding behind the text" (a not infrequent assessment of such commentaries) as allowing for reflection on the text to raise questions and pose possible answers that neither scholar nor reader may have asked before. The text, as an object, becomes the impetus to reflection, the spur to curiosity, and the commentary becomes the report of where that reflection and curiosity has led, as well as a guide to others about what they can learn from such a reading. The tradition of such writing is well rooted in a past filled with Biblical commmentaries; it is clearly secularized in the more recent traditions, but the expectation that commentaries can reveal important truths remains at the heart of most such commentaries today, though unfortunately few works can ever reach the depth of analysis that Klein achieved in his slim volume.

Klein, however, along with Strauss, was exceptional for making such claims in books written 20 or so years ago. The language that I will suggest is so prevalent today proclaiming the usefulness of texts for learning truths about politics does not fill the prefaces of some of the finest work in political theory from the period of the 1960s when positivism and behavioralism may have restrained more open claims, though such assumptions may have been implicit. Michael Walzer, for instance, modestly presents his classic *The Revolution of the Saints* as "a supplement [to] other interpretations of seventeenth century history" (1965, vii). He adds, "My only object is to make Puritan radicalism, so unattractive to my contemporaries, humanly comprehensible" (p. ix). It is we who must recognize the profound insights into the nature of religious movements, indeed all movements that entail self-sacrifice, inherent in his analysis. Though Judith Shklar's study of Rousseau, *Men and Citizens: A Study of Rousseau's Social Theory*, from 1969 has much to say about the psychological foundations of political life, she avoids any introductory assertions about "lessons" or "truths" and leaves it simply at: "Since the title of this book announces its contents quite accurately, I shall not detain the reader with any further explanations" (1969, vii). It is only in the brilliant postscript that she allows herself to reflect on why write such a commentary in the first place and why we may choose "to read and to respond to [Rousseau, Montesquieu, Machiavelli] and to the even more distant Plato and Aristotle with a vigor that few other authors have been able to arouse. In fact, they educate us and that is why they are interpreted and re-interpreted" (p. 218). Today, such language would be prominently and forcefully articulated at the beginning of any such book rather than appended as an apparent after-thought.[10] Joseph Hamburger's study of

Intellectuals in Politics: John Stuart Mill and the Philosophical Radicals focuses on the "ideas the Philosophical Radicals thought relevant to their activities as politicians" (1965, vii) and in tracing through the political odyssey of their principles, he does not ask us to assess or learn from those arguments as much as from the success and failures of political philosophy brought to the political arena. Moving even to books of the late 1970s we find classic works such as Julian Franklin's book on *John Locke and the Theory of Sovereignty: Mixed Monarchy and the Rights to Resistance in the Political Thought of the English Revolution* which begins with the claim only to describe and explain the "transformation in the theory of sovereignty which entered the modern tradition via Locke" (1978, ix). In giving this explanation, he turns attention to an author not part of the canon, George Lawson, whose work Franklin is eager to resurrect for its argument asserting the reversion of power to the people upon the dissolution of government. The book entails detective work, but in its praise for Lawson it also indicates Franklin's own interest in the power of radical democratic theory.

A canvass of the many books of commentary on the texts of the "constructed" and expanded core written by political theorists over the last decade frequently reveals a willingness to articulate strikingly different expectations about what either textual commentary or historical research and detective work will reveal.[11] There is now the open assertion that commentary leads to truth, whether the authors identify themselves with the "teachings" of Strauss or not, and that history can lead to a profound understanding of the possibilities or limits of our own political situations. Peter Euben, for example, in *The Tragedy of Political Theory: The Road Not Taken* (1990), a book that primarily works from ancient Greek texts, tragedies, and Platonic dialogues, begins by noting that perhaps because of Allan Bloom's best-selling book and I.F. Stone's *The Trial of Socrates* (1988) "issues central to our identity as a nation -- the place of education in a democratic polity...the question of moral relativism and the status of knowledge...are deliberated in terms of Socrates' death and Plato's *Republic*"; he goes further: "The principal object of this book is to consider Greek tragedy insofar as it provides a preface for understanding classical political theory and to suggest that the tragedians and those theorists provide in turn a ground for contemporary theorizing" (1990, 4). In contrast to any Straussian reading of the texts he studies, Euben finds in the classical authors the critical questioning of a culture's reifications, the theoretical awareness of the dangers of theorizing which "subjugat[es] or marginaliz[es] parts of the self or others or history" (1990, 304), all of which perspectives certainly derive more from Foucault than from Strauss. Thus, Plato's *Republic*, Thucydides' History, Euripides' *Bacchae* teach of the tragic recognition that is so much a part of our contemporary world: the certainties of the universal truths we so desperately desire may lead to indifference "to the particularities of time, place and person" (1990, 270). There are lessons to be learned, though certainly not Strauss's.

Stephen G. Salkever's *Finding the Mean: Theory and Practice in Aristotelian Political Philosophy* (1991) works largely through analyses of Aristotelian texts. The commentary he offers suggests "that Aristotelian theorizing can produce a more attractive and useful picture of modern liberal democracy -- its problems and its possibilities -- than those yielded by classical republicanism or individualistic democratic theories." Salkever returns to the Aristotelian conceptual structure because "it allows us to overcome our unhappy preoccupation with the distinction between public and private realms." The case he wishes to make is that "Aristotelian practical philosophy [is] a source of education...for a better understanding of ourselves as moderns and liberals" (1991, 8). Aristotle then becomes the guide to the re-examination of the place of virtue in a liberal democracy without the burden of republicanism. Or Thucydides' History in Steven Forde's *The Ambition to Rule: Alcibiades and the Politics of Imperialism in Thucydides* (1989) has an "educational task," namely to show citizens similar to the law-abiding, pious Athenian general Nicias "the inadequacies of some of their most cherished beliefs" (1989, 10), the profound contradiction between nature and politics, the contradiction between human virtue and morality and human nature and the political community. Forde's study of how Athens depends on Alcibiades becomes a discourse on a democratic regime's dependence on anti-democratic principles and frightens us into a recognition of the victories that may destroy us.

The "instrumental" approach is not, by any means, limited to those writing on the ancient Greek authors.[12] Indeed, hostility to the Greek philosophers explicitly informs some of the scholarship in this mold. Stephen Holmes's early essay worried precisely about the educative role that a return to Greek political theory might pose for modern liberal, highly differentiated societies. His essay "Aristippus in and out of Athens" published in the 1979 volume of the *APSR*, albeit overdrawn and a bit cranky, fretted about the normative power and appeal of the classical Greek authors as presented by Arendt and especially Strauss. The argument that if we allow the Greek texts to be our teachers, we face the worst sort of tyranny lies at the heart of Holmes's concern about relying on authors writing in a society so profoundly alien to our own. One is not just studying an historical artifact when one reads

Aristotle's *Politics*, Holmes suggested, or the interesting thoughts of someone who happens to have been particularly bright and perceptive about political matters. One is being educated in how to think about, and especially how to evaluate, the political world in which we live. This is a serious activity. The polemical tone of Holmes's essay is almost necessitated by the seriousness of the threat he envisions. Sabine's texts require no such polemicism.[13] In Holmes's subsequent book on Benjamin Constant, he makes clear that he writes about this thinker not only because of "the subtlety and sheer intelligence of Constant's thinking about modern society and the kind of freedom it makes possible," but because Constant's insights "suggest a major re-assessment of the categories that still dominate the debate about liberalism" (1984, 1-2). In particular, in contrast to his earlier assessment of the Greek authors, "Constant's political theory cannot be considered entirely passe" and we ought to take "Constant's position seriously even today "since his articulation of the key roles of political deceit and truth, of governmental neutrality, of public and private can serve as an alternative to both communitarian and anti-political ideals" (1984, 260).

Holmes uses his analysis of Constant to address the contemporary controversy in the wider field of political theory (and indeed in the public discourse in numerous widely read journals) between so-called liberals and so-called communitarians that characterizes much of the recent works on the great and sometimes lesser texts. One might see such writing as simply appeals to authority: Plato supports this and therefore (depending on our attitude towards Plato) we should (or should not) support that view too. Though this sort of argument may be characteristic of lesser studies, it does not capture the way in which the texts are used by those more skilled in analysis and commentary. The claim is not simply to give greater weight to one's own argument, but to help in analyzing the consequences and educating in the questions raised by the communitarian challenge launched against the liberals. One thinks here, for example, of Nancy L. Rosenblum's *Another Liberalism: Romanticism and the Reconstruction of Liberal Theory* (1987). Though hardly a close study of or commentary on text, Rosenblum draws on the arguments and insights primarily of nineteenth century European authors to argue for a "reconstructed liberalism" that acknowledges "historical individuals" and allows for the "affective, personal, and expressive" (1987, 3-4). Having introduced romanticism into liberalism and arguing that such an integration is possible without transforming liberalism into communitarianism, Rosenblum admits that her Romantic liberalism speaks to needs that are "historically contingent and not universal" (1987, 190). Here truths from textual study need not be true across time and space, but their contingency makes them no less significant, as Rosenblum sees it, for "justify[ing] political life and reconcil[ing] men and women to it" (1987, 190).

While Holmes and Rosenblum have explored the meaning of liberalism through commentaries on texts from the sixteenth to nineteenth centuries, defenders of a more communitarian vision of the political world or of one which encourages political participation and engagement in the public realm have turned to such authors as Aristotle or Rousseau to present "truths" about the possibilities of politics. Alasdair MacIntyre's *After Virtue* (1981) and Benjamin Barber's *Strong Democracy* (1984) are examples here. It took an article such as that of Bernard Yack's (1985) to suggest that Aristotle's polis was hardly the place where a seeker of community should turn or where "political friendship shares the character of personal intimacy" (1985, 104). Yack's commentary, beyond correcting a widespread misconception of Aristotle's political thought, warns about the tendency of "contemporary political theorists...to look to Aristotelian political concepts for a counterimage to liberalism's image of political society" (1985, 109). The correction by Yack is necessary precisely because he sees the inadequate use of texts for teaching values. If we are to learn from Aristotle, then we must read him for what he says, not for what we may want him to say. Commentaries must be truly commentaries and recasting an author to gives one's own view of an issue is merely to call forth witnesses, not to take the opportunity to engage in conversations such as Strauss enjoins us to do. The challenge that historians, literary theorists, feminist theorists, pose is whether such conversations are possible if readers are the creators of their texts or if the texts are dependent on the particularized gender or historical location of their authors or if the texts themselves enslave.

Use of textual analysis as a way of ferreting out the truth and of educating ourselves and others is hardly new. It extends at least as far back as the Platonic interpretations of the Homeric poems. But the explicitly didactic role of explications and textual readings in recently published volumes suggests both why political theory has been caught up in the so-called battle of the books or the crisis of the canon and why the behavioral and positivist attacks so prominent two decades ago appear today more quaint than threatening. With the demise of positivism and the demands for verification as the only philosophic stance for the human sciences, with the rejuvenation of normative discourse in a society concerned with the dangers of an unleashed science, abstraction from evaluative questions no longer can characterize the entire discipline. While social sciences have mostly moderated earlier claims of scientific

precision and have increasingly recognized the more subjective nature of the categories employed, political scientists in general and political theorists in particular are no longer willing to adopt uncritically the distinction of fact and value that controlled the social sciences for several generations and certainly did so when Sabine was writing his influential volume. Political theorists can now turn to texts for normative purposes as these texts are recognized as helping us to clarify our understandings of the good and the bad, the just and the unjust, the beautiful and the ugly.

The threat to this normative approach to political theory comes not from the exponents of a value-free social science as it did two decades ago, but from those who deny that the "texts of the canon" can be read normatively without commentators imposing their own views and paradigms, i.e., that we can ever be in a position to learn from these texts freed from the encrustation of our personal, social, political, and gendered histories. Rousseau in his search to learn who we are by nature had suggested we remember the statue of the sea god Glaucus which could barely be seen under all the disfigurements that seas and storms had brought to it over time. If we are, indeed, so laden with our history, commentaries cannot be "a way of expounding the truth" as Klein had suggested, but the simple expression of personal values imported back to the text commented upon. Insofar as we deny texts the capacity to educate and to commentary the search for truth, political theory reduces to either a subspecies of history where we engage in asking only what the questions were and deny that we can assess the status of those questions, much less the answers given, or it becomes a subspecies of certain forms of literary analysis and feminist theory where the text simply disappears and the reader, the subject, the centered self, replaces the author and all investigation of truth about politics -- or anything else -- becomes meaningless. Should this occur, the intense battles of an earlier generation when political theory confronted political science would fade not because of science's victory, but because of theory's withdrawal from the battle.

The "Historical" Approach: Critique, Transformation -- and Secession?

Texts in Context

Despite Sabine's introduction of the subfield of the history of political thought to the discipline of political science, the study of political theory remained part of a much larger and more ancient tradition of intellectual history. Studies of particular authors who wrote on political topics of their own time that may have had signficance for contemporary times; studies of concepts such as natural law, or representation, or justice that traced terms across the centuries; studies of the influence of one author upon another or of who said what first; all these filled the landscape of political theory. Interpretations were offered; clarifications of meaning identified; sequences of ideas followed. This tradition found a home in both history and political science departments. In the latter, behavioralism, focussing on current practices rather than institutions or the historical conditions that may have given rise to those practices, tolerated the history of political thought largely because of tradition. But it remained isolated, of little more than antiquarian interest giving a touch of literacy and sophistication to an article or essay, but hardly any significance to work in the discipline at large.

All such work, however, became the object of attack by what we may now call the "Cambridge School" or those who advocate the study of "ideas in context."[14] Cambridge University Press has recently embarked on a publication series called simply "Ideas in Context," edited by Richard Rorty, J.B. Schneewind, and Quentin Skinner. The policy statement at the front of the first volume articulates concisely the perspective of this school: "The books in this series will discuss the emergence of intellectual traditions....The procedures, aims, and vocabularies that were generated will be set in the context of the alternatives available within the contemporary framework of ideas and institutions....it is hoped that a new picture will form of the development of ideas in their concrete contexts" (Rorty et al. 1984, ii). We have here the definition of the new "historical" approach to texts, whether of political theory or not.

At an earlier time, such language about "ideas in context" would have evoked images of a Marxist contextualism that often finds in the classic texts an ideological content deriving from contemporary sociological, economic and political conditions -- and in a few books, such as those of Neal Wood, still does. Wood's Marxist contextual approach controls the 1978 book he wrote with Ellen Meiskins Wood, *Class Ideology and Ancient Political Theory*, a work which endeavors to put Socrates, Plato, and Aristotle into their social contexts and claims: "Their political thought can be conceived as the supreme intellectual expression of the increasing class consciousness of the aristocracy during the fourth century" (1978, 3).[15] Wood's Marxist contextualism, however, has not had the widespread impact of the new contextualism of the Cambridge School.[16] The new contextualism, as Wood recognizes and criticizes, is that of language and of agency drawing from what has been generally called the "linguistic turn" of philosophy during the last half century. While the

work of the historian John Pocock brought to the field of political theory the core principles at the base of the new contextual approach (Pocock 1962), it was Quentin Skinner's 1969 essay "Meaning and Understanding in the History of Ideas" that became the flagship statement calling for scholars "attempt[ing] to arrive at an understanding of the work" (1988, 29)[17] to embed a philosophical text in the discursive context of the age in which it was written.[18] Dismissing studies whose "whole point is conceived in terms of recovering the 'timeless questions and answers' posed in the 'great books' and so of demonstrating their continuing relevance'" (1988a, 30) Skinner argues that we must cast off the controlling paradigms of our own age which are inappropriately applied to the thought of the past and search instead for what, borrowing from J.L. Austin, he calls in the early essay, the "illocutionary force" of any text, i.e., the intention of the author writing such a text, something that can only be derived from a "delineat[ion of] the whole range of communication which could have been conventionally performed on the given occasion by the utterance" (1988a, 63-64). Such an approach can at most give us insight not into perennial questions, much less perennial truths, but rather into the "essential variety of viable moral assumptions and political commitments" (1988a, 67).

While this was a tentative first statement, more destructive with its arrows cast at virtually all previous work in the history of ideas (of which the history of political thought was but a part) than constructive, twenty years have brought its share of adamant adherents as well as critics. More significantly, though, there has been a refinement that sets the principles of this approach into a larger theoretical framework that addresses the role of language in the construction of our political environment. Skinner has moved to what he himself admits is a "less polemical statement of the central claims" found in his earlier "avowedly polemical essays" (1974, 278, 279) and acknowledges an interest in "the classic texts [as] worthy of study in themselves," (p. 279) though on the next page he worries about the "distorting perspective" they bring to our study of history. In language reminiscent of Sabine's dictum of including whoever said something interesting, Skinner comments: "To speak of a text as a classic is to imply that there may be special reasons for wishing to understand it" (p. 281). One cannot make that judgment, he suggests, prior to having ascertained the intention and context. In language that he himself does not use, but which we often hear from colleagues in the humanities, to judge a text as a classic is "to privilege" it without the certainty that it deserves that privilege. Or to borrow the image of the sacred geese on the Roman acropolis from the Dedicatory Epistle of *Leviathan*, we study texts not because they are they, but because they are there.

Skinner comes to question his earlier use of Austin's phrase "illocutionary force" by recognizing the complexity of trying to assign intentions on the basis of words. He claims (to cite again the prevailing jargon) that we need to ask what is the range of speech acts that can standardly be performed by a given writer when he makes use of a given set of concepts and terms (p. 289). With this perspective Skinner now claims to introduce the possibility of change and revolutionary force to the texts he assesses, always reminding us, though, of the necessity that "Every revolutionary is...obliged to march backward into battle. To legitimate his behavior, he is committed to showing that it can be described in such a way that those who currently disapprove of it can somehow be brought to see that they ought to withhold this disapproval after all" (p. 296). Skinner seems to be suggesting that somehow the author can, after all, rise above language as a restraint, but that to effect change, the speaker must speak in language understood by the listeners of his times.

The initial revision of his argument cited in the previous paragraph appeared in the 1974 volume of *Political Theory* which featured a symposium on Skinner's methodology. The two commentators included in the symposium were profoundly sympathetic to Skinner's contextualist approach, but complain that Skinner is too limited in his understanding of context. Jonathan M. Weiner takes on Skinner's innovative early work on Hobbes in which Skinner had argued that Hobbes was far less original or outside of common discourse than usually acknowledged and that he was indeed a participant in a particular intellectual circle where his political ideas were hardly radical (Skinner 1966). Skinner had thus suggested that Hobbes's ideas -- like them or not -- were simply not as offensive to his own generation as previous scholars had thought. Weiner's concern was with the limited nature of Skinner's "context," in particular that it avoided the social and the political, and that by understanding context only in terms of ideas Skinner may not provide an adequate basis for assessing the radical nature of Hobbes's thought. Gordon Schochet in his turn raised two separate questions: whether Skinner's goals of total immersion can ever be achieved and whether Skinner's assertion that the "tradition" is simply artificially constructed means that we cannot tell the difference between a greater and lesser work. This is a point to which I will return below.

By 1988 James Tully was able to collect from a wide selection of published criticisms a series of highly provocative and thoughtful essays on Skinner's method in

the Polity Press volume *Meaning and Context: Quentin Skinner and His Critics*. The epigraph for the volume comes from Wittgenstein's *Culture and Value* and captures the essence of Skinner's method: "Words are deeds." Though Skinner's essays in this volume build on the philosophic and literary examinations of such terms as "meaning" and "interpretation," he reiterates his commitment to focus on "not just the issues or themes with which the text is concerned" (1988a, 77), but to shift the emphasis off the "discussion of the idea of the text as an autonomous object, and on to the idea of the text as an object linked to its creator, and thus on to a discussion of what its creator may have been doing in creating it" (1988a, 78). After reprinting the most significant of Skinner's methodological essays, Tully prints the criticisms that confront Skinner with questions about how intent can be identified when the "real reasons" of an individual or a group are hardly accessible (Tully 1988), about whether his methods "render history gratuitously barren" (Femia 1988, 174), or whether Skinner has turned intellectual history into the history of ideologies and burdened and restrained it with excessive philosophical baggage (Minogue 1988).

Skinner defends himself in the final chapter of Tully's volume against the critiques printed in the volume and those published elsewhere in part by arguing again that the history of thought "should be viewed not as a series of attempts to answer a canonical set of questions, but as a sequence of episodes in which the questions as well as the answers have frequently changed" (1988, 235). The goal of recovering the meaning of a text itself, he asserts, must be rejected "in the name of the need to recover, at the same time, what the author of the text may have meant by it" (1988b, 282). In particular, he now responds to the "many critics" who have accused him of relativism, an accusation he rejects since he claims not to be interested in truth anyway: "I have merely observed that the question of what it may be rational to hold true can vary with the totality of one's beliefs. I never put forward the reckless and completely different thesis that truth itself can vary in the same way" (1988b, 256). Most significantly, at the end of his essay, he defends himself against accusations that his own work is motivated purely by antiquarian interests, telling us that the "alien character" of the beliefs of those whose ideas he studies "constitutes their 'relevance'." Reflecting on such alternative possibilities, "we provide ourselves with one of the best means of preventing our current moral and political theories from degenerating into uncritically accepted ideologies." His studies, he tells us, enable us to strengthen "our present beliefs by way of testing them against alternative possiblities, or else improving them if we come to recognize that the alternatives are both possible and desirable" (1988b, 287).

The language here, surprisingly, almost seems to recall Strauss's plea that we read with seriousness the texts of old so that we may see ourselves and the possible limits of our own beliefs. For Strauss "constant intercourse with the greatest minds...is...a training in boldness....the boldness implied in the resolve to regard the accepted views as mere opinions, or to regard the average opinions as extreme opinions which are at least as likely to be wrong as the most strange or the least popular opinions....Liberal education supplies us with experience in things beautiful" (1989b, 8). One has no doubt that Skinner would not particularly care to be associated with Strauss in this fashion and the differences between the two perspectives certainly cannot be denied. But the conversation that Strauss seeks is there in Skinner's most recent claim as he tries to move beyond historicism and antiquarianism. The profound difference, though, lies in that Strauss seriously expects the texts and the conversations they provoke to help in the recovery of the "beautiful," while Skinner poses this as only the faintest of possibilities and perhaps only to assure his readers of the relevance of his studies against accusations of mere antiquarianism. It says much about the more recent role of the history of political thought that Sabine would never even have felt the need to answer such concerns.

Amidst all the methodological essays expounding the need to study meaning in the context of authorial intent, Skinner has produced his two-volume work *Foundations of Modern Political Thought* (1978). The work carries out in part the promise to "surround these classic texts with their appropriate ideological context" as Skinner searches for the origins of the modern concept of the state. Nevertheless, while detailed summaries of the content of minor works are a large part of the study, these minor texts continue to circulate around just those major authors familiar to those who study the canon that has become traditional over the last five decades. The texts may begin to sink into the landscape of discourse, but they never disappear and we never lose sight of the text or the author. Indeed, on almost every page we learn of a text that may be of wide interest to us for more than its discursive content. Skinner's work, though, is not without its own problems of interpretations. Tarcov (1982) in his review of the treatment of Machiavelli in Skinner's first volume gives one of the numerous critiques evoked by the reading of texts Skinner offers. For Tarcov, Skinner takes Machiavelli too readily at his word, sees him as engaging only with his contemporaries and not with the political philosophers of the past; thus, by setting Machiavelli only in the context of his own times, Skinner fails to recognize the equivocations and paradoxes in the texts. The contextual method, as Tarcov analyses it, leaves the writing as timely only for the

writers' own time and not for ours. Skinner thus may fail to recognize what may be Machiavelli's intention or the true "illocutionary force" of his writings: to speak past his time and to us.

Apart from Skinner's and Pocock's work there are few examples of how all the methodological discourse about authorial intent and textual meaning affects the practice of study of texts. Strauss's approach to political theory, at least two academic generations older than the Cambridge School, has spawned an ever increasing number of works that pay homage to his methods of analysis and his philosophic concerns, revealing both the benefits of the careful and imaginative use of his approach as well as the profound defects of its simplistic application. We must await a comparable set of works before we can assess the success of the Cambridge School's approach and its potential contributions to political theory -- or its abstraction and secession from the enterprise of political theory. The evidence on this will emerge from the "Ideas in Context" series, the first volume of which was yet another collection of largely methodological essays by philosophers, historians, and political theorists about the possibilities of a history of ideas (Rorty et al. 1984). The second volume, *Virtue, Commerce, and History*, is primarily a compilation of Pocock's essays published from 1976-82 (1985). With regard to issues of direct concern to political theorists, there is only Pocock's book and David Lieberman's *The Province of Legislation Determined: Legal Theory in Eighteenth Century Britain* (1989). We are left to wonder, however, about the degree to which these studies, wonderfully intriguing with their detailed presentations of political language at work, help us address the questions political theorists address, i.e., the degree to which they will ever allow us to transcend the level of historical curiosity despite the methodological baggage. The richness of the works that have been written is truly impressive and the range of scholarship wide. Nevertheless, the methodological essays may raise the more theoretically interesting questions about the political power of language, its control and creation of environments, while that same methodology tells us that the vast scholarly endeavors cannot lead to theoretically interesting questions precisely because of their embeddedness in time and place.

Despite the ambivalence expressed in Skinner's response to Schochet's 1974 question about how to identify greater or lesser works, Skinner is part of an editorial team including Raymond Geuss and Richard Tuck that is editing the "Cambridge Texts in the History of Politics" from the Cambridge University Press. While the "Ideas in Context" series immerses the texts in the linguistic landscape, the "Cambridge Texts in the History of Political Thought" is a much more active and ultimately probably more important endeavor, making available to scholars and students a wide variety of "the most important texts required for understanding the history of political thought." The advertisement for the series does indicate that the "scholarship of the present generation has greatly expanded our sense of the range of authors indispensable for such an understanding" and that the series will include "a number of less well-known works, in particular those needed to establish the intellectual contexts that in turn help to make sense of the major texts;" so far, the series is giving us at a steady pace fresh, accessible, well-bound paperback editions of a large number of previously difficult to acquire, but key texts of "canonical" authors edited by top-flight scholars.

In an earlier article (1988) I made note of a Committee of Political Theorists constituted by the American Political Science Association that met in Washington in 1943 to discuss the state of political theory. That committee concluded that despite "broad differences" among political theorists about the principles and ontological foundations necessary for the study of political science, the committee as a whole was unified in its conclusions that the profession as a whole must continue to study Plato, Aristotle, Augustine, Cicero, and scores of others because "they make us conscious of the content of our own minds" (1944, 730). The specific, though rather mundane, concern that emerged from the meeting of these political theorists was the lack of current editions of the great thinkers such as Marsilius, Bodin, and Sidney for both teaching and scholarship. This was a problem to be remedied once the war ended. Now, almost 50 years later, that problem is finally being addressed most significantly by the Cambridge series (and, we must note, by the publications series of the Liberty Fund, whose volumes are not only well produced and well bound, as are the Cambridge University Press volumes, but also deliciously inexpensive).[19] Access to these texts should in fact do much to bolster the status of the texts. There almost seems something contradictory between the two Cambridge series, but perhaps the series of texts indicates, in fact, that the texts we use for reflecting on politics will hardly slide into the landscape and will instead remain at the core of the study of political theory, whether one adheres to the instrumental approach or not.

Variations on Texts in Context

John Dunn, identified by Richter, for example, as part of the Cambridge School (1986, 620), certainly an advocate of the principal methodological approaches of that school and included in compilations of scholarly work of that group, is willing to go beyond the recovery of the discursive meaning to claim that his scholarship

will help in the badly needed "rethinking" of political theory. Modern political theory requires this rethinking, Dunn claims, "because it is philosophically so feeble and politically so maladroit," a condition he describes as "becoming increasingly hazardous both for particular human collectivities and for the species as a whole" (1985, 1). His studies of Locke are to aid modestly in reversing this trend by exploring the source and the role of trust in Locke's political thought. While he finds that source in the "simple frame of Puritan values which Locke inherited from his father," Dunn goes further to claim "there is simply no conceptual truth in political theory more fundamental than the truth that men trust in what they can" (1985, 33). Indeed, in his essay comparing Locke, Hume, and Smith he recognizes in Locke's anguish about the possiblity of rationality without theocentricism that Locke points us to "an anguish which we still have coming to us." "It has taken us nearly three centuries to catch up with him" (1985, 67). However much Dunn may embed his Locke in his discursive history, Dunn still turns to Locke for the truths he may tell us about ourselves, our political possibilities and limits. The contradiction between text as educator and text as historical construct and datum seems to fade here as commentary turns into forewarning.

Don Herzog, who is not at all a part of the "Ideas in Context" perspective and who spends little time engaging in methodological debates, does illustrate in his *Happy Slaves: A Critique of Consent Theory* (1989) how the study of texts, of social history, of political discourse can all help us to reflect on the construction of some of the critical concepts of political theory, in his case, consent. His work emphatically denies the possibility of discovering truths in the texts we read, but he reads the texts as suggesting creative solutions to the political and social problems of the time in which they were written. Thus, he discovers in the text's confrontation with political circumstances aids in puzzling through the questions with which a political theorist must deal. Thus, for example, his reading of Hobbes ignores (for the most part, at least) the standard reconstructions of Hobbes's thought as self-interest properly understood leading to civil society; instead he finds that Hobbes's concern is to generate a language in which the arguments for disobedience disappear or become incoherent. Rather than searching for a consistency which Herzog asserts is not there, we must recognize that "Hobbes' brilliance lies partly in his ability to step back from political disputes...[H]e asks himself, what conceptual frameworks make these disputes possible? Then he tries to design new frameworks to replace old ones" (1989, 109). The reading of the text here is only part of the endeavor, one chapter out of several which are to explore consent theory, but by embedding the text in the political and social crises of seventeenth century England, Herzog is able to explicate both Hobbes and consent theory from the novel perspective which focuses on challenges to reasons not to obey rather than on reasons for obedience. The text here is neither an oracle of truth nor another datum in an historical tale, but a powerful prod to reflection on a key political concept.

Concepts in Context

Herzog's study of consent theory in Hobbes draws on the concept of consent in the seventeenth century, but Herzog is not interested in tracing the history of the concept; rather he wants to puzzle through the concept itself. A concern with conceptual history that may or may not lead to conceptual clarification for our own time, though, is re-emerging especially among a community of German scholars under the leadership of Reinhart Koselleck who is supervising the production of a multi-authored, multi-volume resource tracing the historical development of political concepts in Germany.[20] Melvin Richter in a series of articles (1986, 1990) has attempted to alert English and American scholars to the significance of this major cooperative endeavor that "provide[s] synchronic analyses of language, situation, and time, as well as diachronic analyses of continuity, alteration, and innovation in these political vocabularies" (1990, 42), and to suggest a possible compatibilty between the approaches of the Cambridge School and the German conceptual historians. Distinctive about this work in Germany is the project's concern with placing concepts, rather than texts, in context. While analytic and ordinary language philosophy had illustrated the inadequacy of certain political concepts by looking at them in their use in conversations, the German scholars see the concepts not only in the present; they are studied as well in their move across time. In this endeavor, the interpretive activity is less in the texts than in the language of the period and the social history under examination. Thus, we find attention to dictionaries, encyclopaedias, handbooks, and thesauri (Richter 1990, 47). Insofar as this project studies the language of politics, the questions that arise about intentions and illocutionary force characteristic of the Cambridge School or about truth and moral education that motivates Strauss and his students cannot and do not surface. Rather, as Koselleck himself puts it: "Concepts do not only teach us the uniqueness of past meanings but also contain the structural possibilities, treat the concatenations of difference, which are not detectable in the historical flow of events....It is only concepts that demonstrate persistence, repeatable applicability, and empirical validity...which indicate that a once 'real' history can today appear generally possible" (1985, 90).

Nevertheless, Richter sees this scholarly endeavor as a valuable, but largely unrecognized, resource for scholarship in the history of political thought: "Historians concerned with political theory written in English might well consider the feasiblity of an analogous enterprise...of such a conceptual history of political concepts in English. [This would offer] the possibility of engaging in a comparative history of leading concepts in political theory written in English, French, and German" (1986, 634). In asking Anglophones to follow the example of the German scholars, Richter envisions an integration of the methodologies of the German project and the interests of John Pocock and Quentin Skinner, or as he puts it: "How compatible are these German and the Anglophone modes of treating political language by rigorously historical methods." The compatibility lies in the benefit the Anglophones would get from the German method of "non-reductive use of social history" and recognizing units larger than the individual theorist or school of thought, while the Germans would learn from the Anglophones the importance of the issues raised about "political thought and theorizing as forms of linguistic action" (1990, 69). In proposing this mutual education, Richter appears to forget the "text." While Strauss and Skinner certainly approach texts in a diametrically opposed fashion, they still acknowledge the primacy of the text as the window through which they discover their respective goals; for the German school, the text becomes just one of many resources for discovery of the conceptual history.[21] The text is neither an actor as in Skinner's understanding, nor a conversational partner who will lead us to question and grow morally and philosophically as in Strauss's. The text becomes only datum.[22]

Terence Ball, nevertheless, tries to follow through on Richter's plea in his book *Transforming Political Discourse* (1988) where he makes an effort to integrate all three of the above approaches, without fully acknowledging the theoretical oppositions. Arguing that our "political discourses help to transform us making us who and what we are as political agents and citizens...[A]s the concepts constitutive of our speech change, so do we" (1988, ix), he suggests the power of language to control us. In contrast to Strauss here, the language does not illuminate or reveal truths freeing us from accepted opinions, nor does it enable us to reflect more deeply on the puzzles of political concepts. Rather, we become in Ball's argument creatures of our own language instead of its creators. For precisely this reason he is interested in the transformation of language and applauds the fact that, "[a] static and ahistorical view is at last giving way among Anglo-American philosophers to a more historical approach to the study of language and in particular the language of political theory" (1988, 6).

Skinner's and Pocock's approach presents a host of challenges to political theory if it is to remain a part of political science. If the study of the text is simply to learn of authorial intention, does it give us insight into political phenomena? Does it aid us in the exploration of the normative goals of the political communities? The approach is explicitly hostile to the normative concerns of political theory since the texts give no lessons about good and bad, just and unjust, only about the way such language may be used by authors. Meaning is not plumbed for truth, but for intent, and the question may rightly be raised about what this has to say to political scientists. Ashcraft's work (1986), which goes beyond authorial intent alone, though that is very much part of his endeavor, to the exploration of political movements and the political theorist as a participant in those movements, goes back to some of the issues that made Walzer's book on the Puritan saint a classic. But without that sensitivity to the potential political consequences of texts and their engagement in the crises of the time, the danger exists that the study of texts as part of discursive contexts will look in upon itself and remain a subfield isolated in history departments away from and not speaking to the concerns of political scientists or political theorists. Strauss's approach, with its numerology and searches for hidden meanings, with its elevation of ancients over moderns, with its fierce challenge to a study of politics he saw as eviscerated by social science, provoked firestorms of controversy and made its presence vividly felt in the discipline for better or worse. Text as historical datum sinking into the landscape allows the political scientist to ignore the normative issues raised by the texts.

The Texts, the Canon, and the Question of Gender

Linda Zerilli's article challenging feminist scholars to dismiss the masculine texts and discourses that imprison us in a traditional world view can help to frame the discussion of the two dominant strands of political theory by questioning the possibility of an abstract truth such as Straussian commentary strives to achieve and by making radical claims about both the text and the reader as enmeshed in time, place, and gender. By even raising these broad methodological questions, Zerilli's article illustrates how significant a transformation has taken place in the study of gender and the history of political thought over the last two decades. When feminist concerns first hit political theory in the 1970s, the goal often was to document the dreadful history of misogynist statements by one male author or another, statements that have served to justify the exclusion of women from the

political realm and confine them to the private world of the family. Plato (everywhere except in Book V of the *Republic*), Aristotle, Aquinas, Machiavelli, Rousseau, Hegel all were ready targets for such attacks. The title of a 1979 edited volume, *The Sexism of Social and Political Theory: Women and Reproduction from Plato to Nietzsche*, captures well the tenor of such work.[23] Over the last decade, however, feminist scholars have engaged the texts of the past far more seriously -- both to learn from them and to understand how these texts might give insight into the presumptions underlying the liberal, democratic regimes. While American feminist scholars in political theory have not for the most part changed so much that the texts formerly castigated as oppressive documents responsible for all womankind's suffering are now wholly welcomed, the vitriol is gone. A recognition of the need to learn from these texts and what I have labeled the "instrumental" approach surfaces more often than not; the acknowledgement that introducing questions of gender can yield new insights into our political understanding characterizes much of the recent work in this area. Zerilli's concern is that such scholarship traps women in the vise of masculine political concepts and language.

My own work (Saxonhouse 1980, 1984, 1985, 1986) has aimed at turning attention away from the theorist bashing language of the earlier generations of feminist scholars to show how attention to gender can give us new insights into the arguments of authors from the Greek playwrights to Machiavelli. Those new insights, I argue, enable us to recognize the dangers of exclusion, of demands for uniformity, and the dismissal of the private as relevant for analyses of political life. Thus, the ancient authors may illuminate for us the limitations of our use of gender in our understanding of political life. Similarly, Jean Elshtain in her *Public Man, Private Woman*, the first half of which traces the concepts of public and private through the history of political thought, begins, "To condemn or praise the thinker and ignore the thought is not the way a theoretical mind works" (1981, xv). In urging us to be theoretical, Elshtain urges us to read in order to learn, e.g., from Plato or Aristotle or Augustine, what each one has to say about the relation of the family to city, rather than simply to condemn.

Wendy Brown in her work *Manhood and Politics: A Feminist Reading in Political Theory* (1988), which takes a profoundly different perspective than mine and Elshtain's, notes as well, however, "Certainly it is easy enough to declare every well-known political theorist 'wrong' on the nature of man, manhood, woman, and womanhood. Such a declaration is also uninteresting. Far more interesting and important are the sources, permutations, power, tenacity, and implications of such formulations" (1988, 14). Brown's work ranges chronologically from ancient Greece through Weber looking not so much for the exclusion of women themselves from politics and their "relegation to subordinate spheres and statuses." This is well-known. Rather, the texts she studies "offer a rich articulation of masculinist public power, order, freedom, and justice" (1988, ix). Accepting this richness of the texts of political theory, she argues that the politics she finds in the great texts over the last two and a half millennia are "gendered" in their alienation of the head (male), which is intellect and reason, from the body (female), which is dirty necessity. She calls for the re-integration of the body into the political world. The texts in the analyses they receive from Brown give us no universal truths; rather, they have been responsible for profoundly disturbing divisions within society and yet "from each [of the texts] we can learn not only what is wrong with a politics rooted in a masculine tradition, but what is compelling about it such that it calls for transformation rather than thoroughgoing condemnation" (1988, 7). Brown's plea for the re-gendering of politics by giving it a "feminine" body may obfuscate a need we have to remove from our discourse the language that gives genders to such concepts, but her argument forces us to return to texts and their language, to re-think our use of gender-laden terms. In Zerilli's understanding, however, Brown is still captured by the masculine discourse, relying on male texts to set the agenda for her reformed politics.

In the mode of studying male authors from the perspective of gender, not to condemn but to learn, Hanna Pitkin (1984) offers an extensive analysis of Machiavelli's texts since, as she puts it, "For me, thinking about politics and thinking about Machiavelli have become interconnected enterprises, each illuminating and obscuring the other" (1984, 3). She goes on to describe the "dual intent" of her book making clear her own adherence to the instrumental approach, despite the gender and misogyny of Machiavelli. Her book is to address two issues: "The narrowly specific one of interpreting a particular thinker on the basis of a body of texts and the more general one of understanding the subject matter those texts address: ourselves as political creatures" (1984, 3). But to engage in this discovery about ourselves as political creatures, Pitkin insists that the category of gender be introduced and that Machiavelli's insights and the lessons he can teach us depend on "his anxiety about being sufficiently masculine." Thus, her study is permeated with the images of women that lace Machiavelli's pages whether in the political treatises or in the comedies or in the histories and biographies. Pitkin's concerns are the profoundly political concerns of citizenship and foundings. But neither, as she learns from her reading

and analysis of Machiavelli, can be addressed without attention to gender, as the political concepts (well beyond the dominant one of Fortune as a woman) of public and private, mastery, autonomy all appear to depend on perceptions of the relations between the sexes and in particular the fear of the female. Pitkin's work is among the most profound of this genre, making evident the gender relations previously unrecognized in studies of Machiavelli, and, more importantly, making clear the significance of those relations. Studying the possible sources of Machiavelli's language, she does not allow the language or the texts to "sink into the landscape;" rather, they reveal the possibilities of a collectivity of civic virtue as well as the profound dangers that misogyny may pose to that collectivity and a "best" political world.

Carole Pateman in *The Sexual Contract* also returns to the classic canon of political theory, not so much to learn about ourselves as citizens as does Pitkin, but to uncover gender-related terms so as to reveal the sinister hidden assumptions lurking behind the forcefully articulated arguments of some of the heroes of liberal theory. Writing primarily on the origins of liberalism in the works of the seventeenth century, she discovers there what she describes as the "repressed dimension of contract theory" (1988, ix), namely the sexual contract that is the title of her book. Analyses of the writings of Hobbes, Pufendorf, Locke, and Rousseau suggest that a hidden or unacknowledged conjugal contract, which does not depend on equality, precedes the social contract. This means, according to Pateman, that women are never participants in the social contract and therefore must continue in liberal society as subordinates even as men are free. These studies yield insights into the origins of what she calls modern patriarchy where men remain masters. In her more detailed discussion of Hobbes' role in the construction of modern patriarchy (1989) she carefully glosses Hobbes' text to draw out the inherent difficulties in Hobbes' theories for a contract theory that avoids the assumptions of male domination over the female. Pateman's approach, while reminiscent of the earlier theorist bashing noted above in its condemnation of liberal theorists for not providing for a truly equal and free political community for women, goes further than the early efforts and, whether or not one agrees with her conclusions, shows, as Catherine Zuckert has put it, "how deeply rooted, far-reaching and complex the problems are" (1990b, 662).

To the degree that scholarship concerned with gender has remained wedded to texts in political theory, it has often clustered around particular figures in the canon. By his almost unique willingness to write in favorable terms about the prospect of women as active participants in the political realm, John Stuart Mill has been the focus of considerable scholarship, both favorable and critical, by feminist scholars. When he published his *On the Subjection of Women* in 1869, he became the first major political theorist from the traditional canon since Plato in the *Republic* to welcome the prospect of women as equal participants in the public life of the community. Nevertheless, while Mill's *On Liberty* and *On Representative Government* and *Utilitarianism* all were widely acknowledged as part of the discipline of political theory, important works to be included in any tradition of political theory texts, it was only in the 1970s with the rise of the most recent feminist movement that *On the Subjection of Women* gained recognition as a text worthy of consideration and perhaps as a contribution to our assessment of the role of women in the life of the political community.[24] Though the essay has been subjected to criticism for its failure to carry the principles of equality in the public sphere back into the private sphere (Okin 1979, chap. 9) or its methodological commitment to an "impossibly 'pure' empiricist methodology" (Ring 1985, 28; also 1991, chaps. 3, 4), it was also welcomed as an all too unusual expression of theoretical interest in the public role of women. Mary Lyndon Shanley (1981) found in it a plea for the equality and friendship in marriage that had not previously surfaced within the tradition of political theory.

While Mill and Plato have been the objects of much debate among those interested in gender and political theory, virtually all of the others included in the traditionally constructed canon have been subjected to similar analysis. A recent volume, *Feminist Interpretations and Political Theory*, edited by Shanley and Pateman (1991), reprints a whole series of articles and chapters written mostly during the 1980s on authors from the "canon" -- Plato, Aristotle, Hobbes, Locke, Rousseau, Hegel, Marx. Analyses of these authors are expanded by the introduction of gender into the way that they are interpreted. No uniform or dominant readings characterize these essays as did the essays included in the 1979 *The Sexism of Social and Political Theory* mentioned above. Rather, the implications of an author and his or her arguments for gender issues are extracted and the consequences of ignoring gender explored.

The introduction of gender into the readings of these texts, though, is not necessarily to bring to them questions that they did not address. One could argue that the study of politics necessitates an awareness of gender insofar as politics entails the distinction between public and private (or the denial of such a distinction) and that the liberal forgetfulness of gender, while not necessarily implying Pateman's sexual contract, blinds us to the multitude of issues raised by the public and private spheres and by the questions of difference in a political theory founded on fundamental principles of equality. Jean Elshtain's 1981 book, *Public Man, Private Woman*,

set the tone for this investigation of theories articulated in the works we read when she prefaced her own study by commenting: "Although few of the thinkers I shall examine felt compelled to explain or speculate as to why a public-private demarcation had emerged in the first place, each seems under an obligation to justify and to explain the particular division that exists in his own epoch and sustains ongoing social forms, whether to defend it or as a contrast to his own alternatives" (1981, 9). Or Jennifer Ring's more recent work (1991) shows how the epistemological principles of any political theory must confront the challenge of gender and the claims of some feminist writers that gender entails differences in the manner in which each sex is able to know. Are there different ways of knowing and thus different understandings of political life? The analyses of Mill, Hegel, and Marx in Ring's book focussing on whether there is a "gendered" epistemology again suggest how far we have moved from simply pointing the finger at the misogyny of the past.

It is not that the questions, let's say of public and private and epistemology, are being imposed now that we have discovered gender as a significant category, bringing, as e.g., Skinner might say, our own paradigms to the texts we read; rather, crises of modern times enable -- or force -- us to recognize issues. We then find in the works of the great philosophers not that these issues are the new construction of modern times, but that the previous generations of scholars working in the field of political theory have simply not bothered to ask questions that the authors of the classic texts had asked themselves. Pateman herself notes this problem; having written about political participation (1970) and political obligation (1979) through reflection on writers such as Hobbes, Rousseau, and Mill, she writes in the preface to a recent collection of her essays how some of those essays, written before she recognized the importance of gender, remain framed within the traditional masculine assumptions about the character of political theory. We may be in a position to learn from the authors of the past because we are prepared to ask forgotten questions, whether about gender, opinion, justice, equality, and so forth. It is only by the careful reading of the texts that we can learn whether those before us posed these questions and what their answers might have been. Collingwood warned about bringing our questions to the past, but changes in our own social structures may enable us to recognize questions asked previously which we ourselves have denied as legitimate or, more likely, forgotten how to ask. To make this argument, though, we must reject the principle underlying Zerilli's critique, namely that there is a different discourse for male writers and for female writers, that women caught up in exploring the ideas of male authors become themselves the exponents of male value and the articulation of male perspectives. The writings of scholars such as Pitkin on Machiavelli suggest how much will be lost if we allow ourselves to read these texts as particularized according to gender and race.

Pateman concludes her essay on Hobbes with the telling quote from the eighteenth century feminist Mary Astell: "If *all men are born free*, how is it that all women are born slaves" (1989b, 463)? Pateman's reference to Astell raises the question, but does not answer, whether women authors reflecting on the principles of liberal theory at its foundings perceive those principles from a fundamentally different perspective. In the volume of essays that she and Shanley have edited, Plato, Aristotle, and Rousseau are side by side with Mary Wollstonecraft, Simone de Beauvoir, and Hannah Arendt, authors unnoted by Sabine or Strauss and Cropsey. The structure of the Shanley-Pateman volume thus introduces the female voices, but does not deny the need to engage the male authors as well, nor does it raise the worry about the co-optation that may come from such engagement. The recovery of forgotten women theorists has meant the addition of new voices to the general discourse of political theory and not just the recovery of a feminine voice. Mary Dietz's work on Simone Weil (1985) and the multitude of recent writings on Hannah Arendt suggests that the study of these female authors has far less to do with gender than with the larger (can I say, universal?) questions that confront all students of political theory concerned with violence, political action, political will, and so forth. The challenge, however, does remain to integrate authors who because of our own limited vision of whose works deserve study, whatever time, place, or body they may inhabit or in whatever genre they may write, have not been part of the discipline, though they may have much to say to us about our political world. The concern among some feminist scholars has been with those who have been unfortunately excluded only because of gender. Their inclusion ought not be based on gender, but on their ability to make us engage in conversations from which we can learn.

The plea, now often heard, to include women authors, whether because they speak with a different voice or not, makes us consider as well the more general question of who -- and what -- is to be included in the study of political theory.

Who's Included

In writing his book on the history of political thought, Sabine chose to include whoever said interesting things about politics, and he produced a long list that includes minor authors of minor pamphlets next to authors, who, however much they may be liked or

disliked, are recognized as profound thinkers. There seems to have been little self-conscious reflection about who would make it into the history of political thought and who would be left out. Gunnell's concern about Strauss was the opposite, that the inclusion was too self-conscious thereby creating a false sense of a gigantomachy between moderns and ancients that never took place and thus idiosyncratically excluded and included texts. Skinner's methodology denying privilege to one text over another raised the question of how we might go about deciding which texts to study if we did not have the capacity ahead of time to identify those which were worthy of such study. It poses the problem stated so well in Plato's *Meno*. If we do not know the truth, how will we recognize it when we find it?

How, indeed, do we identify out of the vast philosophical and political literature of our intellectual past those texts which we ought to study, whether we wish to use them "instrumentally" as sources of education and moral training, or to set them into their historical and discursive setting? Allan Bloom, dismissing with an exclamation point any talk of canons, responds to this issue of who ought to be read by serious students by looking to the list of texts "generated immanently by the [great] writers themselves" (1980, 123-24). Spinoza supposedly alerts us to Machiavelli, and Hobbes to Aristotle, and more significantly (since we would most likely take Machiavelli and Aristotle seriously anyway), Rousseau to Xenophon. But Bloom's method here is not very satisfying since it leads to the exclusion of those who may not have been readily accessible (e.g., Antiphon) or who may have been dismissed for frivolous reasons, such as being of the wrong sex, to have been taken seriously as an author even by the "great writers." Nor does this help us to clarify where this chain of "greats" begins. Strauss himself was aware of the problem of generating this list of whom to include in our conversations when he commented: "It is merely an unfortunate necessity which prevents us from listening to the greatest minds of India and of China; we do not understand their languages, and we cannot learn all languages" (1989b, 7). This he wrote in 1959 well before the calls for multi-culturalism in the university became the coin of common discourse. Were we to have access to those works, we should read them alongside those from our Western culture. This seems to be one of the few programs set forth by Strauss not followed through by his students.[25] Strauss further warns us that while "we may think that the possible alternatives are exhausted by the great thinkers of the past...we cannot exclude the possibility that other great thinkers might arise in the future -- in 2200 Burma -- the possibility of whose thought has in no way been provided for in our schemata" (1989a, 30).

Admittedly we cannot look that far into the future, but still characteristic of the study of texts over the last decade or two has been the enormous expansion of texts beyond the Hobbes-Locke-Rousseau schema, not so much in terms of non-Western authors as Strauss himself had envisioned, but in terms of previously unstudied Western authors and, in particular, literary genres usually assigned to other academic disciplines but now recognized as part of political theory. Whoever says interesting things about politics need not so identify himself or herself as doing so. In part, it was the work of Strauss himself that led to this vastly expanded corpus. Sabine's list was long and apparently inclusive, but because of his narrow understanding of the meaning of what is political, the authors he included were narrow in their approaches and understood politics only in terms of public affairs and openly acknowledged public concerns. Strauss's willingness to expand the object of political philosophy to the considerations of the best life, of the gods and politics, of virtue, of nature, of family enabled him to resurrect authors who had been dismissed as second-rate (Xenophon), as belonging to a different discipline (Thucydides), as merely comic, bawdy playwrights (Aristophanes), or as from so different a culture they were beyond the interest of students of Western Civilization (Al-Farabi). As his study of Plato's *Euthyphro* shows, a Platonic dialogue that appears to be concerned with piety really enables us to consider the relation between the legislative art, the gods, justice, and the "half-truth" of the gods as superfluous for legislation (Strauss, 1988a: Chap. 8).[26] Or he explains how a comedy about ancient tragedy (*The Frogs* by Aristophanes) is really about the the poet's role as the educator of citizens (1966, chap. 8).

We can find evidence even in the *American Political Science Review* that the list of texts now read as political theory has expanded dramatically. There now are articles on Homer (Dobbs 1987), on Thucydides (Orwin 1984, 1989) and on Aeschylus (Euben 1982); none of these authors made it into the pantheon of political theorists identified by Sabine in 1937.[27] Peter Euben, justifying the publication of a collection of essays on the ancient playwrights entitled *Greek Tragedy and Political Theory*, writes, "As the very idea of juxtaposing Greek tragedy and political theory implies, I regard the former as analogous to, if not itself a kind of theoretical activity" (1990, xi). To study ancient tragedy is to practice political theory precisely because those tragedies written as a lesson for the citizens of Athens force their audiences, whether in the modern or the ancient world, to question the most basic principles of human organization and justice. The *Oresteia* brings us face to face with the dreadful need for, as well as the dehumanizing consequences of, the judicial system, or Euripides' *Ion*

explores the forces that drive political systems towards xenophobia, or his *Bacchae* points to the problem of defining membership that confronts all communities, political or otherwise.

Similarly, novelists and playwrights help Judith Shklar on her ramble through the issues she confronts in *Ordinary Vices*, a work which builds primarily on reflections on the essays of Montaigne (who also failed to make it into Sabine and the Strauss and Cropsey compendia). Stephen Salkever's analysis of Aristotle (1991) introduces a discussion of E.M. Forster's *Howards End* to develop his analysis of Aristotle's understanding of the relationship between psychological theory and political practice, thus bringing together a twentieth century novelist and a fourth century B.C. philosopher to teach about the relations of political practice and practical wisdom.[28] And beyond the plays and the novels we find published in *Political Theory*, George Kateb's analysis of Walt Whitman's "Song of Myself" as a poem that makes us aware that individuality conjoined with connectedness is necessary for democratic society. The boundaries that previously defined whom to incorporate in the schema of political theory discourse now seem to have disappeared, and we are left with only the imagination of the current practitioners as the limit on which authors and works are to be plumbed for their insights into the problems of political life.

Skinner's work has meant the expansion of sources in a quite different direction. He has turned not so much to alternative literary genres, but to authors who may not be the first-rate minds with whom one would want to carry on a Straussian conversation, but who become part of the corpus because their writing gives insights into the use of language at the time at which they wrote. Virtually every page of Skinner's two-volume *Foundations of Modern Political Thought* is filled with summaries of authors known only to the most erudite scholars. Meanwhile, the German school of Koselleck is bringing in dictionaries and documents that make no pretense to argument but only serve as historical resources for study.

The expansion in genres studied, the recovery of authors previously forgotten (or consciously ignored) by political theorists is matched as well by the emergence of twentieth century authors whose works have come to serve the same role as the texts studied by Sabine and Strauss and Wolin. Strauss's works, his commentaries, his essays, his speeches, have themselves become the object of intense analysis. Drury's (1988) attack is thus far the only book-length analysis directly on his writings, though *The Review of Politics* (1991, 53, 1) put out a rare special issue devoted exclusively to articles about Strauss's thought. In a prefatory note by the editors, they note that "over the past decade, scholars of his writings have inquired of his writings whether his teachings contribute to a more 'free' society, to a more 'responsible' society, or a cult of 'godlike' philosophers" (Deutsch and Nicgorski 1991).[29] Others of the twentieth century whose works have become part of the conversation of political theorists and their writings are becoming the object of the type of textual engagement previously reserved for authors of the stature of Plato and Hobbes. The next decade will reveal whether they continue to reward this level of intellectual plumbing for meaning and insight. Most conspicious here are those -- Heidegger, Arendt, and Foucault -- who have challenged any complacency engendered by enlightenment philosophy, who have taken the modern world to task for its political and epistemological assumptions. The challenges are serious -- and often frightening -- but to understand those challenges requires serious engagement with the texts of these authors. That is beginning and making its way to the center of the subfield. While at least with Heidegger and Foucault much of the academic attention has come in other disciplines (especially philosophy and literature) the importance of their thought as a challenge to the traditionally accepted modes of political action, of political analysis, of textual analysis is confronting the discipline at large.

Without avoiding or excusing Heidegger's support of National Socialism in the Germany of the 1930s, authors like Dallmayr (1984), Newell (1984), Gillespie (1984 chap 5), Zuckert (1990c), and White (1990) have been and are exploring how Heidegger's works help us grapple with the modern political experience of nations, community, freedom, technology, and science in a world that must be defined by time and by history. Studies of Arendt are multiplying almost daily. There may be something of a desire among feminist scholars to bring Arendt into the pantheon of male authors as a female intruder, despite what may be seen by some as the masculinity of her language and her thought, but there is as well the widely acknowledged power and clarity of her vision of the crises of the modern world and a sympathy for her attempt to re-introduce a politics of action and speech into an unadorned world of labor.[30] While Foucault's books, essays, and published interviews confront us with the serious challenge of whether texts can have any meaning that does not derive from a set of power relations outside the text itself, the challenges he has posed go well beyond the problems of textual meaning to questions of power, epistemic and linguistic, questions of the meaning of subject and actor, of freedom and discipline, and much more. He thus presents us with challenges to the very activity of reading in the field of political theory, of engaging in any analysis of power and social relations, of evaluating social practices and regimes. The very

meaning of, responses to, and effects of those challenges has prompted the exploration of how we are even to read the texts he has left, as political theorists are beginning to bring him into the ken of their interpretative efforts (e.g., Phelan 1990; Miller 1990; Thiele 1990).

The expansion of the corpus in all directions is important for deepening our knowledge and resources. As with every field of study, however, as it expands the practitioners become more and more separated and conversations among scholars more difficult. A smaller core of texts means a more focused conversation among political theorists, but also a conversation that can become constricted and narrow, looking only in on itself. The wider the core of readings, the more difficult it becomes to carry on the conversation. Insofar as political theory is conversation about the just and the unjust, the best regime and the worst, legitimacy and illegitimacy -- the expansion of readings and the possibilities of new understandings of these questions are to be welcomed. The challenge, though, is to maintain the conversation despite the expansion. It is not clear that the last generation of scholars has been able to achieve this.

Conclusion

R.G. Collingwood, faced with the ugliness of the Albert Memorial, was forced to confront the meaning of beauty for himself and for the architect of the memorial. He claimed that he could not understand the memorial or appreciate it if he used his own standards of beauty; he needed to understand how the memorial met the standards of a different time and a different person. In this way, perhaps, he learned the limits of his own conception of beauty and was led to discover a beauty that transcended the expectations of his own set of experiences. The texts that we include in the study of political theory likewise cannot be understood insofar as we go to them only with our own questions and our own standards. To this extent we must be able to set them in a context, to see them as having authors, as speaking in a language that a particular set of a past population could understand. But this need not leave them as simply artifacts of the past -- curiosities of long faded values. Collingwood recognized that Scott's standards of beauty may not have been his own, that the role of this memorial may not have matched his own conception of that role, but in so doing he need not learn the relativity of all standards of beauty. The challenge may be Socratic: to raise questions about the opinions precisely by showing that those opinions may not be universal. But as with Socrates, to learn that our truths may not themselves be universal does not lead to the impossibility of searching for truths. It leads rather to the *aporia*, the admission of ignorance, that is at the foundation of all learning.

To be able to learn about the limits of our opinions we need to understand the context of the works we read; if we do not, we are left with our questions unanswered. Despite the attacks on texts launched by those critical of their "gendered" language, of their status as object rather than subject, of their psychological and historical distance from us, we have seen no lessening over the last decade of commentary on and engagement with texts. What we have seen is the increased willingness to acknowledge the educative role of these texts -- a role that may make them more dangerous than they were when they were read simply to set the record straight about who said what, when, and first. Socrates was executed by the Athenians for corrupting the young as he goaded them to ask new questions of their established norms and perhaps find those norms wanting. The return to texts for this normative education does entail a certain degree of danger. Though theorists today have no illusions that the hemlock is likely to confer martyrdom's immortality on them, they are prepared to confront the profoundly discomforting conclusions that a Nietzsche or a Heidegger may force us to acknowledge, the undermining of the enlightenment foundations of knowledge and the political consequences of that critique. If anything, it is likely that the next decade will see continued and increased attention to precisely those texts that do force us to question some of our most casually accepted assumptions. It is the unsettling quality of these texts that make them far more powerful educational tools than perhaps even Matthew Arnold envisioned in his pleas for a literary canon. What has appeared to be given in any society may not be; but that is precisely why we read these texts and why they are at the center of so much academic debate of late.

On the other hand, the historical debates that set those texts into a discourse of a time and place so distant from our own, that assume fundamental discontinuities rather than continuities, that force us to spend our time learning a new language can make these works seem sterile. Yet, as Strauss recognized and as Collingwood warned us, we cannot read those texts without language skills, broadly conceived. Sophocles' *Antigone*, for example, is not about fighting for the rights of self-expression of the individual, as it is far too often understood to be. To read the play in that way is not to learn from Sophocles. But if we know nothing about the absence of a language of individual rights to self-expression or of the centrality of the conflict between household and the establishment of the polis in ancient Athens, we may make this mistake. The overwhelming challenge for the next generation is then to resist the

appeal of a too comfortable isolation between the two major approaches to political theory -- the first, in which commentary fails to acknowledge that the lessons of any text will be bound up in the language and conceptual framework of particular times, and the second, in which history will diminish texts to mere historical data lacking the powerful educative role they have played in the past. While we may leave the Cambridge School to debate the methodological possibilities of recovering the "illocutionary force" or "meaning," we ignore to our detriment their guidance about language.

For sure, to read and spend our time reflecting on and writing commentaries about a Plato or a Thucydides or a Nietzsche or a Simone Weil represents a leap of faith that these works will repay our efforts. At times we may be disappointed. But to deny the possibility that we may learn is to lose an opportunity to rethink our opinions and to forget what scholarship and education is all about.

Notes

Thanks for help on this essay are owed to a number of people. John E. Jackson's skepticism about texts helped me define the initial structure of this essay and his comments on an early draft helped me refine it. Don Herzog, Tim Fuller, and an anonymous reviewer gave sage advice about some inaccuracies and made me excruciatingly aware of all that I have had to leave out. Shanetta Paskel provided invaluable bibliographic help.

1. Gunnell introduces his contribution to the first edition of *Political Science: The State of the Discipline* by distinguishing between PT (political theory as a subfield of the discipline of political science) and pt (political theory as a more general interdisciplinary body of literature, activity, and intellectual community) (1983, 3). If anything, the interdisciplinary nature of political theory has expanded enormously in the last decade.

2. There are two chapters in this volume dealing with political theory. The subfield does not divide itself easily -- or indeed at all. The artificial line drawn between the two chapters is contemporary and perennial issues. Clearly the political theory confronting the contemporary political world draws on the traditions articulated in the history of political thought and the way in which we read that history depends on our contemporary concerns. Attempts not to overstep the artificial boundaries should not be construed as avoidance of the issues with which the other chapter is dealing.

3. See Skinner (1974, 283-4) for an acknowledgement of his debt to the thought of R. G. Collingwood.

4. Condren (1985) takes this history of the study of political thought back to an 1855 volume by a Robert Blakey entitled *A Brief History of Political Literature from the Earliest Times* and a 1902 book by W. Dunning, *A History of Political Theories*. Nevertheless, for American political theorists it was Sabine's volume that defined the field and especially located the texts studied as epiphenomena of the age in which they were written.

5. Strauss is certainly aware of the problems inherent in such an injunction. In his essay on Heidegger, he comments: "The only question of importance, of course, is the question whether Heidegger's teaching is true or not. But the very question is silent about the question of competence -- of who is competent to judge" (1989a, 29).

6. In this construction I have not focused on the role of Sheldon Wolin's influential volume, *Politics and Vision* (1960). Wolin adopts, as he puts it, an historical approach, though one far more limited than we find in Sabine or the Strauss and Cropsey *History of Political Philosophy*, but he does so with the "conviction that an historical perspective is more effective than any other in exposing the nature of our present predicaments" (1960, v). Though Wolin's perception of those "predicaments" contradicts Strauss's version on almost every point, the study of texts for both is the means to address their determination to aid in the recovery of meaning for political life.

7. A constant stream of republications of Strauss's work and new collections of his lectures and essays, sympathetically introduced by Pangle (Strauss 1983 and 1989a) and Gildin (Strauss 1989c) attests to the continued impact of Strauss on a whole generation of political theorists in America, though clearly not in Europe. This is not the place to explore the divisions within that generation, east coast and west coast Straussians, etc., who while debating the reading even of Strauss himself do not deny the power of Strauss or the texts he studied. Meanwhile, Strauss has been the object of a particularly hostile reading by Drury (1988), but see Timothy Fuller's review of Drury's book for a telling and moderate response (1991).

8. In no way is the following meant to suggest a direct link between Strauss and the authors who see political theory as instrumental or Sabine and those who engage in the historical pursuit of political theory. Indeed, many of the scholars would be horrified to see themselves identified with one or the other. My point is simply that there is a common intellectual drive that motivates scholars who may appear to have significantly different orientations.

9. I have structured this essay in terms of the debate between the approaches of Strauss and the Cambridge School. To do so, as the anonymous reviewer pointed out, I have not engaged some of the more serious threats to the text and our capacity to read them whether instrumentally *or* historically that have come from Nietzsche, Heidegger, and Foucault. My failure to address these challenges is not to diminish their importance -- only to admit the limits of an essay of this sort, as well as its author's level of comfort with and competence in the works of these thinkers.

10. Compare here Shklar's colloquy with Montaigne in *Ordinary Vices* (1984) in which she makes clear the moral purpose of her work; despite a self-effacing description of the work as "a ramble through a moral minefield, not a march toward a destination" (1984, 6) and her comments at the end of the "ramble" that "I have contributed nothing to homiletic literature, and I have not harangued 'modern man,'" there lies behind these disclaimers the deep concern that citizens of a liberal society understand that they must "put cruelty first."

11. The survey of works presented here can in no way pretend to be comprehensive. It is not. The list of books and articles published in the area of the history of political theory and political thought is vast. Reflection on the last ten years can only allow for reference to a small number of works as exemplars of trends rather than offer a comprehensive statement of what has been done. Thus, the larger corpus of writing about Hume, Hegel, Oakeshott, Voegelin, and many others will not be addressed -- not because such work is any less important to the understanding of the trends in political theory, but because of limits of time, space, and my own expertise.

12. It should not surprise us, though, that many of the first and second generation of students of Strauss continue to write on the classical authors whose insights into things political Strauss repeatedly favored, e.g., Nichols's two books (1987, 1992) or Coby (1987), but in response to Strauss's broad interests we find other favored periods or authors as well. For just a beginning, see Weinberger (1985); Melzer (1990); Kelly (1987); Schwartz (1984); Gildin (1983) on Rousseau,

Tarcov (1984) and Grant (1987) on Locke; Danford (1990) on Hume; Smith (1989) on Hegel; Gillespie (1984) on Hegel and Heidegger.

13. As James Nichols points out, however, in his comment on Holmes's piece, Holmes may greatly exaggerate the danger by misreading Strauss to be a crude instrumentalist engaging in "'the imprudent importation of doctrines there [ancient Greece] espoused into the context of a highly differentiated society,'" (1979, 130) despite Strauss's firm assertion that "We cannot reasonably expect that a fresh understanding of classical political philosophy will supply us with recipes for today's use" (Strauss, *City and Man*, quoted by Nichols, 1979, 129). See also Strauss's review of John Wild where he comments: "[T]he teaching of the classics can have no immediate practical effect, because present day society is not a *polis*" (quoted in Tarcov 1991, 3).

14. Richter (1990, 49) apologizes for using the label "Cambridge School" since the phrase may not describe "a group as cohesive as that name implies." I make no such apologies.

15. More recent books by Neal Wood on Locke (1984) and Cicero (1988) reiterate the importance of embedding "a classic text...in the appropriate social, political and economic context" (1984, 2). According to Wood, since most classic texts in political theory are "histories from above" and thus written by a member or client of the ruling classes the work must be read in the context of history from below. From this perspective, Wood criticizes the exponents of the Cambridge School for recognizing only ideas and paradigms as history rather than seeing in history the major social and economic forces of the past. For Wood, those of the Cambridge School fit well into the philosophic rather than the historical camp and by implication are of less interest to those concerned with politics (1984, 10-12).

16. Of quite a different order, perhaps standing all on its own, is the monumental work of Richard Ashcraft (1986) on John Locke. Ashcraft sets Locke's *Two Treatises* firmly into the intellectual context of the seventeenth century, but is particularly interested in placing Locke in the political world of the dissidents of Restoration England. As such, the volume goes beyond recovering "the subjectively intended meaning of Locke's action in writing the *Two Treatises of Government*" (1986, 12) to the study of political movements. The Cambridge School's methodology can only contribute partially to this endeavor (1986, 12 note 12) which takes Ashcraft more deeply into historical intrigues than a study of seventeenth century discourse would.

17. The references that follow are to the reprint of the essay in Tully (1988).

18. Skinner comments in note 2 that his theories apply to any practice of the history of ideas. He just happens to use examples from political thought because that "reflects my own specialisation" (1988, 291 note 2). But it could just as easily have been the the study of esthetics or of ethics. For a consideration of the expansion of Skinner's views in precisely that direction see the series of essays in Rorty et al. (1984).

19. The most recent list of texts in the Cambridge series includes Aristotle, Bentham, Constant, Hooker, Leibniz, Locke, Machiavelli, J.S. Mill, Montesquieu, More, and Paine, hardly authors of "lesser known works." The Liberty Fund has reprinted important (but perhaps lesser known) classics such as Mandeville's *Fable of the Bees*, Algernon Sidney's *Discourses*, Lord Acton's Essays, Hume's Essays, and much more.

20. A Collection of Koselleck's essays, *Futures Past*, has been put out in translation by MIT Press (1985).

21. In a very different endeavor Harvey Mansfield's new book (1989) on the executive claims to resemble a "conceptual history," but a conceptual history that emphatically learns from texts, especially Machiavelli, not for clarification of the use of a term, but to raise serious questions about the nature of current practice.

22. Richter's original article in *Political Theory* in 1986 drew forth two rejoinders from Jeremy Rayner (1988, 1990) with an intervening response from Richter (1989) in which the two debated the compatibility proposed by Richter in terms of the implications of the philosophy of language and semantic analysis for the respective schools.

23. O'Brien (1981) is an example from this earlier period of a far more thoughtful work critical of "male-stream political thought" for its denigration of the importance of reproductive labor within any political community.

24. We can note that Sabine (1937, chap. 32), for example, writes of the *Autobiography*, of the essays on Bentham and Coleridge, of the *Logic*, of *Utilitarianism*, *On Liberty*, *On Representative Government*, but makes no mention of *On the Subjection of Women*. The chapter on Mill by Henry M. Magid (1987, 784-801) in the Strauss and Cropsey volume does not mention the essay at all, neither in the text nor in the appended list of recommended readings. Nor does it surface in Wolin's book. It took almost 60 years after Carrie Chapman Catt put out an edition of the work in 1911 (New York: Frederick A. Stokes Company) for the work to be republished by Alice Rossi and MIT Press. Now there are a multitude of collections in which this essay appears.

25. Though see the paper by Salkever and Nylan presented at the 1991 APSA meetings for a start in this direction.

26. My own work on the *Euthyphro* (1990) and the *Symposium* (1984b) gets its inspiration from Strauss's insistence that all Platonic dialogues are about politics, not just the *Republic*, *Statesman*, and *Laws*.

27. This is not even to begin to catalogue such articles that now regularly fill *Political Theory*, *Interpretation*, *Journal of Politics*, *Review of Politics*, *Polity*, and other such journals where we find numerous articles in political theory.

28. Although I have not discussed American political thought in this essay, in the context of the present point I should note Catherine Zuckert's book with the wonderful double entendre title *Natural Right and the American Imagination: Political Theory in Novel Form* (1990a) in which the classics of American literature are explored for insights into the political philosophy of the American regime.

29. See also the volume edited by Alan Udoff (1991). Though this volume is subtitled "A Critical Engagement," Udoff's concluding comment in his introductory remarks, "It is a blessing that Leo Strauss lived and wrote" (22), suggests that it isn't quite that.

30. The writing on Arendt has exploded recently. A few citations must suffice: Kateb (1983), Benhabib (1988), Jacobitti (1988), Honig's (1988) criticism of Jacobitti, Bradshaw (1989), Dossa (1989), Ring (1989); even this does not capture the flurry of panels at meetings and works in progress on Arendt's thought.

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2

Political Theory in the 1980s: Perplexity Amidst Diversity

William Galston

The topic of this survey, even narrowly defined, encompasses an unmanageably large literature, and I must begin by announcing what I have been allowed, or compelled, to exclude. Unlike its predecessor, this volume contains a separate chapter dealing with, inter alia, specific interpretations of historical figures in the "tradition" and general theories as to the nature of the interpretive activity; I will therefore touch on such matters only briefly and in passing. Nor (for reasons that I hope sections I and II will make clear) will I spend much time on the kinds of theorizing explicitly directed toward empirical research programs (which is not to say that political theory and political science can afford to proceed in isolation from one another -- or so I shall argue).

Nor, finally, can I do justice to the dramatic flowering of scholarship focused on American political thought during the past generation. I can hazard a few generalizations: As compared to the once-dominant Hartzian view, many current interpretations are more inclined to consider civic republican and theological supplements to Lockean liberalism, to advance revisionist views of the content of America's Locke, to revive and extol what might be called the transcendentalist/pragmatist tradition, and to bring to the fore the voices of all-but-forgotten dissenting movements. And as Judith Shklar suggested in her APSA presidential address, we are on the whole increasingly likely to see connections between American political thought and an American political science embodying in its empirico-statistical methods the democratic principle that the lives of ordinary people are intrinsically significant (Shklar 1991, 4). These manifestly inadequate remarks will have to stand in place of the fuller treatment this topic deserves (see also Kloppenberg 1987; Hirshman 1990; Pangle 1988).

Even within the circumscribed area I do try to cover, no brief survey can possibly do justice to the full variety of interesting and important work. As an alternative to silent or inadvertent omission, I have constructed a topically organized bibliography that, while necessarily less than comprehensive, is considerably fuller than a simple list of textual citations. Readers wishing to go farther than I could in a particular direction may find it useful to refer to the appropriate section of the bibliography with which this chapter ends.

I. Basic Narrative Strategies

The story of political theory during the past decade can be told from many different points of view, and the selection of any one is bound to prove in some measure arbitrary. Setting to one side, for the moment, substantive disagreements that inevitably affect the choice of plot line, there are at least three basic narrative strategies, not mutually exclusive, that might be employed. The trajectory of political theory can be seen as shaped by the shifting contours and imperatives of its disciplinary locations, by the political problems and conflicts on which it reflects (or, alternatively, of which it serves as ideological representation), and by the inner logic of its theoretical development and disputation. While observers of political theory disagree as to which should be given pride of place, most would agree that each has played a significant role. At the risk of getting ahead of my story, but in the hope of giving the reader some preliminary orientation, let me offer a brief characterization of political theory during the past decade under the three headings just enumerated.

Disciplinary Location

With regard to disciplinary location, the 1980s began with political theorists divided between departments of political science and philosophy, in fitful communication across the border but largely cut off from other members of their respective disciplines. These conditions persisted through the 1980s, with the added

complication that political theory came to be practiced in, and opened itself to, other disciplines as well. For example, the quantity of political theory carried out within the legal academy escalated sharply (see for example Hirshman 1990; Perry 1988; Symposium 1988), and the impact of literary, psychological, and social theory (much of it of European provenance) on American political theory was substantial.

As disciplinary divisions grew more complex, geographical divisions eroded significantly. British and American theorists crisscrossed the Atlantic at record rates; while Jurgen Habermas tirelessly incorporated Anglo-American theory into a renewed European critical theory, Richard Rorty worked to bring Europeans such as Nietzsche and Heidegger into the American pragmatist fold.

This is not to say that the substantive implications of geography disappeared altogether. On the contrary: because political theory is increasingly a multinational discipline, it is more important than ever to reflect with some self-awareness on differences among political cultures that may have an impact on objects and styles of theorizing.

For example, it is familiar (but not incorrect) to suggest that in comparison to the U.K., the United States is more socially diverse and more affected by rights-based individualism. These contrasts do not determine theoretical outcomes, but they do help set theoretical agendas. For example, prompted by the many social and cultural differences in American society, American political theory has begun to ask whether universalism and impartiality may not often mask ignorance about, and even bias against, these differences. America, the land of individualism, has also become the battleground of a contest between defenses of individualism based on Kant's conception of autonomous moral agents and critiques of individualism inspired by Hegel's conception of individuals as socially embedded and constituted. Cultural diversity is linked as well to the fact that, compared to British liberals, Americans have been far more strongly drawn to theories that see the state as neutral with respect to competing conceptions of the good life and, therefore, as abstaining from promoting any particular idea of human perfection (see Barry 1990, li). Here, too, debate widened during the 1980s as dissenting theorists questioned the possibility and desirability of neutrality and anti-perfectionism, so understood.

Another key difference between the United Kingdom and the United States lies in the presence of a major left-wing party in the former and its absence in the latter. In this light, it is not accidental that many American theorists have opted for an aestheticized withdrawal from the standard political arena in favor of literary and cultural concerns.

One may also speculate that the greater residual appeal of utilitarianism in the U.K. vis-a-vis the U.S. is related to the contrast between (on the one hand) a unified civil service and plenipotentiary parliament, which can at least be imagined as performing aggregative calculations, and (on the other) the dispersion of political power and the omnipresence of rights-based thinking characteristic of America. (While surely suggestive, these reflections on plausible relations between theory and cultural context should not be taken to suggest any straightforward reduction or subordination of the former to the latter.)

Political Problems and Conflicts

With regard to the complex interplay of political theory and politics, a number of developments stand out. Chief among them was the diminished appeal of Marxism as political practice. To be sure, key Marxist categories such as alienation retained a measure of critical force, and theorists inspired by Marx continued to work at a high level of rigor (see especially Cohen 1988; Elster 1985; Reiman 1987; Roemer 1982, 1988). Still, for many the ties to Marx became increasingly attentuated.

The decline of Marxism coexisted, however, with a continuing desire in many quarters for a critical standpoint vis-a-vis various aspects of contemporary society. In both the United States and the U.K., the 1980s were dominated by a resurgent conservative politics that some theorists (primarily libertarians and social conservatives) applauded but that left many traditional liberals and social democrats stunned and disoriented. One possible response was a renovated liberalism more aware of, and responsive to, the weaknesses that had opened the door to conservative triumphs (see Rosenblum 1989, "Introduction"). Another possible response was a defense of classic socialist ideals more attuned to the demonstrable practical and moral advantages of market economies and democratic politics (e.g., Miller 1989). A third line of response was a critique of conservative politics framed in non-liberal, or antiliberal, terms: thus "virtue" was counterposed to "greed," and "community" was invoked against selfishness, against social isolation, against individualism run amok. This strategy could of course draw upon the enduring tropes and sentiments of antiliberalism (Holmes 1989). A fourth response drew upon the "postmodern" criticisms of the Enlightenment philosophical-political project launched by European thinkers such as Martin Heidegger, Jacques Derrida, Michel Foucault, and Jean-Francois Lyotard; political theorists mining this vein could be seen variously as proposing a critique, or intensification, of liberalism (see especially Connolly 1990).

The felt need for a critical vocabulary was augmented by the multiplication and growth of insurgent groups within liberal societies. During the 1980s, new movements based on ethnicity, gender, and sexual orientation came to the fore. These trends were particularly pronounced within the United States, with its relatively open immigration policies, surge of women into the non-domestic workforce (including academia), and legal system that encourages litigation as a vehicle of social change. Some theorists argued that liberalism, properly understood, offered the most promising framework for articulating and accommodating these new demands. For example, Susan Moller Okin argued that we can have a "humanist liberalism" that fully includes women, but only if we can devise "a theoretical basis for public policies that, recognizing the family as a fundamental political institution, extends standards of justice to life within it" (Rosenblum 1989, 53). Others argued that the liberal discourse of equality and diversity was a cloak for various forms of hierarchy and hegemony; they opted for theoretical expression through concepts such as multiculturalism, decentering, and difference (for the debate within feminism, see Rhode 1990).

More broadly, developments during the past decade have had the effect of calling into question the traditional focus of much political theorizing on the public institutions of the modern nation-state. This interrogation has taken place along numerous fronts -- within nations, among nations, and across national boundaries. In response to changing gender relations, feminist thought has moved to reject, or fundamentally reconfigure, the distinction between public and private life. The upsurge of ethnic identification has led some theorists to explore the moral claims of distinct subcommunities within nation-states (see especially Kymlicka 1989, chaps. 7-10).

Among nations, questions such as the international enforceability of basic rights and resource transfers in response to emergencies and the exigencies of democratization and development are clearly on the agenda. The renewed activism of transnational institutions such as the United Nations, the World Bank, and the European Community furnishes meaningful sites for the application of general principles (for a comprehensive survey, see O'Neill 1991).

Across national borders, we see the rise of multinational corporations and of technologies that transfer information instantaneously and globally. These and similar entities create new sites of power whose relation to the reach of national states, individually or jointly, is yet to be determined. The increased salience of transnational environmental issues such as ozone depletion, global warming, and the Chernobyl disaster is generating new issues of distribution and of agency. More recently, the economic collapse and political breakup, first of the Soviet European empire and then of the Soviet Union itself, has exacerbated fears concerning massive refugee flows -- fears already widespread in nations such as France, Germany, and the United States that have experienced increased levels of immigration during the past decade (Schuck and Smith 1985). The Soviet collapse has also heightened the risk of proliferation of nuclear arms technology and expertise, raising new issues of national sovereignty and global responsibility.

Inner Logic of Theoretical Development

With regard, finally, to the inner logic of theoretical development and dispute, a number of developments stand out. To begin with, key figures from the past were reappropriated for contemporary purposes. To the Kantian revival already underway in the 1970s were added refurbished versions of, inter alia, Aristotle (representing deliberation and civic virtue), Hobbes (contractual origination), Locke (individual rights and property), Rousseau (direct democracy), Hegel (civil society and community), Wollstonecraft and Mill (the subjugation of women), and Nietzsche (subversion of the liberal self and social order). It was not difficult to deploy one's favorite against the competitors: communitarians reenacted Hegel's critique of Kantian formalism; participatory democrats repeated Rousseau's strictures against Lockean privacy; Foucauldians replayed Nietzsche's relentless critique of bourgeois society; and so forth.

To some extent, theoretical discussions occurred within rather than among the arenas demarcated by these selective reappropriations. Thus (to take but two examples), Will Kymlicka could write a first-rate Kant-inspired introduction to *Contemporary Political Philosophy* that made no mention whatever of thinkers such as Foucault, Derrida, Gadamer, and Heidegger (Kymlicka 1990) while William Connolly offered an equally meritorious Nietzschean meditation on *Political Theory and Modernity* with but one passing reference each to Rawls and Kant (Connolly 1988). This points to a broadly significant contrast: among analytical political theorists working within philosophy departments Nietzsche rarely rates more than a passing glance, while for many political theorists working within political science departments, he has come to occupy a strategic position. Indeed, as Jurgen Habermas has argued, the aspect of contemporary European thought that has had the greatest impact in America during the 1980s represents, in one way or another, a reworking of Nietzschean themes (Habermas 1987, chap. IV).

I do not mean to suggest, however, that the parallel revivals of Kant and Nietzsche have altogether failed to intersect. On the contrary: the broad tendencies they represent became locked in one of the major theoretical conflicts of the past decade, between Enlightenment modernity and its "postmodern" critics.

This conflict is worked out on at least three levels. First, contemporary Nietzcheans are deeply mistrustful of the idea of "truth," seeing truth-claims as occurring within systems of thought and practice that represent either the effects of power or the distortions and exclusions of metaphysically laden discourse. While contemporary philosophers working within the Anglo-American tradition do not typically embrace objectivist or realist accounts of truth, they do tacitly assume that rational discourse serves functions other than self-assertion or the maintenance of given power relations.

Second, non-Nietzscheans affirm, while Nietzscheans deny, the utility of some concept of the "self" as a more or less transparent, unified, and self-governing entity. Throughout the 1980s, for example, John Rawls has rested his reconstructed theory of justice on a specific understanding of "moral personality" -- individuals as agents endowed with the moral powers of acting in accordance with principles of justice and of shaping and revising their own conceptions of the good. Nietzscheans, by contrast, have declared the "death of man" understood as the autonomous sovereign ego, emphasizing instead individuals as sites of language, power relations, and the unconscious.

Third, Nietzscheans are far more likely than are non-Nietzscheans to understand "modernity" as a unified and questionable historical phenomenon -- that is, to focus on the most dubious features of bureaucratic rationalization and bourgeois life and to interpret them as the progressive self-undermining of Western rationalism. While most Nietzscheans, at least in America, see their efforts as bolstering a less hierarchical and more democratic egalitarian order (Connolly 1988, 168-75), their critique of modernity nonetheless recapitulates many nineteenth-century conservative and aristocratic animadversions against the emerging liberal order (Holmes 1989, 245-47). Non-Nietzscheans, by contrast, tend to criticize contemporary society not as relying on untenable views of reason, justice, and society, but rather as failing to live up to those ideals. The rational/moral superiority of modernity is assumed; the problem lies not with our norms, but in our deeds.

The rise of postmodernism, and in particular the increasing influence of Michel Foucault on political thought, contributed to a reorientation of critical theory during the 1980s. While Habermas, for example, has continued to develop his analysis of communicative rationality as the basis of social legitimacy and to criticize the traditionalist hermeneutic enterprise of Gadamer and other German neo-Aristotelians (McCarthy 1978; White 1988), he has also embarked on a comprehensive critical examination of postmodernism (Habermas 1986, 1987). While postmodernism is typically identified with the non-Marxist left criticism of liberal democracy, Habermas has redescribed it as a form of covert conservatism with roots in the aesthetic-aristocratic antiliberalism of the nineteenth century (Habermas 1980).

The clash between modernists and postmodernists was but one of the issues that configured political theory in the 1980s. Another, triggered in the early 1980s by the publication of Michael Sandel's *Liberalism and the Limits of Justice*, Alasdair MacIntyre's *After Virtue*, Michael Walzer's *Spheres of Justice*, and a series of essays by Charles Taylor, was the "communitarian" critique of liberalism as (among other flaws) abstract, unhistorical, hyperindividualist, resting on an unsustainable metaphysic of the self, indifferent to virtue, and blind to the kinds of human goods that are enjoyed only in the company of our fellow human beings. This communitarian critique, inspired in considerable measure by Hegel, overlapped significantly with the "civic republican" critique inspired by J. G. A. Pocock's *Machiavellian Moment* (Pocock 1975) and by Hannah Arendt. Key civic republican theses such as the primacy of direct political activity oriented toward the common good were deployed against (what was taken to be) the liberal defense of privacy and self-interest.

As we shall see (Section III), the liberal response throughout the 1980s was complex. By the end of the decade, many liberals were turning back toward "civil society" -- the complex weave of groups, associations, and subcommunities -- as a mediation between individuals and state institutions. This move, inspired variously by Tocqueville, Hegel, Catholic social thought, and successful movements such as Poland's Solidarity and Czechoslovakia's Velvet Revolution, enabled liberals to recognize the emotional and moral possibilities of collective action while preserving their commitment to individual liberty. Indeed, some liberals had begun to argue that liberal autonomy was not only compatible with, but required, state action that protects subcommunities, sometimes through special legal status (Raz 1986; Kymlicka 1989, 1992; Kukathas 1992).

Parallel to the rediscovery of civil society came a third important theoretical development, the rediscovery of the "state." Theorists conducted a multi-front war against pluralism, structural-functionalism, and classical Marxism, all interpreted as modes of reducing the state to (at best) a dependent variable of social processes. In the face of this social reductionism, state theorists sought to restore an understanding of public institutions as at least quasi-autonomous and to show how this perspective could illuminate a range of empirical issues (see especially

Krasner 1984; Nordlinger 1981; Skocpol 1982; Skowronek 1984). In response, leading figures from the previous generation of theorists contended that the state had never really been expelled from political theory and research (Almond 1988; Lowi 1988) or, more pugnaciously, that its reemergence would mean reemersion in the conceptual morass from which political analysis had extricated itself only recently and with such great difficulty (Easton 1981).

A fourth key development of the past decade was the renewed interest in pragmatism, sparked by thinkers such as Richard Rorty and Richard Bernstein. Pragmatism was interpreted, first, as an alternative to philosophical "foundationalism." Rather than looking outside to metaphysics, natural science, epistemology, or theology for its fundamental propositions, social theory was free to proceed by reflecting on its own characteristic problems; the point was context, coherence, practical testing, or even imaginative projection, not linear movement from premises to conclusions (Rorty 1982, 1989; Bernstein 1985b). Pragmatism served, second, as a way of reinvigorating the idea of democratic participation through deliberation; John Dewey's understanding of "social intelligence" came increasingly to the fore (Bernstein 1985a, 1992; Anderson 1990, 1991; Westbrook 1991).

Finally, no story of political theory in the 1980s would be complete without stressing the extraordinary development and vitality of feminist thought. Its explorations ramified into all aspects of politics, society, personality, and inquiry: the constitution and construction of gender differences; the retrieval of neglected writers, agents, and questions, and the corresponding expansion or reconstitution of what political theory theorizes about; the exploration of covert gender assumptions in theoretical categories such as the public/private distinction, rights, and justice; the examination of bias and discrimination in practical spheres structured by such categories; and the questioning of entire modes of philosophy and social inquiry (those based on, e.g., abstraction and "objectivity") as gender-based and partial. Overlying this ramification of subject matter is the reenactment within feminist theory of debates across the range of social thought: feminism is now practiced in liberal-humanist, democratic, Marxist, post-structural, psychoanalytic, social-constructionist, and biological-essentialist modes, among others. Whatever the next key steps may be, it seems safe to predict that feminist thought will play a central role in the political theory of the 1990s. (For clear summaries of recent trends see Coole 1990; Randall 1991; Phillips 1991, chap. 2; Kymlicka 1990, chap. 7. More generally, see Additional Bibliography, section 2.)

II. The Chronological Beginning: The 1980s

To the diversity of narrative lines corresponds the multiplicity of possible points of departure for the telling of the tale. I have chosen to begin at what, given my assignment, is the chronological beginning -- to wit, the early 1980s -- and with a document that is at least institutionally appropriate, namely, John Gunnell's historically based contribution to the predecessor volume in this decennial series (for a comprehensive account of recent disciplinary histories of political science, see Farr 1988).

At the outset of a detailed account stretching over nearly a century, Gunnell distinguishes 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)" (Gunnell 1983, 3). For much of the century, the theoretical discourse of politics was intimately connected with political science, either as the framework of its research program or (especially during the 1950s and 1960s) as its ardent critic. During the 1970s, however, both these forms of connection became attenuated, PT withered, and pt flowered as a more or less autonomous endeavor with its own journals and characteristic concerns.

This shift, the separation (if not outright divorce) of political theory from political science, is the first of Gunnell's principal theses. The others concern the nature of the theoretical activity itself. In Gunnell's view, the political theory of the 1970s was characterized by a variegated *dispersion* of issues and concerns and by an *alienation* from real political life. He writes, with something approaching nostalgia, about the vanished epic struggle between behavioralists and their traditionalist critics. He describes, with something approaching contempt, what he regards as pt's metatheoretical self-absorption and deliberate self-distancing from actual political phenomena. And he predicts that these three features of pt -- divorce, dispersion, alienation -- will continue through the 1980s and beyond (Gunnell 1983; see also Gunnell 1986, chap. 1).

Of these three predictions, that of divorce has been borne out in only a qualified sense. A number of leading democratic theorists have appropriated, or critically encountered, empirical findings (see, inter alia, Beitz 1989; Dahl 1989; Fishkin 1991; Gutmann 1987; Mansbridge 1990); students of civic education have of necessity examined concrete social processes (Murchland 1991); rational choice theorists have increasingly used their deductive conclusions as hypotheses guiding empirical research (for a useful summary, see McLean 1991; Grafstein 1992); and liberal theorists are getting

down to cases (Kymlicka 1989). By and large, however, theorizing directed toward the practice of political inquiry has been limited compared to the 1950s and 1960s, and the reintegration many hoped might occur after the termination of the behavioralist/traditionalist conflict has failed to materialize.

Following David Easton's 1969 call for a "post-behavioral" political science more oriented toward practical issues, "policy studies" was heralded as the arena for the new synthesis (or eclecticism, or at least truce). But in a recent survey of the first two decades of the *Policy Studies Journal*, Ethan Fishman concludes that the promise of greater integration has gone largely unfulfilled. Few students of public policy have sought to avail themselves of the conceptual and normative insights offered by the philosophical tradition; and while some political theorists have tried to engage pressing issues of contemporary American politics (notably abortion and affirmative action), few have translated their arguments into publicly accessible terms. As reasons for this disappointing result, Fishman cites "mutual antagonism": on the one hand, the lingering positivism and technical incrementalism of most empirical policy researchers, and on the other, the enduring self-absorption and hyper-textualism of many political philosophers (Fishman 1991).

With regard to Gunnell's second prediction -- continued dispersion within political theory -- I have already mentioned the proliferation of theoretical genres during the past decade. There has been a fair amount of sniping across genre borders, but notably less cross-fertilization. For example, many accounts of "democratic deliberation" tacitly presuppose either that under conditions of full deliberative equality, something approaching unanimity is to be expected or, failing a full consensus, that the results of a majoritarian decision procedure are unproblematic. But there is little empirical support for the former assumption, and the latter has been called into question (to say the least) by the line of inquiry Kenneth Arrow and others initiated some decades ago. One may seek to resolve these difficulties by reducing the centrality of majoritarianism for democratic theory (see Beitz 1989, chap. 3); one is not free simply to ignore them (for more extended remarks along these lines, see Iain McLean's essay in Held 1991, 172-96).

Let me now inspect the issue of dispersion through a somewhat wider-angled lens. Writing in the context of British political theory, David Miller focuses on three principal shifts of emphasis during the past two decades. Critical studies of classic texts were largely replaced by "conceptual history" as practiced by Quentin Skinner, J. G. A. Pocock, John Dunn, Terence Ball, James Farr, and others (see Pocock 1985; Tully 1988; Ball 1988; Ball, Farr, and Hanson 1989). Analyses of specific concepts (freedom, obligation, and so forth) were largely replaced by more systematic normative political theories, many in the liberal and/or contractarian mode. Finally, theories exploring and justifying political institutions such as parliamentary government were replaced by theoretical analyses of public policy -- especially welfare state policies, at least in the British case (Miller 1990).

While not sharply dissenting from this description, David Held offers an historical account emphasizing addition rather than substitution. For example, he suggests that analysis of specific concepts remains viable as a mode of theorizing, coexisting with (and sometimes shading into) the elaboration of more comprehensive normative structures. (Consider, inter alia, Richard Flathman's careful inquiries into concepts such as freedom, authority, and citizenship; Flathman 1987.) Similarly, he suggests that normative-theoretical reflection on institutions is supplemented, but hardly replaced, by parallel analyses of public policy issues. And he emphasizes the importance of developments in the form of "model-building" political theory drawing on the resources of theoretical economics, game theory, and rational choice theory (Held 1991, 16-18; see also Elster 1991 and McLean 1991a).

The inevitable question is how (or whether) these lines of theorizing fit together. A generation ago, Brian Barry suggested that if the (then just barely beginning) upsurge of systematic normative theory constituted a return to traditional political philosophy's concern with the nature and conditions of good forms of government, the development of the theorizing Held dubs model-building should be viewed as "technical aids to clear thinking about this subject matter" (Barry 1989, 17).

There was, and is, much good sense in this proposal. As I have already suggested, however, theorists in both modes have been notably slow to embrace it. One reason, perhaps, is that model-building typically presents itself as more than technical. As Jon Elster has recently put it, "Rational choice theory is first and foremost normative. It tells us what to do in order to achieve our aims as closely as possible. It does not, in the standard version, tell us what our aims ought to be" (Held 1991, 116-17). Rational choice theory, then, addresses its normative assessment to the nexus between individual aims and collective choice procedures (or institutions); it is uncritical regarding individual aims themselves. But this last point is ambiguous: it can be read as saying that no form of theory can rationally assess individual aims or, alternatively, that only forms of theory other than model-building can do so. The former represents not a Barry-style technical aid for, but rather a direct challenge to, normative theories that seek to appraise and where necessary modify individual goals and attachments. The latter tacitly proposes a division of

labor between theories of preference-formation and theories of preference-aggregation, an arrangement that would at least make possible some synthesis along Barry's lines (for further remarks along this line, see Miller 1990, 432-33).

Gunnell's third prediction, concerning alienation, raises even more complex questions. To begin with, the standard for engagement with "real politics" is hardly self-evident. This much is clear: political theory is located somewhere between philosophy and politics, partaking of both, identical with neither. No doubt much contemporary theory is too abstract and self-contained to shed much light on politics. Still, to have any chance of providing its distinctive form of illumination, theory must stand at some distance from the practice of politics; accusations of excessive distance are always contestable and not infrequently unfair.

Beyond this relatively easy step, matters quickly become more complex; the distance between theory and practice cannot be measured as the crow flies. Michael Walzer has argued for a kind of "connected" criticism that confronts the practices of a specific community with its own immanent norms. On inspection, he suggests, it turns out that these norms are quite concrete, embedded in specific functional "spheres" of overall community life (Walzer 1983, 1987). John Rawls has proposed a normative structure whose basic building blocks -- inter alia, conceptions of a well-ordered society and of moral personality -- are drawn from the public culture of a liberal democratic community. This too is a form of connection, albeit at a higher level of generality than Walzer thinks appropriate (Rawls 1980). Even more general, yet still in some measure connected, is Habermas's effort to draw norms transcending (but valid for) specific social contexts out of the commitments immanent in undistorted communicative practice (Habermas 1987, lecture XI).

To be sure, the link between these various normative standpoints and concrete political problems is not always straightforward. But it seems unreasonable to expect theory by itself to do all the work. At its best, theory can establish a general framework that helps us pick out, and put in order, the features of actual institutions and policies needed for their assessment. Even when concrete phenomena do not directly inform the discovery (or construction) of principles of judgment, they will form indispensable elements of their meaning and application.

However abstractly political theory presents itself, it is seldom as far removed from politics as it may appear. On the contrary, it may well be characterized as the continuation of politics by other means. To repeat a theme already broached: many contemporary theories represent varieties of discomfort with life in liberal democratic/bureaucratic welfare states. That these theories become elaborate and even abstruse does not mean that they wholly lose touch with, or fail to explicate, the experiences out of which they arise. That they employ unfamiliar vocabularies and grammars of criticism reflects the special situation of contemporary antiliberalism -- namely, the eroding credibility of Marxism as alternative practice.

This much, though, must be conceded to Gunnell: many theoretical disputes reach the point of diminishing or even negative returns long before they grind to a halt. Theoretical discourse may begin as rooted, but it can (as Gunnell suggests) end as aimlessly self-referential, as a gear spinning without connection to any other machinery. This is the phenomenon inspiring Brian Barry's "nightmarish feeling that 'the literature' has taken off on an independent life and now carries on like the broomsticks bewitched by the sorcerer's apprentice" (Barry 1989, 18). It can be ameliorated only by book publishers and journal editors more prepared than are many today to blow the whistle on exhausted topics and to take chances on less entrenched or faddish approaches.

At roughly the same time that Gunnell was composing his trenchant account, *Political Theory* published a special issue entitled "Political Theory in the 1980s: Prospects and Topics." Two of the seven essays dealt with political theory as a discipline. Douglas Rae offered a Gunnellish critique: rather than boldly theorizing about politics, imperatives of security and career led academics instead to "theorize about theories about politics." The result was a lush profusion of "conceptual essays, genetic theories of theories, biographies of theorists, reductions of theories, projective transformations of theories, cryptographies of theories, consequentialist surveys of theories, structural analyses of theories, deconstructions of theories" (Rae 1981). Writing in a similar vein (if somewhat more affirmative tone), Brian Barry noted the difference between political theory as practiced in departments of political science and of philosophy: driven in each case by distinctive intradisciplinary career pressures, the former tends toward bloated and repetitive exegeses of other theories, while the latter tends toward subtle distinctions and ingenious arguments that are largely evidence-free and add little to our substantive understanding (Barry 1981).

There was much truth in these disciplinary descriptions, and not much change in the decade since (although, as I suggested earlier, these stylistic differences have been overlain with substantive ones). Still, one wonders whether these phenomena are specific to political theorists, or rather are endemic in the life of modern academics generally. The institutionalization of intellectual life in the university has fostered disciplinary parochialism and careerism and has in some ways

reinforced a perennial intellectual tendency toward disengagement from serious practice. But the historical alternatives for intellectuals -- the whims of aristocratic or haute-bourgeois patrons, the petty tyrannies of editors and publishers, the unsubtle coercion of parties and movements -- have not been so inspiring either. There is a tension between freedom of inquiry and the imperatives of any institutional structure -- that is, between the life of the mind and the felt necessities of physical and social existence -- that cannot be fully overcome. The most that can reasonably be expected is constant awareness of, and struggle against, those aspects of practical context that deform thought.

Whatever their exaggerations, the arguments of Rae, Barry, and Gunnell can be understood as bracing antidotes to the mood of self-congratulation that had settled over political theory in the early 1980s in the wake of its phoenix-like rebirth. The standard account goes something like this: The "death" of constructive political theory and its replacement with the history of political thought was in fact murder by positivism. But with the dwindling plausibility of positivism through both external challenge and internal critique, the possibility reemerged of a political theory sobered by the demolition of earlier naivete but confident of its ability to engage in non-emotivist, adequately defensible normative discourse. The space thus opened was occupied with the publication of John Rawls's *Theory of Justice*, which triggered a period (not yet ended) of intense activity and palpable progress (see especially Plant 1991).

This story was too simple to be entirely true. As Barry has recently argued, few of the mid-century philosophers fingered as the hitmen were actually positivists, and it is just as plausible to ascribe the decline of political theory to a mushy but pervasive utilitarianism (Barry 1990, xxxii-xxxviii). This then yields the Revised Standard Version: Rawls is still at the heart of the story, but now as an anti-utilitarian who presupposes and builds upon the discrediting of logical positivism.

Still, doubts could be, and were, raised, concerning the viability of this entire theoretical style. John Dunn, for example, wondered whether the new breed of normative anti-utilitarians (among whom he included Robert Nozick, Ronald Dworkin, and Bruce Ackerman as well as Rawls) were "utopian" in the negative sense of overlooking inescapable questions of political possibility. Political theory, he declared, "is not principally an exercise in the appraisal and grading of the comparative ethical merits of different societies. Rather, it is a segment of human practical reason." Among other implications, this means that political theory cannot simply impose itself upon, but must directly take into account, the constraints of practice; for political theorists, sociological innocence is utterly debilitating (Dunn 1985, 154-55, 159-160).

Granting, arguendo, the good sense of Dunn's general point, one may wonder whether it is wholly fair to all the theorists he reproaches. Rawls for one has contended throughout the 1980s that political theory must be political through and through: it must be rooted in the public culture of the societies it addresses, and it must build upon basic features of those societies -- in the case of liberal democracies, on the fact of pluralism. Doubts can be raised as to the adequacy of the "commonsense political sociology" on which Rawls relies, but its central place in recent iterations of his theory can hardly be denied.

Yet this fact does not by itself suffice to meet the deeper thrust of Dunn's criticism, which calls into question not just the relationship between theoretical norms and empirical facts, but the currently popular characterization of political theory as the development and justification of normative propositions. Rather, Dunn insists, political theory is principally an attempt to understand what is really going on in society, and its agenda is therefore set (or ought to be set) by developments in a "historically given practical world" (Dunn 1985, 1).

Dunn's counterproposal has the merit of highlighting the fact that the demise of positivism spawned more than renewed normative activities. It also legitimized a conception of interpretation as a distinctive feature of social inquiry -- specifically, an interpretive practice carried on within traditions and history and with a keen self-reflective awareness of the interpreter's specific, though open and malleable, finitude (Held 1991, 13-16). Granting the legitimacy of interpretation, however, the proposed substitution of understanding for appraisal cannot be fully executed. As Dunn himself points out, "the question of whether an existing assemblage of human practices is essentially appropriate as it stands or whether it requires drastic and systematic reconstitution is at the core of social and political theory." For this reason, "a simple appeal to the authority of practice has no determinate content and is necessarily either evasive, insidious, or vacuous." The critique of philosophical detachment launched by Oakeshott and Wittgenstein among others serves as a necessary corrective to naive or vicious abstraction, but "it hardly makes a very adequate philosophical approach to the rational critique and prudent revision of human practices" (Dunn 1985, 174-75). What we seem to be left with, then, is the more measured if less challenging view that while concrete political practice provides the proper objects of political philosophy, such practice cannot directly resolve the choice it regularly sets

between change and the status quo. Political philosophy is not free to disregard the phenomena of (e.g.) political economy and ethnic division, but the questions they raise of appraisal and response are at least distinguishable from those of understanding *simpliciter* (for related features of this issue, see Galston 1980, 25-27 [on Oakeshott]; Bernstein 1985b, 154-55 [on Gadamer]; Beiner 1989 [on the Gadamer/Habermas debate]).

It is harder to set aside another of Dunn's objections. The new wave of anti-utilitarian theories, he insists, lacks the means essential for its self-justification: while deeply Kantian in inspiration, the theories offer "a Kantian ethics without any convincingly characterized defense of or surrogate for a Kantian ontology." To be sure, the quasi-Kantians are not alone in this; the past decade has also yielded neo-Aristotelianism without teleological metaphysics and neo-Hegelianism without History or Totality. Still, the thinkers among whom modern theorists scavenge for inspiration were at least addressing serious problems, which we are avoiding: "to raid their philosophical creations, like magpies, for odd bits of more or less sparkling material to adorn our own less ambitious intellectual domiciles is to display a particularly unreflective intellectual indolence" (Dunn 1985, 179-80).

This criticism reflects an important feature of contemporary theory, much discussed during the 1980s. Since the publication of Richard Rorty's *Philosophy and the Mirror of Nature* in 1979, the rejection of "foundationalism" has gathered strength. Since Descartes (the argument runs), both natural science and social theory were bewitched by the false metaphor of solid (indubitable) foundations as giving strength to otherwise rickety (that is, contestable) propositions. But now that we "know" that no such foundations are available, that the very idea of such foundations is incoherent, we can get on with the business of theorizing as a self-consciously social enterprise rooted in the beliefs and practices of the enterprising community (Rorty 1979).

Many critics of contemporary anti-foundationalism believe that these Rortean arguments proceed too briskly (not to say blithely). We are not faced, they insist, with the stark choice between Cartesian first principles and Wittgensteinian "forms of life" as sources of normative judgment; there are coherent alternatives to both objectivism and relativism (Bernstein 1985b; Herzog 1985). The internal critique of socially established practices can yield surprising results. Moreover, the rejection of X as a basis for political theory does not entail the rejection of all relation between X and theory: certain political-theoretical theses may turn out to be inconsistent with (say) particular metaphysical views of the self, and vice-versa (Galston 1991b, chap. 2). If so, the heart of Dunn's objection is sustained. While we may no longer be able to do political theory in the grand metaphysical style of previous centuries, we are not free to assume that it is an autonomous and self-sufficient endeavor.

III. Contemporary Liberal Democracy

As I noted earlier, the proliferation of political theory during the 1980s renders the selection of any single narrative line to some extent arbitrary. I want to suggest, however, that one plausible way of telling the story is to focus on contemporary liberal democracy -- in particular, on the diverse ways in which it has been attacked and defended.

In the hope of bringing some rough order into the thrust-and-parry, let me offer a schematic description of the political system at issue. Contemporary liberal democracy includes, at a minimum, the following features: constitutional government, based on popular consent and operating through representative institutions; a market economy subject to some degree of political restriction and regulation; a system of welfare provision and social insurance; a diverse society with a wide range of individual opportunities and choices, and of independent groups and associations ("civil society"); and a substantial, strongly protected sphere of privacy and individual rights. What follows is a brief review, seriatim.

Constitutional Government

During the 1970s it was widely argued that liberal democracies were excessively responsive to their publics -- whence "overload," inflation, and crises of governance. Far from being historical accidents, many thought, these trends represented an unfolding inner logic -- a democratic corrosion of previous hierarchical and cultural restraints on public demands. During the 1980s, by contrast, the emphasis shifted to the inadequate responsiveness of representative institutions. This indictment included numerous counts: the insulation of political elites from ordinary citizens; excessive reliance on courts at the expense of a more majoritarian, citizen-based politics; the disproportionate impact of organized "special interests" on the legislative process; the diversionary and demagogic manipulation of public dialogue, tactics magnified by modern communications technology; and, most fundamentally, the exclusion of the citizen-body from direct involvement in the institutions of self-government.

Underlying these specific indictments could be discerned three democratic standards allegedly violated in

everyday practice: equality, deliberation, and participation. It is by no means clear, however, that these standards are in practice harmonious -- that reforms intended to enhance any one of them will necessarily promote the others (see Fishkin 1991). Nor is it clear what each, considered separately, in practice requires. Take democratic equality: Does it apply to individuals or groups? Does it require only fair opportunity, or in addition some attention to results? These and related issues have arisen in the context of interpreting and enforcing the U.S. Voting Rights Act (for the most thorough discussion, see Beitz 1989, chaps. 6 and 7). There is somewhat more agreement on the general proposition that inequalities of money and private power should not be allowed to distort the political articulation of competing views within the democratic community, but the practical requirements of this proposition are disputed (see, e.g., Beitz 1989, chap. 9; Fishkin 1991, 29-34, 99-101).

Reflecting in part the low level of contemporary political discourse, the past decade has witnessed an explosion of interest in democratic deliberation (see Additional Bibliography, section 4). Many theorists would agree with Jane Mansbridge that "the quality of deliberation makes or breaks a democracy" (Murchland 1991, 125); many have offered normative descriptions of improved deliberations; the impact of pragmatist and Deweyan analogies between scientific and political communities has been substantial. Practical and institutional proposals have, however, lagged behind, in part because deliberative ideals run up against some harsh realities of campaigns and representation in mass democracies, but also because, taken to extremes, the emphasis on deliberation can turn into a romantic escape from politics. Sound deliberation will improve the articulation of differences, but it will not necessarily erase them. Not all deliberation will be conducted as a quest for the common good, and unforced consensus will rarely be its product. Deliberation is an indispensable aspect of politics, but not (in most circumstances) a full substitute for the exercise of power.

Rising interest in deliberation is in some respects an aspect of the broader concern with direct citizen participation in democratic governance. While the necessity of some sort of representation in the modern nation-state can hardly be denied, some theorists have taken this fact as proof of the superiority of smaller political units, and many have looked to local politics, grassroots organizations, and group rights as counterweights to large-scale representative institutions. The classic Madisonian argument that representation enhances deliberation is not much in vogue; the perfectionist Arendtian claim that only direct participation develops and expresses citizens' humanity has carried the day (for quintessential statements, see Barber 1984 and the essay by Pitkin and Shumer in Murchland 1991, 106-14; for a thoughtful dissent on behalf of representation and a more skeptical view of politics generally, see Kateb 1981).

One difficulty with the participatory ideal, evident in practice but underexplored in theory, is its tendency to shift power toward those with greater inclination and opportunity to invest substantial time and energy in political activity. As students of post-1968 Democratic Party presidential nominating processes have long argued, this shift does not necessarily lead to results that are more representative of all interests and views within the relevant community.

Cutting across specific debates about equality, deliberation, and participation is the broader question of how far democracy's writ runs. Some theorists have argued that the existence of a hierarchically organized corporate economy is incompatible with the principles of a democratic polity (e.g., Walzer 1983) while others offer a less unitary view (e.g., Miller 1989, chap. 9). This is one manifestation of the wide-ranging disagreement concerning the amount of differentiation that does, or should, exist within a liberal democracy; compare, for example, Stephen Macedo's characterization of liberalism as a comprehensive regime with Charles Larmore's critique of that "expressivist" idea (Macedo 1990, chap. 2; Larmore 1987, chaps. 4 and 5).

Markets

During the past generation, the long-running theoretical debate between friends and foes of market economies has been enriched by practical experience in the advanced industrialized West, in nations struggling with the legacy of Stalinism, and in post-colonial regions experimenting with different models of development. We are now in a position to evaluate what has been gained, lost, and learned (or relearned).

The market, it is now widely acknowledged, is what its staunchest defenders always insisted: a remarkable mechanism for transmitting information and for inducing change. It promotes efficiency, generates wealth, fosters individual mobility and opportunity, and increases personal freedom (see Miller 1989, part II). These are not inconsiderable advantages; they help explain the rise of conservative parties throughout the West during the 1980s, the delegitimation of command economies in Eastern Europe and the former Soviet Union, the attack on state influence over the economy throughout Latin America, and the increasing influence of the Asian model of development vis-a-vis the Tanzanian socialist model so popular in the 1970s.

But as recent experience suggests, there are important entries in the debit column as well. The

market is insensitive to the distribution of income and wealth among economic classes and geographical locations; indeed, there are indications that under contemporary conditions it tends to exacerbate preexisting disparities. Left to its own devices, the market does little to alleviate the burdens of the dislocations it induces: witness the struggles of communities and regions dependent on declining economic sectors. To individuals as well as firms, the market presents various barriers to entry that are bound to have unequal impacts on different social groups, especially when (as today) educational attainment commands an increasing wage and mobility premium. As currently organized, the market does little to ameliorate tensions women experience between workplace and family or to reduce persistent inequalities flowing from this tension as well as from continuing discrimination in pay and promotion. The market is structured by rules that it neither creates nor enforces, so if the political sphere does not exercise its authority appropropriately vis-a-vis the market, inefficiencies and scandals result. The market does not achieve a self-regulating balance between consumption and investment or, for that matter, between short-term and long-term interests. And added to these difficulties rediscovered in recent years are the classic kinds of market failure known (if sometimes underestimated) all along: imperfect information, externalities not factored into perceived prices, and inadequate provision of the public goods that undergird sustainable economic growth.

To the extent that the liberal democratic project is bound up with rising material aspirations, its historic link to (suitably regulated) market economies cannot be regarded as accidential. But that is not the end of the matter. To begin with, most theorists deny any easy identification of distributive justice with the outcome of unadjusted market processes. Whether one invokes the criterion of need, or desert, or equal respect, or equality of opportunity, the operation of the market is bound to seem in some measure morally arbitrary, if not repugnant. The issue of how the imperatives of aggregation and distribution are related is one of the staples of contemporary theory. Rawls's "difference principle" represents an influential proposal for balancing these two imperatives, seen as partially opposed. It is also possible to argue, as do some libertarians, that distribution has no standing independent of the market (see Kymlicka 1990, chap. 4; Miller 1989, chap. 2); with Ronald Dworkin, that aggregate growth has no independent normative status (Dworkin 1985, chap. 12); or, with some advocates of workplace democracy, that the motivational effects of a more just distribution will simultaneously promote enhanced productivity and growth (Reich 1989, chaps. 9 and 23).

A second objection to markets goes deeper. The commitment to growth can be questioned on both empirical and normative grounds: empirically, as resting on counterfactual assumptions about resources, technology, and the global "carrying capacity"; normatively, as embodying a rapacious and exploitative attitude toward a nature whose beauty is to be cherished and integrity respected. These considerations have sparked the rise of "Green" parties in Europe and of a worldwide environmental movement, and they have helped revive the centuries-old tradition of anti-modernist thought, the latest wave of which seems inspired by Heidegger. It remains to be seen, however, what will happen if democratic majorities come to see a conflict between the maintenance of the environment as a public good and the opportunity to acquire private goods historically afforded by economic growth. I might add that as the walls separating the capitalist West and socialist East fell, it became apparent that polities linked to market economies had been far more responsive than were those of command economies to issues of environmental degradation.

The third, perhaps most fundamental, objection to markets focuses on the quality of human relationships it entails, within the process of production and for society generally. This critique, propounded in overlapping ways by Marxists, utopian socialists, and traditionalists, emphasizes the dehumanizing effects of specialized labor, the cash nexus, the relentless destabilization of established communities, and the loss of understanding and control summed up in the term "alienation."

This objection persists because it reflects real problems. The division of labor is indeed humanly problematic; Adam Smith himself argued that "The man whose whole life is spent in performing a few simple operations...generally becomes as stupid and ignorant as it is possible for a human creature to become." Materialism is unattractive and hardly conducive to moral depth; the constant dynamism of markets undercuts the desire for security and stability; and so forth.

But what is the standpoint from which these criticisms are launched? A central teaching of the biblical tradition is that human labor is alienated, not accidentally or temporarily but intrinsically. From this standpoint, which deserves more theoretical consideration than it usually receives, the demand for an end to alienation represents a covert desire for the (impossible) return to Eden. Or if we wish to remain on the purely secular plane, we may remind ourselves of the old joke: What is the difference between capitalism and communism? Capitalism is the exploitation of man by man; communism is just the reverse.

To repeat, this is not to say that the underlying normative concern -- for the development of healthier individuals and societies -- is beside the point. Indeed, the liberal reform impulse in the West during the past 150 years has worked to make market relations of both production and exchange more decent and humanizing, while preserving (as Marx also sought to do) the most dynamic, productive, and liberating elements of market systems. The results are imperfect, the effort incomplete and ongoing. But what is the alternative? The return to artisanship and small-scale production represents at best an option within, not a substitute for, the market; and as the work of Robert Lane, among others, suggests, there is little evidence that in practice (as opposed to ideology) nonmarket systems score higher along key normative dimensions than do markets (Lane 1978, 1986, 1989).

Welfare

At the heart of the liberal effort to mitigate the less desirable effects of the market lies the modern welfare state, the practice of which has spawned a significant theoretical literature (Gutmann 1988; Moon 1988). It has been justified variously as the protection of the most vulnerable members of society, as the provision of basic human needs, as the reflection of the liberal commitment to autonomy and consent, or as recognition of basic communal cohesion and shared fate (Miller 1990, 435). Judgments concerning its practical adequacy within any particular community will reflect expectations, frequently competing, as to the purposes it should serve (for a discussion within the U.S. context, see Marmor et al. 1990, chaps. 2 and 7). Welfare policies have been criticized by libertarians, as illegitimate state interference with property and personal freedom; by neo-conservatives, as fostering dependency and thereby perpetuating the ills they are intended to counteract; by egalitarians, as paternalistic and dehumanizing; by traditional leftists, as social pacification masquerading as humanitarianism; and by Foucauldians, as an aspect of the insidious extension of social discipline throughout modern life.

In contemporary U.S. politics, the characteristic bureaucratic mechanisms for the delivery of welfare services have come under attack (from dissidents in both major parties) as cumbersome, inefficient, and diversionary, and an effort is now underway to think through less bureaucratic alternatives. This practical development gives rise to an intellectual challenge, the elaboration of a theory of what might be termed "market welfare," similar to that faced by theoreticians of market socialism.

Social Diversity

Central to the self-understanding of liberalism is the conviction that it provides the widest possible scope for diversity -- that is, for conceptions of the good and ways of life that individuals and groups may select, or to which they may adhere, without bias or interference. This conviction has evoked a range of objections. Some feminists and multiculturalists argue that in practice liberalism tacitly endorses and enforces a constraining (patriarchal, racist, Eurocentric) vision of human life. This raises the question, much debated among these critics, whether the cure for the diseases of liberalism is more liberalism -- a society more aware of and responsive to the full claims of diversity -- or rather a break with liberalism in the name of some version of separatism.

Within liberal theory, the discussion of diversity has revolved around the issue of "neutrality." Can liberalism be understood as a generalization of the idea of religious toleration, neutral with respect to ways of life and conceptions of the good? On which understanding of neutrality is this claim most plausible? During the 1980s, the pro-neutrality arguments of liberals such as Rawls, Dworkin, and Ackerman have been countered by liberals who believe that neutrality is not possible or, if possible, not obviously desirable. Still others argue that the issue is not universal theory but rather contextual commitment: neutrality means opposition to historically specific kinds of state intervention. Amidst this proliferating debate, a rough-and-ready consensus may be shaping up: while no form of political community can be fully neutral in any sense, liberal communities are more capacious and accommodating than any others, and they possess the inner resources to reflect on, and in some measure rectify, their historic biases and constraints (on neutrality, see generally Additional Bibligraphy, section 9).

The neutrality discussion led to, or joined with, the question of community. During the 1980s, communitarians suggested the liberal emphasis on diversity came at the expense of devotion to the common good and that, taken to an extreme, it might undermine patriotic identification with, and even the viability of, liberal societies. The liberal response took three forms. It was argued, first, that the rhetoric of the common good had historically functioned as a cloak for elitism, militarism, and state oppression and that suspicion of this category was amply warranted; second, that liberalism embodied a conception of justice that all citizens could reasonably embrace and that could serve as an adequate basis for shared commitments and common endeavors; and third, that over and above justice, there was a distinctively liberal conception of the good that informed liberal communities.

Another count in the communitarian indictment was the claim that liberalism could not accommodate any adequate account of virtue. In the wake of the relentless liberal corrosion of shared beliefs and practices on which the virtues depend, we were now living "after virtue;" but life under such circumstances was barren, conflict-ridden, and degrading (MacIntyre 1981). A related thesis, of civic republican inspiration, was the charge that the privileged position of self-interest within liberal thought (and life) ruled out virtue understood as selfless devotion to the common good.

Once again, the liberal response was complex. Influenced by an interpretation of early modern history, some liberals argued that the distinction between selfishness and selflessness does not map neatly onto the distinction between good and bad; piety can produce more cruelty, and idealism more evil, than selfishness ever did (Holmes 1989, 233). Others argued that liberalism was by no means devoid of an understanding of virtue, one distinguished from the contemplative, republican, and Christian conceptions, to be sure, but not without its own dignity and integrity (Galston 1991a; Macedo 1990; Shklar 1984). It does appear that it is easier for liberal theory to accept a conception of the virtues as instrumental to the preservation of liberal societies than as intrinsic to human excellence. But in circumstances of deep practical and philosophical disagreement as to the content of intrinsic excellence, this inclination is not obviously a disadvantage.

Privacy and Individual Rights

Beginning with its effort to decouple political life from intractable religious disputes, liberalism has employed a distinction between the public and private spheres. As Nancy Rosenblum has observed, the boundary has been anything but fixed and inflexible. Still, its existence (and corresponding arguments about its location) has been constitutive for liberal thought. Therein lies a perennial liberal vulnerability: it can always be charged that a matter of public concern has been erroneously immunized from public scrutiny or that a matter deemed "private" illicitly influences public deliberation (Rosenblum 1989, 5, 7).

In past generations this charge was most likely to be leveled by Marxists against the privatization of economic relations; today its most usual provenance is feminists critical of the privatization of gender and family relations. It can be argued that gender inequalities distort liberal politics, or that liberal politics defines and defends such inequalities; at any rate, that the liberal state cannot remain neutral with regard to them (Okin 1989). The difficulty is to determine, if the personal is truly the political, how the public/private distinction is to be preserved in any form. This is one reason why many contemporary feminists have altogether rejected the liberal problematic.

Another reason is the liberal focus (critics would say fixation) on rights. Here many feminists close ranks with communitarians, of whom they are otherwise quite critical. Rights are abstract, but life is concrete; rights are individual, but life is interpersonal and social; rights are defensive and conflictual, while what we need is connection, care, and cooperation; rights pretend to universal validity, when they are at best dubious social constructions (Gilligan 1982; Pateman 1988; Ruddick 1984).

The liberal defense of rights has historically taken fear of disaster as its point of departure: rights may be, or do, much of what their critics allege, but where would we be without them? In our desire for a more caring society and more meaningful politics, let us not forget the danger of tyranny. Besides (it may be said in a more affirmative mode), rights help secure the possibility of true individuality, pluralism, and shifting involvements. If we really prize the opportunity to shed traditional roles and undertake various experiments in living, rights are needed to secure the structure of liberty (see the essays by Shklar, Kateb, and Rosenblum in Rosenblum 1989). In a more theoretical vein, defenders of rights argue that they are far more than "nonsense upon stilts," as charged by classical utilitarians, or fictions on a par with belief in unicorns and witches, as alleged by contemporary communitarians (for a survey, see Galston 1991b).

The debate over rights is part of a larger dispute over the appropriate way of conceiving the relation between individuals and communities. During the 1980s, communitarians typically charged liberals with embracing the myth of the presocial individual, of overlooking the myriad ways in which individuals are socially constituted, and therefore of overestimating the extent to which individuals can separate themselves from social aims and attachments. Liberals counterattacked, charging communitarians with historical inaccuracy and philosophical ineptitude: liberalism has never been so foolish as to deny that we become human only in society, but it does not follow that society must be organic or solidaristic. The liberal idea of individualism is political, not metaphysical, directed at illicit forms of public or private power based on unsustainable claims to knowledge and personal superiority.

By the end of the 1980s, however, the debate had subsided somewhat and was in the process of being reconfigured. One of its initiators, Charles Taylor, suggested (some would say conceded) that the contending parties had talked past one another. "Ontological" issues divide individualists from holists, while "advocacy" issues

divide (say) partisans of individual rights from defenders of solidaristic communities. Taylor's thesis is that while there is at least some connection between them, one's stand on the former set of issues does not rigidly determine any position on the latter: for example, it is possible to combine holism at the ontological level with individualism on the political plane (Taylor 1989). If the dispute does not completely disappear, it is to say the least radically reconfigured. In particular, attention can shift to the arena of advocacy, in which (as many commentators have observed) communitarians have been remarkably reticent about advancing substantive counterproposals to classic liberal positions.

This lengthy chronicle of controversies between supporters and detractors of liberalism could leave the impression that recent liberal thought is more monolithic than it is. In fact, as in so many other parts of political theory, liberalism witnessed a notable proliferation during the 1980s. There were numerous disagreements over the scope of liberal rights, the requirements of liberal equality, and the content of liberal justice. Deontological liberalism was developed and denounced. Some theorists revived Millean utilitarianism as a basis for modern liberalism, while others sought support in Hegel, Aristotle, Montaigne, or Emerson, among many others. Following Isaiah Berlin, some argued that liberalism expresses, or requires, the existence of plural and incommensurable values, no one of which enjoys clear priority; others pursued Ronald Dworkin's suggestion that liberalism represents the working out of a single master value, a distinctive conception of human equality.

The variety of substantive liberal positions was almost matched by the range of justificatory strategies. Rawls was thought by many to have shifted from an argument offered as valid regardless of time and place, appealing to general features of human rationality and conditions of existence, to an argument presenting itself as an interpretation of democratic public culture (Galston 1991a, chap. 7). Richard Rorty followed up his critique of foundationalism with an unabashed account of liberalism as ethnocentric (Rorty 1983). Critics of particularist liberalism (e.g., Waldron 1987) countered by arguing that liberalism is unintelligible without some kind of universalist claim. Pursuing this strategy, some thinkers inspired by Habermas, Dewey, and Aristotle systematically explored the structural parallels between the scientific conditions of free inquiry and the political conditions of a free society (Anderson 1990; Salkever 1990; Spragens 1990). Still others favored a mid-range, contextualist approach, presenting liberalism as the outcome of a series of specific historical choices between better and worse political alternatives, understood deliberatively and prudentially (Herzog 1989; Holmes 1984). At present there is arguably more agreement among liberals (indeed, between liberals and non-liberals) concerning concrete political judgments than concerning justificatory strategies; but whether this fact suggests the need to abandon, or rather to reemphasize, the process of justification is itself the subject of controversy.

IV. The Future Agenda of Political Theory

While not exactly unjudgmental, the preceding remarks have been as nearly descriptive as I could manage. In this final section I wish to take up the editor's charge to discuss the future agenda of political theory, which will necessarily (she notes) "reflect a more personal view of the field."

As a general matter, I believe, theorists should try harder to take real political controversies as their point of departure and to attend to the terms in which these debates are conducted. There should be less top-down theorizing or, to put it another way, more of an effort to employ the method of reflective equilibrium -- judgment of abstract principles in light of concrete political realities, not just vice-versa -- that Rawlsians often preach but seldom practice (see Gutmann 1989, 342). Theorists should also reduce their reliance on a characteristic method of analytical moral philosophy: the employment of micro-examples either forcibly extracted from their real-world contexts of meaning, or invented outright.

If theorists are to begin with actual political issues, they must work harder at the outset to be empirical. By this I mean two things. The theoretical agenda must be set to a greater extent by the major practical questions now before us, and we must try to see the issues as they are, not as ideologically prepackaged. This is not to say that political theory can return to the epistemology of immaculate perception; it is to say that the basic contours of most issues are marked out by some combination of empirical research and common sense that political theorists (and for that matter political leaders) ignore at their peril.

The night before I wrote this paragraph, for example, Mikhail Gorbachev resigned as president of the defunct Soviet Union, acknowledging among other errors his inability to see the forces of disintegrative nationalism at work in the empire he ruled. The ideology of socialist universalism had blinded him to what was happening before his eyes. During the 1990s, political theorists will render themselves as irrelevant as Gorbachev if they ignore the global nationalist tide -- not to mention the shared language, ethnicity, history, and religion so often at its heart.

Even when ideology does not lead to denial, it can generate distortion. The Marxist picture of liberal

democratic society as comprehensively exploitative began, and ended, as a caricature; so too the Foucauldian picture of liberal democratic society as comprehensively disciplinary. To be sure, each of these orientations provokes thought and casts light onto previously darkened areas. But both mistake a part for the whole; both offer a totalizing interpretation of social reality based on that part; and neither in the last analysis can validate its preferred counterproposal without referring to the liberal norms it seeks to expunge. As Nancy Fraser has argued, "By claiming that panoptical autonomy is not the horror show Foucault took it to be, the Habermassian humanist challenges him to state, in terms independent of the vocabulary of humanism, exactly what is wrong with this ...society and why it ought to be resisted" (Fraser 1985).

Theorists too often assume that pointing to flaws, real or alleged, in the theoretical justification of a particular political system suffices as a critique of practices within that system. It is not difficult, for example, to uncover difficulties in various philosophical accounts of rights. But that is hardly the end of the matter; we must also attend to the ways in which rights function in practice, and to the real-world consequences of trying to do without them. Empirically sensitive theory will reason from politics to texts and arguments, not just the other way around. The alternative is the political equivalent of the aeronautical proof that bumblebees cannot fly.

If theory does become more empirically aware, we may expect a diminished role for what Ian Shapiro has called "gross concepts" (Shapiro 1989). We will spend much less time arguing about liberalism versus communitarianism (or civic republicanism), foundationalism versus contextualism, and so forth. Our theoretical disagreements will be finer-grained; they will illuminate, rather than substitute for, political controversies. Rather than going on holiday, our words will get to work.

Admittedly, the task of thinking empirically involves more than common sense and is greatly complicated by the sheer proliferation of research. As David Miller has noted, perhaps the greatest practical dificulty that now faces political theory "is simply the immense body of empirical material that modern political science has collected" (Miller 1987, 385). But this is an argument for intelligent selection, not blithe obliviousness. Political theorists could do worse than read this volume cover to cover before returning to their business.

This is not to say that political theory should adopt a merely passive or receptive stance toward political science. Theorists can perform an important critical function by pointing out the role of unacknowledged conceptual and normative commitments in empirical research programs. In some cases, theory can play a constructive role in the formulation of such programs (consider the impact of Arrovian theorems on research into agenda-setting processes within legislative bodies). Theory can even shape sophisticated empirical techniques to test the validity of its own internally generated hypotheses, as in the case of simulated "veil of ignorance" experiments (Frohlich and Oppenheimer 1992). The plea is only for a greater receptivity to the world on the part of theorists all too ready to distance themselves critically from it, or to impose themselves imaginatively upon it.

This should not be interpreted as a call for quietism, let alone for the abandonment of the task of critical appraisal central to political theory since its inception. It is a proposal as to how these tasks can be carried out more effectively. Another proposal in the same vein rests on the proposition that criticism is not enough. Theorists dissatisfied with the status quo have an obligation to set forth an alternative that is both preferable and practicable. In political theory, as in public life, the operative question must always be, "Compared to what?" Whether or not it is substantively correct, Winston Churchill's famous comment -- that democracy is the worst form of government except for all the others that have ever been tried -- has methodological force for theory as well as practice. It is one thing to point out incompleteness or imperfection in a political system, quite another to conclude (on that basis) that we should refrain from supporting it. Political theorists must take more seriously the possibility that our most cherished ends conflict with one another and that even under the most favorable circumstances, neither individuals nor societies can "have it all" (Barry 1990, xxxix-xliv; Lukes 1989). A simply utopian critical standpoint -- one resting, for example, on an unrealizable vision of complete liberation or perfect harmony -- is worse than useless.

An implication is that normative theory must have definable consequences. In this vein, let me propose a new "difference principle": every theorist should be required to ask, If what I say were adopted as valid or true, what difference would it make? The answer to this question need not take the form of new policies or institutions or (r)evolutionary movements; it may involve only a changed stance toward politics on the part of the theorist and others similarly situated; but it must be definable and (if the theorist is acting responsibly) explicitly defined.

Another is that political theory must be made more publicly intelligible than much of it is today. This is not to say that every argument can or should be conducted non-technically. It is to say that the principal conclusions of theory, and the considerations supporting

them, must be translated into forms of expression open to scrutiny by a wider audience. The alternative is a mandarin enterprise sealed off from political life, neither nourishing nor nourished by nonacademic perspectives. In this regard, indications of seriousness would be, on the one hand, an increased propensity on the part of individual theorists to publish in semi-popular journals such as *The New Republic*, *The American Prospect*, *Dissent*, *The Atlantic*, and *Commentary*, and on the other, a greater disposition on the part of the profession generally to honor and reward a more generally accessible style of theorizing. On the former front, anyway, some recent developments have been encouraging; but it is too soon to declare the long-awaited return of the public intellectual.

With regard to the substantive agenda of future political theory, let me offer a few proposals that go beyond the general injunction of increased empirical and political awareness.

During the 1980s, neo-pragmatic attacks on "foundationalism" combined with the Nietzschean hermeneutics of suspicion and the skeptical residue of positivism to call into question the rational status of normative assessment. The result was not what one might have expected -- diminished quantity of, and confidence in, such assessment -- but the very opposite, a kind of blithe normative self-assertion. The "self" doing the asserting could be individual (the Rortyean "strong poet") or collective (the Rawlsian "democratic public culture"), but in either case it was regarded as self-sufficient. But this view of the matter is patently insufficient, for the simple reason that when self-assertions come into conflict it provides no way of adjudicating among them.

Armed with the anti-Cartesian insight that absolute certainty and verification are not to be expected, political theorists should apply themselves once again to the task of developing reasonable support for the principles they espouse. This process might involve the kind of sophisticated naturalism invoked by contemporary neo-Aristotelians. It might also involve a cautious reengagment with processes of inquiry that have historically influenced political theory, but which the contemporary revival has set aside -- in particular, metaphysics, the natural sciences, and religion. Along these lines, theorists would scrutinize, more systematically than heretofore, the proposition that our public principles can be "political, not metaphysical." And they would pay more attention to the kinds of inquiries now conducted within the American Political Science Association under the organizational rubrics of "Politics and Life Sciences" and "Politics and Religion." At the very least, a political theory practiced in this way would no longer be so unself-consciously (hence dogmatically) secular or so thoughtlessly confident that recent biological discoveries leave our understanding of political behavior and institutions untouched.

Under the influence of *A Theory of Justice* and analytical moral philosophy, political theory during the past two decades has emphasized the assessment of public policy outcomes and of individual orientation toward public choices; Kymlicka's survey goes so far as to identify contemporary political philosophy with "theories of justice" (Kymlicka 1990, 3-5). These were hardly insignificant concerns, but they pushed into the background questions of institutional design and assessment. (Remarkably, a major influence of Rawls's *Theory of Justice*, which concentrates on the question of designing just institutions, has been to push to the background questions of institutional design and assessment.) There are some welcome signs that this imbalance is now being addressed (Brennan and Lomasky 1989; Elster and Slagstad 1988; Phillips 1991; Rothstein 1992), and one hopes that this countertrend will continue to gather strength. One of the defining characteristics of politics, after all, is the embeddedness of individual behavior within humanly contrived, and revisable, institutional structures; their assesment and reform has now, in the wake of the collapse of communism, assumed an urgent importance without precedent in recent history.

Also shoved aside by the focus on distributional issues were questions of individual character as both formed by, and sustaining, public institutions and policies. Here again there are welcome signs of a turning tide. The easy belief that the efficacy of public endeavors is unaffected by citizens' character (to be determinedly old-fashioned, by their virtue and vice) is giving way to a more complex interplay of social structure and individual agency in which the latter is seen as something more than negligible, and other than a dependent variable (Galston 1988; Wilson 1991). Theorists are increasingly interested in problems of both explicit civic education and undesigned but potent processes of character development. Here as elsewhere, it is vital to rejoin theoretical concerns to empirical political inquiry; for example, many ideologically driven propositions (from both left and right) about the effects of markets on personality turn out to be of at best questionable validity (Lane 1989).

Related to questions of character is the even wider issue of human psychology and its relation to politics. During the 1970s the impact of Freud on political theory, so profound in the 1950s and 1960s, perceptibly diminished. Much liberal theory of the past two decades has deployed a relatively thin and abstract psychological vocabulary, in which motivations of morality and self-interest have largely displaced attention to the passions. In this respect, at least, the recent

resurgence of interest in Nietzsche seems likely to have a salutary effect on our understanding of phenomena such as fear, anger, and resentment (Connolly 1990); so too efforts to explore the political significance of psychologists such as Melanie Klein (Alford 1989, 1990). In general, today's political theorists would do well to remember that many of the most enduringly important theorists of the past achieved their impact in no small measure by bringing stunning visions of human desires and passions to center stage.

Another issue that must be more fully joined in the 1990s is the relation between group identity and liberal democracy. This issue arises both on the Right, with the claims of tradition and fundamentalist communities, and on the Left, with claims based on (inter alia) race, gender, and ethnicity. Specific controversies from multicultural education to affirmative action to free exercise of religion are at stake; so too is the adequacy of a mode of political theorizing that restricts itself to individuals and public structures as core elements. A range of materials could well be brought to bear on these questions -- among others, constitutional conflicts, classic discussions of civil society, feminist arguments, and forms of comunitarianism that abandon the identification of cultural community with political community. Most fundamentally, theorists will have to debate the extent to which the public principles of a liberal democratic order should be extended to, and if necessary imposed upon, the inner workings of its multiple and diverse subcommunities (for an important start see Kukathas 1992; Kymlicka 1992).

The final item in this informal agenda for the 1990s is the nature and limits of political authority. This is an issue that arises in the context of American political culture, historically torn between mistrust of, and need for, central authority. And it is an issue woven in various ways through the fabric of contemporary political theory, in the work of libertarians, liberal skeptics such as Michael Oakeshott, Isaiah Berlin, and Richard Flathman, liberal proceduralists such as Stuart Hampshire and H. L. A. Hart, and democratic dissenters such as George Kateb (drawing on the Emersonian tradition) and William Connolly (drawing on Foucault). The problem, as Flathman puts it, is that while we cannot possibly do without authority altogether, we must never lose sight of the ways in which it (necessarily) restricts the processes of free reflection and judgment (Flathman 1989). The challenge, which is both moral and institutional, is to find ways of chastening authority, of maximizing spaces for unforced individuality and commonality, that do not render authority incompetent to perform its essential tasks -- in particular, the necessarily institutional protection of the very space for freedom that can seem, but is not, wholly independent of the practice of authority (for a provocative effort to meet this challenge, see Flathman 1992).

As I have stressed throughout this essay, the 1980s have witnessed both a continued gap between political theory and political science and an enormous proliferation of theoretical agendas and styles. To some considerable extent these trends are inevitable. Still, I cannot avoid the suspicion that the ratio of synthesis to analysis in contemporary theory is far too low. Parallel to developments within separate theoretical arenas, we need more discussion about how they might fit together into a more systematic theory of politics. This amounts to a plea for something less than Kantian architectonic but more than postmodernist bricolage, perhaps along the lines of the medieval "order of the sciences."

I referred earlier to Brian Barry's proposal a generation ago that welfare economics, game theory, and rational choice should be used to clarify traditional questions of political value. No doubt some today would wish to amend this proposal, and others to reject it outright. It would surely have to be broadened to include the significant theoretical developments of the past thirty years. But it is at least headed in the right direction. In our theory as well as our politics, we should devote as much attention to the "unum" in the 1990s as we gave to the "pluribus" in the 1980s.

Notes

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2. Feminist Thought
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4. Utilitarianism
5. Community/Republicanism/Virtue
6. State and Civil Society
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8. Liberalism
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10. Liberal Justice/Equality of Opportunity
11. The Nietzschean/Postmodern Controversy

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3

Feminist Challenges to Political Science

Susan J. Carroll and Linda M. G. Zerilli

The study of women and politics within the discipline of political science was stimulated by and has evolved simultaneously with the contemporary feminist movement. Prior to the emergence of the feminist movement in the mid-1960s, few books or articles pertaining to women were written by political scientists (a notable exception being Duverger 1955), and from 1901 to 1966 only eleven dissertations focusing on women were completed (Shanley and Schuck 1974). The Women's Caucus for Political Science, founded in 1971, began in 1972 to sponsor several papers on gender at the annual meetings of the American Political Science Association, and during the early to mid-1970s the first few path-breaking books on women and politics were published (e.g., Amundsen 1971; Kirkpatrick 1974; Jaquette 1974; Freeman 1975).

From such humble origins, the subfield of women and politics grew at a rapid pace. By the early 1990s the numbers of papers, articles, and books written by political scientists focusing on women and politics or feminist theory had grown considerably. For example, more than 60 gender-related papers were presented at the 1992 Annual Meeting of the American Political Science Association, and in 1991, *Women & Politics*, a scholarly journal devoted to publishing empirical and theoretical work on women and politics, published 24 articles and reviewed 21 books on women and politics and feminist theory. The growth of a body of scholarship focusing on women and gender within the discipline of political science has paralleled similar (although frequently more rapid) patterns of growth in other disciplines in the humanities and social sciences (DuBois et al. 1985). In addition, work on gender in political science has been strongly influenced by the rapid development of interdisciplinary work in women's studies.

The study of women and politics also became more institutionalized within the discipline throughout the 1970s and 1980s. Two of the most important developments were the establishment in 1981 of the journal, *Women & Politics*, and the formation of an Organized Section on Women and Politics Research within the American Political Science Association in 1986. In addition, in 1986 the political science department at Rutgers University became the first in the country to offer women and politics as both a major and minor field of study toward a Ph.D. Most of the larger political science departments now have at least one faculty member who specializes in gender politics, and many departments now offer women and politics courses as a regular part of their undergraduate curriculum.

The work being done in this rapidly growing field has important implications for all political scientists, not just those who are specialists in women and politics. Feminist scholarship poses a set of questions that challenge the theoretical and epistemological foundation on which the discipline is constructed. Sometimes implicitly and sometimes explicitly, the work being done by feminist scholars raises important questions about both *what* we study as political scientists and *how* we study it.

Framework

This essay will examine the questions posed by women and politics research about what we study as political scientists and how we study it in the context of three analytically distinct categories of research on women and politics. The first category consists of critiques of the ways in which political theory and empirical research in political science have traditionally excluded women as political actors and rendered them either invisible or apolitical. The second category consists of research that has attempted to add women into politics, to make them visible as political actors, while accepting the existing dominant frameworks of political analysis. The third category consists of research that calls existing frameworks and assumptions into question; work within this category suggests that our dominant frameworks cannot accommodate the inclusion of women as political actors and that many of the frameworks, assumptions, and definitions central to political science must be reconceptualized.

We would not argue that these categories of work on women and politics are chronologically distinct or that research in any one category is more important than research in another. Although much of the early research on women and politics falls into the first two categories while the third category is of more recent vintage, all three categories of research are apparent in current work. Similarly, all three have made important contributions to our understanding of the ways in which gender and assumptions about gender have permeated the discipline. While we treat these three categories of research as analytically distinct for purposes of this essay, the demarcations among them are admittedly fuzzy. One category tends to flow into another, and the work of a single scholar sometimes cuts across two or even all three categories.

We do not intend to provide a comprehensive review of the literature on women and politics; given the proliferation of work in recent years, such a review would be a monumental task, which fortunately is beyond the scope of this essay. Instead, in discussing the three categories of research, we draw largely on literature from political theory and from American politics both because our specializations are in those fields and because these are the two areas in political science where the greatest amount of work on gender has taken place. Through this review we hope to demonstrate that both feminist theory and more empirically oriented work on gender have posed serious and often similar questions about what we study in political science and how we study it.

Women as Invisible or Apolitical

In political science as in other disciplines, feminist scholarship had its origins in critiques of the ways in which the philosophical canon of political theory and the empirical canon of behavioral political science excluded women and women's activity from their subject matter, often rendered women invisible, and employed stereotypical assumptions about women's apolitical "nature" and their behavior. Although several important critiques appeared in the 1970s (e.g., Bourque and Grossholtz 1974; Shanley and Schuck 1974; Jaquette 1974; Iglitzin 1974; Goot and Reid 1975; Boals 1975; Okin 1979; Elshtain 1979a), recent critiques continue to add to our understanding of the ways that the discipline as a whole and specific subfields within the discipline either have failed to deal with women or have treated women in a stereotyped manner (e.g., Randall 1991; Ackelsberg and Diamond 1987; Nelson 1989; Sapiro 1989; Grant 1991; Halliday 1991).

Inasmuch as political science is interested in questions of citizenship, many feminists have turned their attention to the historical tradition of Western political theory to explain both the invisibility of women as political actors and the sexist attitudes toward women that have permeated much of the discipline. The earliest feminist critiques were concerned with breaking the silence about women that had characterized -- and to some extent still does characterize -- the scholarly literature on the canon of political theory. Even as feminists found that the majority of past political theorists had excluded women from participation in the public sphere, they also found that these theorists had not simply neglected women in their texts (Okin 1979; Elshtain 1981; Saxonhouse 1985; Eisenstein 1981; Shanley 1982). On the contrary, the classic theorists were deeply worried about what they deemed most often to be the disorderly influence of women on political affairs. Women were not simply missing in the canonical texts; rather, they had been read out of the Western tradition by political theory scholars (Jones and Jonasdottir 1988).

In important respects, then, feminist political theorists set out from the start to make visible what their academic colleagues had made invisible, namely women. Although women were present as a subject of no small concern for the classic theorists, they were absent as political actors. Thus feminists were intent to show, first, how the tradition has justified women's exclusion from participation in the public sphere, and, second, how that exclusion has defined what counts as citizenship across a wide historical range of political theories, up to and including modern democratic theory. Because women had been a virtual non-topic within the scholarly literature in political theory, many feminists were concerned initially with chronicling what past political theorists had said about women and showing how what they had said had been used to justify women's banishment from civic life. Focusing on the "patriarchal attitudes" of political theorists from Plato to Hegel, feminists found that most theorists had portrayed women as not fully human or fully rational or fully political beings (Figes 1970; Mahowald 1978; Brennan and Pateman 1979; Clarke and Lange 1979; Pateman 1980a). Whether women were defined in terms of their disruptive sexuality, lack of justice, incapacity for reason, or all of the above and more, the classic theorists had cast them as being utterly deficient in those qualities that were deemed necessary for active participation in the civic community. "Women *qua* women," in short, were "excluded from the public, political, and economic spheres" (Clarke and Lange 1979, viii).

Those feminists who focused on the "blatantly anachronistic or flatly misogynist elements" of the classic texts (Brown 1988, 11) decried "the sexism of political theory" and declared the Western tradition "utterly bankrupt" as a means for advancing feminist theories of

citizenship and sexual equality (Clarke and Lange 1979, xvii; Figes 1970). Even feminists who did not advocate such an outright dismissal of the canon agreed that the classic theorists offered little in the way of a political analysis of the sexual division of labor in the family (Okin 1979; Elshtain 1981; Eisenstein 1981). Part of the problem, as Susan Moller Okin argued, was that political theorists naturalized the family and women's place within it. They viewed women in strictly functionalist terms: "Philosophers who, in laying the foundation for their political theories, have asked 'What are men like?' 'What is man's potential?' have frequently, in turning to the female sex, asked, 'What are women *for*?'" (Okin 1979, 10).

Susan Moller Okin was one among a growing number of feminists who came increasingly to view the less than flattering images of women found throughout the Western tradition as being more than merely incidental to representations of the political. What a theorist said about women, feminists came to argue, was absolutely crucial to how he conceptualized the terms of citizenship (Pateman 1980b; Eisenstein 1981; Elshtain 1981; Saxonhouse 1985). Not only were concepts such as justice, rights, and consent articulated in the absence of women as political actors, but also their meaning was constituted through that absence. This core insight, although not fully developed in the feminist literature of the late 1970s and early 1980s, was crucial for the scholarship on gender for at least three reasons: one, it enabled feminists to contest the "add women and stir" approach to political theory; two, it offered a way of thinking through women's contemporary status as second-class citizens; and, three, it suggested that the legacy of the Western tradition on the discipline of political science has been to treat women as political outsiders whose proper place is in the family.

Critiques of the empirical canon of behavioral political science research have in many respects echoed the themes that have characterized critiques of the political theory canon. Feminists have examined and found problematic the treatment of women in such classic works as: Angus Campbell et al., *The American Voter*; Robert Lane, *Political Life*; Fred Greenstein, *Children and Politics*; Gabriel Almond and Sidney Verba, *The Civic Culture*; Robert Dahl, *Who Governs?*; and Hans Morgenthau, *Politics Among Nations* (Bourque and Grossholtz 1974; Sapiro 1979; Tickner 1991).

Perhaps the most striking observation about the traditional behavioral political science literature is how seldom women are mentioned and how little serious and sustained attention is devoted to explaining their behavior. Regardless of whether women were physically present or absent among the population studied, they are invisible in much of the pre-feminist literature. Even when women were present among the subjects studied, and even when an examination of their experiences would have contradicted the main conclusions of a study (as, for example, Virginia Sapiro maintains would have been the case for Robert Dahl's *Who Governs?*), women were most often ignored (Sapiro 1979).

Nevertheless, a clear and coherent picture of women and their behavior emerges from works in the empirical canon, including several of the books mentioned above, that do explicitly (even if only briefly) examine women's behavior. The portrait is one of women as apolitical at worst and politically deficient at best. Women are portrayed as lacking in political interest and involvement (e.g., Berelson, Lazarsfeld, and McPhee 1954, 25; Campbell et al. 1960, 489-90). They have low political efficacy and belief systems that lack conceptual sophistication (e.g., Campbell et al. 1960, 490-2). They vote less often than men, and when they do vote, they tend to defer to and vote like their husbands (e.g., Campbell et al., 485-6, 492-3). They "personalize" politics, paying more attention to personalities than to issues (e.g., Greenstein 1965, 108; Almond and Verba 1963, 535). They are more conservative in their political preferences and voting (despite the fact that they vote like their husbands) (e.g., Almond and Verba 1963, 535) and less tolerant of left-wing political groups such as Communists and socialists (e.g., Stouffer 1955, 131-55).

Feminist scholars have shown that this political portrait of women is based on research riddled with untested assumptions and methodological flaws (see especially Jaquette 1974; Bourque and Grossholtz 1974; Goot and Reid 1975). Because of the problems that characterize the analysis of women in much of this literature, it is impossible to ascertain fully the extent to which the research reflects an accurate portrayal of women's political behavior at the time and the extent to which the portrayal of women as far less politically engaged and sophisticated than men is a product of gender-related biases on the part of researchers. At a minimum, it seems fair to conclude that the assumptions and biases that are reflected in this research (as discussed below) led to an exaggerated portrayal of differences between women and men.

Early behavioral political scientists accepted unquestioningly the public-private split and the definition of woman as primarily oriented toward responsibilities and activities in the private sphere evident in much of Western political thought (Elshtain 1974). The family was viewed as a monolithic unit (Sapiro 1989; Goot and Reid 1975) with the male as the family head and dominant political representative. A woman's primary obligations were assumed to be to her roles as wife and mother. Men's political behavior became the norm against which women's political behavior was measured

and found lacking (Bourque and Grossholtz 1974). These assumptions not only were untested, but also seem to have been accepted as "natural," with change in women's and men's roles viewed as either inconceivable or undesirable.

This series of interrelated assumptions affected both *what* questions were asked (and, more importantly, not asked) about women's political behavior and *how* women's political behavior was studied by pre-feminist behavioral political scientists. Because women were viewed as apolitical and oriented primarily toward the private sphere of home and family, findings about women's lower levels of involvement and interest in politics were not considered problematic. Political scientists did not empirically investigate the question of why women in the general population did not show higher levels of engagement with politics; rather, they assumed they knew why. Similarly, the near-absence of women from positions in political elites, particularly as public officeholders, was not a question that intrigued or disturbed political scientists. Rather, the under-representation of women among political elites became a question worthy of investigation only after the advent of feminism both outside and inside the discipline.

The ways in which the assumptions of pre-feminist behavioral political scientists affected their work are even more evident when it comes to *how* women's political behavior was studied. Feminist critics have devoted considerable attention to showing that the application of supposedly objective research methods to the study of women's political behavior in the empirical canon was hardly value-free.

Feminists have pointed to a number of ways in which behavioral political scientists can be accused of practicing "bad science" in investigating women's attitudes and involvement. Several scholars have provided examples demonstrating that political scientists exaggerated or even misconstrued their findings to fit their preconceived notions about women's nature and the ways that women and men differ (e.g., Goot and Reid 1975; Bourque and Grossholtz 1974).

When researchers found legitimate differences between females and males, they almost always interpreted the differences in such a way as to make females appear apolitical. For example, Fred Greenstein asked children what they would do if they could change the world; he noted that girls were more likely to suggest "a distinctly nonpolitical change" such as "Get rid of all the criminals and bad people" (Greenstein 1965, 116; Bourque and Grossholtz 1974, 243). Similarly, Susan C. Bourque and Jean Grossholtz pointed out that findings of lower levels of political efficacy for women when compared with men were interpreted as a sign of men's political competence and women's incompetence. They suggested alternatively that such findings might reflect women's "perceptive assessment of the political process" and that perhaps men "express irrationally high rates of efficacy because of the limitations of their sex role which teaches them that they are masterful and capable of affecting the political process" (Bourque and Grossholtz 1974, 231).

Researchers sometimes asked questions which were clearly biased in such a way as to elicit responses that would construe males as political and females as apolitical. For example, Lynne B. Iglitzin, who examined questionnaires used in three classic political socialization studies (Greenstein 1965; Hess and Torney 1968; Andrain 1971), concluded that in many of the questions, "Politics is portrayed as a male-only world by the unvarying use of the male gender, the pictures chosen, and the limited and stereotyped choices of answers provided" (1974, 33).

Finally, political scientists engaged in a practice that Bourque and Grossholtz termed "fudging the footnotes" (1974). Bourque and Grossholtz found that some political scientists cited sources for statements about the political attitudes and behavior of women that, when checked, did not really say what had been attributed to them. For example, they noted how Robert Lane's contention in *Political Life* that political conflict between husbands and wives is usually resolved by the wife being persuaded by the husband was contradicted by the study he cited in support of his contention. Instead of persuasion by the husband, the study found mutual influence between husbands and wives and a tendency for children to follow the mother's partisan predisposition in cases where parents disagreed on political party preference (1974, 234-5). Bourque and Grossholtz provide other examples showing that early behavioral researchers sometimes "fudged the footnotes" by citing data from earlier studies without the qualifications and context that had accompanied them in the original text, thus misrepresenting the original findings.

While the "bad science" that characterized the examination of women's behavior in some of the classic studies can be corrected by eliminating the biases of the past and practicing "good science" in future research, some feminists have raised criticisms of behavioral political science research that are more fundamental and not nearly as easily addressed as are mere charges of bias. One of these critics is Jean Bethke Elshtain, who has claimed that the problem lies with an epistemology that separates facts and values. She has explained:

> The problem is more complex and fundamental than any charge of bias. *It is that every explanatory theory of politics supports a particular set of normative conclusions.* To have an explanatory theory,

> the analyst must adopt a framework linked, implicitly if not explicitly, to notions of human nature and human purposes. This framework sets the boundaries of the phenomena to be investigated. Some factors of social life will be incorporated, and others will be expunged from view before research begins (1979a, 242).

In Elshtain's view (a view shared by many other feminist political scientists), behavioral political science has adopted a framework that excludes much of what women do from political analysis and relegates most women to a private realm outside of politics. Elshtain has observed:

> Within mainstream political science, what has been described traditionally as politics tends to factor women out of the activity and has excluded for many years the questions raised by feminists. Such questions are relegated to a sphere outside organized political activity and are dismissed as private "troubles" (1979a, 243).

Like Elshtain, Barbara Nelson has also pointed to problems with both the epistemology and the content of political science. While acknowledging that many feminist political scientists would wish to retain the systematic study of events as one type of research, she has argued that feminist critics raise questions about "the universalism of the findings and disinterestedness and unconnectedness of the observer" that cannot be corrected simply by acknowledging or attempting to remove values or biases from empirical research (1989, 22). She has observed:

> The study of gender is revolutionary because it threatens the belief that most existing research is gender neutral and universalistic. In political science, like the social sciences in general, women may not fit into existing social theories, which suggests that in many cases social knowledge believed to be cumulative may not be cumulative at all (1989, 22).

As for content, Nelson has concluded, based on her analysis of three popular introductory textbooks and two recent self-reflective volumes on the state of the discipline, that an overemphasis on electoral politics is one of the reasons why political science has devoted so little attention to women and their experiences. She has urged that as a discipline we must:

> ... include in the study of politics not only the recognition of the exclusion of women from what is traditionally political, but also the inclusion of politics in what women have traditionally done. We shall have gone a long way to creating a more inclusive political discourse if we give attention to families, communities, voluntary groups, social movements, and the welfare state, just to name a few topics. The discipline needs a two-fold strategy, emphasizing that all political subjects are gendered while also giving attention to those areas and concerns where women have traditionally put their political energy (1989, 21).

Feminist critiques of both the philosophical canon of political theory and the empirical canon of behavioral political science suggest that contemporary political scientists should regard much of what was written about women prior to the 1970s with skepticism. The pre-feminist empirical literature is so heavily influenced by assumptions about women's apolitical nature that it is difficult to separate scientific fact from fiction. The end result is that we know relatively little about women's political behavior prior to the development of feminist scholarship both because women were so seldom studied in any serious and sustained way, and because when they were studied, they were examined through a biased lens.

The feminist critiques of the empirical canon also provide evidence that a positivist epistemology can (and perhaps always does) mask an underlying set of normative assumptions about human nature and, in particular, about women's nature. Many feminist scholars within the discipline today are very wary of claims to objective, value-free research and universal truths. Their wariness stems, in part, from the discovery of strong biases in pre-feminist scientific research regarding women's political behavior. Their wariness also stems, in part, from the realization that women's absence from the subject matter of much behavioral research means that the universal truths supposedly being discovered through this research are, in fact, gendered -- i.e., based on men's experiences of politics, not women's.

Finally, feminist critiques of the philosophical canon of political theory and the empirical canon of behavioral political science call into question our conception of politics with its foundation in the split between public and private life. Rather than take that split at face value, feminists have endeavored to analyze it historically and to show how a more critical conceptualization of it would significantly alter how we think about the meaning of citizenship and about women as citizens.

Debunking the Myth of the Invisible, Apolitical Woman

Much of the theoretical and empirical work undertaken by women and politics scholars during the 1970s and 1980s was aimed at making women visible in political theory and behavioral research and correcting the biases of the past; this type of work continues into the present. Women and politics scholars utilized existing disciplinary (and sometimes nongendered interdisciplinary) frameworks and approaches to examine both the portrayal of women in the works of major political theorists and women's political behavior at citizen and elite levels. These scholars attempted to dispel both the notion that women were apolitical beings and the idea that women's (and men's) roles in society were dictated by nature and thus immutable.

This body of women and politics literature is important because it demonstrates the importance of gender as a category of analysis within mainstream political theory and political science. However, it is also important for another reason. In subjecting women and women's experience to political analysis, in applying existing disciplinary frameworks and approaches to women, this research pointed to possible limitations and inadequacies in those frameworks and approaches. Focusing on women and women's experience in an attempt to fit women into the picture often revealed that important adjustments were necessary; women did not always fit simply and neatly into the existing picture. In some respects this research suggested that important material was left out and that the existing picture was only a partial representation of political reality; a wider-angle lens was needed. In other respects this research suggested that the picture itself needed to be recomposed in order to encompass women's experience.

In an effort to complicate the familiar image of public man and private woman, several feminist political theorists have rethought the development of the modern public sphere through the historical lens of women's political participation and experience. For example, Joan Landes (1988) has argued that the emergence of the bourgeois public sphere in enlightenment France cannot be understood adequately without attending to the enormous social influence of salon women in the Old Regime. No mere ornament to the Royal Court, the salon "belonged to a wider urban culture," and was "distinguished by its 'worldliness' and cosmopolitan character" (1988, 25). As key players in the "system of advancement for merit" (1988, 24), salon women were crucial to the social changes that brought about the demise of the landed aristocracy.

By making gender a central category of her analysis, Landes has enriched our understanding of the class struggles that altered the face of eighteenth-century Europe. She has shown that an ideology of female domesticity was crucial to the social ascendancy of the middle classes, and that the modern public sphere was historically constructed through the exclusion of women. When women and their interests are properly accounted for, Landes has argued, "enlightenment begins to look suspiciously like counterenlightenment, and revolution like counterrevolution" (1988, 204). The implications of her claim that the eighteenth-century citizen was not a generic but rather a gendered category are far-reaching: "the [contemporary] women's movement cannot 'take possession' of a public sphere that has been enduringly reconstructed along masculinist lines" (1988, 202).

The tension that Landes has located between political equality and sexual inequality has been taken up by several other feminist theorists, who have been concerned to challenge conventional tales about the historical emergence of modern democracies. Mary Lyndon Shanley (1989) has pursued a similar line of investigation into Victorian debates about universal suffrage and female domesticity. Shanley has found that political women challenged nineteenth-century liberalism to live up to its promise of equal rights for all. While recognizing the failure of most Victorian feminists to question the sexual division of labor in the family and to advocate economic equality for the laboring masses, Shanley has also called our attention to how they exploited the tenets of liberal ideology to serve a wide range of progressive ends. In contrast to Landes, she has suggested that the contemporary women's movement can continue to do so.

One of the most important contributions of feminists who have integrated gender into their work on the origins of liberal society has been to highlight the tension between women as rights-bearing citizens and women as an oppressed sex-class. Some scholars have argued that this tension is the very motor behind liberal feminism, and that it has the potential to radicalize the mainstream women's movement (Eisenstein 1981; Shanley 1989). As Zillah Eisenstein has observed:

> Liberal feminism involves more than simply achieving the bourgeois male rights earlier denied women, although it includes this. Liberal feminism is not feminism merely added onto liberalism. Rather, there is a real difference between liberalism and liberal feminism in that feminism requires a recognition...of the sexual-class identification of women as women....This recognition of women as a sexual class lays the subversive quality of feminism for liberalism because liberalism is premised upon women's exclusion from public life on this basis. The demand for the real equality

> of women with men, if taken to its logical conclusion, would dislodge the patriarchal structure necessary to a liberal society (1981, 6).

In a later book, *Feminism and Sexual Equality* (1984), Eisenstein concluded that the more American liberal feminists radicalize their agenda in response to an unresponsive state -- which has sought, at least since 1980, to curtail the most fundamental political demands brought by the mainstream women's movement (e.g., the ERA and abortion rights) -- the more the tension between women as a sex-class and women as citizens would be exposed. Liberal feminists, in short, would be forced to recognize the limits of liberalism.

Other feminists have argued that the problem for liberal feminism lies in its appeal "to the bureaucratic apparatus of the state and of the corporate world to integrate women into the public sphere through programs seeking equal opportunity and affirmative action" (Ferguson 1984, 4). This reliance on the bureaucratic state to obtain feminist ends, critics have maintained, is antithetical to an anti-hierarchical "vision of individual and collective life" (Ferguson 1984, 5; Elshtain 1981; Ruddick 1989; Denhardt and Perkins 1976).

Many feminists are wary about enhancing the power of a state whose surveillance capacities, they have argued, are cause for alarm. According to Carole Pateman, liberal feminism comes into an uneasy alliance with the state when it relies on the idea of the contract to promote social equality for women. In a time when "the influence of contract doctrine is extending into the last, most intimate nooks and crannies of social life," Pateman has written, feminists have every reason to worry about "the conjuncture of the rhetoric of individual rights and a vast increase of state power" (1988, 232). Pateman rejects the notion that the liberal discourse of rights alone can further feminist struggles for social, economic, and political equality. The putatively autonomous individual who has property in himself and who makes contracts, she has argued, has never applied fully to women. On the contrary, the proprietal self is a masculinist construction that has historically assumed and continues to assume men's property in women. For women, the freedom to contract has always been constrained by their sexual subordination to and economic dependence on men. Arguing that "a free social order cannot be a contractual order," Pateman has urged feminists to develop "other forms of free agreement through which women and men can constitute political relations" (1988, 232).

In contrast to the skepticism of those feminist political theorists who maintain that the women's movement cannot attain its goals of sexual equality through mainstream politics, many empirical feminist political scientists have suggested that women must enter the formal political arena in order to achieve equality. Thus, a significant body of empirical work conducted by women and politics scholars in the 1970s and 1980s focused on electoral politics. In particular, several of the first books to be published on women and politics focused on women public officials and party activists (e.g., Kirkpatrick 1974, 1976; Diamond 1977; Githens and Prestage 1977). Other early work focused on women's collective efforts to influence the policy-making process (e.g., Freeman 1975).

One of the primary tasks undertaken by the earliest research on women in political elites was that of challenging the near invisibility of women in the empirical canon of political science by demonstrating the presence of women among officeholders and activists. Thus, Jeane J. Kirkpatrick began the conclusion to her path-breaking study of 50 women state legislators serving in the early 1970s with the statement, "The most important finding of this study is that political woman exists" (1974, 217). Countering the image of women as apolitical, as by nature different from men, she then went on to assert that the political women she studied were similar in many respects to male politicians, in particular in their social backgrounds and psychological characteristics (1974, 220). However, to reassure those who at the time might have thought that any woman similar to a male politician could not be "normal," Kirkpatrick explained:

> ... *"political woman" is not grossly deviant from her female peers.* She is not necessarily "masculine" in appearance or manner; she has not necessarily rejected traditional female roles and interests. Quite the contrary. The political women on whom this book is based are... [almost all] wives and mothers.... Well-groomed, well-mannered, decorous in speech and action, these are "feminine" women in the traditional sex-stereotyped sense of that word (1974, 219).

When viewed from the perspective of the 1990s, Kirkpatrick's book, like other early empirical work on women and politics, appears cautious, conservative, and restricted in scope. However, when considered in its proper historical context, i.e., as a response to the invisibility and apolitical (or politically deficient) image of women within the existing behavioral political science research of the time, the importance and the (at least implicitly) challenging nature of *Political Woman* and other similar works are immediately apparent.

Women and politics scholars have attempted to make political women visible not only as individuals, but also as an organized, collective political force. Working

within traditional disciplinary frameworks, women and politics scholars have analyzed both the feminist and anti-feminist movements and their successes and failures in affecting public policy outcomes (e.g., Freeman 1975; Costain 1980, 1982; Gelb and Palley 1982; Gelb 1989; Boles 1979; Mansbridge 1986; Mathews and De Hart 1990; Klatch 1987).

Like Kirkpatrick and others who studied women in political elites, women and politics scholars who studied mass political behavior conducted research that countered the image of women as apolitical or politically deficient. While some researchers continued to find evidence of notable differences between women and men in political orientations and behavior in the U.S. and other countries (e.g., Jennings and Farah 1980; Rapoport 1982, 1985), most researchers in the 1970s and early 1980s found little evidence that women in the United States differed greatly from men in their political orientations or behavior. In part, the findings of few sex differences in these studies resulted from the elimination of much of the gender bias which had influenced both the methodology and the interpretation of findings in earlier research and which had led to a portrayal of women as more politically different from men than they undoubtedly were. However, in part, the lack of significant sex differences in most of the studies conducted in the 1970s and 1980s also reflected actual changes in political behavior that had taken place as differences between men's and women's social roles, education, and employment decreased.

Research conducted in the 1970s and 1980s demonstrated that women were as likely or almost as likely as men to participate in political activities such as working in campaigns, writing letters to public officials, attending political meetings, and contributing money to candidates or parties (Hansen, Franz, and Netemeyer-Mays 1976; Welch 1977; Baxter and Lansing 1980; Beckwith 1986). In addition, Sandra Baxter and Marjorie Lansing found women in all age groups to be as interested as men in following campaigns -- a finding that they noted "contrasts sharply with the myth of nonpolitical woman" (Baxter and Lansing 1980, 46). Research on childhood socialization conducted during the 1970s suggested that politically relevant differences between boys and girls also seemed to be few in number and limited primarily to political interest and knowledge (Orum et al. 1974; Sapiro 1983, 38).[1] Berenice A. Carroll, writing in 1979, summed up the feminist empirical research on women in American politics as follows:

> ...the picture which emerges is...one...of women holding political attitudes and engaging in political behaviors very similar to those of men, at all levels from school children to party activists and local officeholders. On almost all measures of voting, participation, efficacy, activism, ideology, and performance, sex differences between men and women, if present at all, are small (1979, 292).

In contrast to both the philosophical canon of political theory and the empirical canon of behavioral political science, which viewed differences between women and men as natural, women and politics scholars writing in the 1970s and 1980s generally attributed those sex differences which did occur to gender role socialization and/or adult gender roles. Differences between women and men, whether in elites or mass publics, were viewed as socially constructed and thus changeable rather than as natural and immutable. As the socialization of women and the opportunities and roles that were open to them in society changed, so too would their political behavior.

In this vein, studies of women's political orientations and participation examined the effects of education, finding increased education to be related to increased participation for women as well as to higher levels of political interest and efficacy (e.g., Sapiro 1983; Hansen, Franz, and Netemeyer-Mays 1976; Welch 1977; Baxter and Lansing 1983; Poole and Zeigler 1985). Working outside the home also was found to be related to women's involvement in conventional forms of participation, with employed women participating at rates similar to those of men, and women who were full-time homemakers participating at notably lower rates (Andersen 1975; Welch 1977, 724-5).[2] Finally, studies suggested that having children had an adverse effect on both political participation and various political orientations (Sapiro 1983, 177; Jennings and Niemi 1981, 296-7). The implication of these findings was that women's levels of participation would be likely to increase further as women increased their educational attainment, entered the labor force in larger numbers, spent a smaller proportion of their lives raising children, and received more help with child care. Thus, the few existing sex differences in political orientations and participation might well diminish as changes took place in women's roles and their socialization into those roles.

At the elite level, women and politics scholars devoted considerable attention to examining the question of why so few women held public office, and here, too, early feminist explanations often focused on gender role socialization and adult gender roles. Several studies focused on delegates to national party conventions and other party activists, who were considered a potential pool of future candidates for public office. These studies consistently found that women were less ambitious for public officeholding than their male counterparts

(Jennings and Thomas 1968; Costantini and Craik 1977; Kirkpatrick 1976; Fowlkes, Perkins, and Rinehart 1979; Sapiro and Farah 1980; Jennings and Farah 1981; Costantini and Bell 1984). Women's lower levels of political ambition were generally explained in terms of gender differences in political socialization and gender roles although the explanations were sometimes linked to lack of political opportunities. M. Kent Jennings and Barbara G. Farah, for example, observed, "For the present...cultural norms and structural conditions -- especially the dual demands of homemaking and career plus inequities in the opportunity structure -- continue to exert a dampening influence on the political life expectancies of women" (1981, 480).

The emphasis on gender role socialization and the effects of gender roles in explaining why few women held public office was prevalent in literature examining other politically active women as well. In a frequently cited study, Marcia Manning Lee examined the factors that kept women who were active in their communities from running for office. In addition to fear of sex discrimination, the two factors which held women back were their responsibility for care of young children (a factor that held few men back) and their perceptions of appropriate and inappropriate roles for women; most viewed holding public office as improper behavior for a woman (Lee 1976).

Research on women officeholders also emphasized the constraining effect of gender roles and gender role socialization (Diamond 1977; Stoper 1977; Mandel 1981). Kirkpatrick, for example, noted that traditional sex role requirements were the principal constraints preventing women from running for office. She observed that the women legislators in her study were remarkable precisely because they had managed to blend their political lives with traditional roles. She asked:

> If these women can do it, why can't/don't others? Perhaps because they lack high self-esteem and broad identifications, habits of participation, a desire to influence public policy, political skills needed to do so, a husband willing to cooperate, the empathy, flexibility, self-knowledge, and energy needed to live a busy and complicated life when so many less demanding alternatives are so readily available (1974, 240).

As this passage from Kirkpatrick's *Political Woman* illustrates, much of the literature stressing gender role socialization and gender roles implied that individual women themselves had to change their lives in order for women's representation in public office to increase. Observing this tendency in the literature, Berenice Carroll wrote in 1979, "There is still a heavy focus on sex-role socialization, with its implicit tendency to 'blame the victim' (or her mother) and to place on women the burden of changing sex roles without changing the system which requires the existing sex-role patterns, rewards those who conform to it, and punishes those who defy it" (1979, 306). Whether in response to this observation by Carroll and others or for other reasons, a change in emphasis occurred in the 1980s within the literature on political elites, especially that material dealing with the question of why women were underrepresented numerically among public officeholders. Many researchers turned their focus away from examining how gender role socialization and adult gender roles kept women from being more like the "political men" who held public office. Instead, they focused squarely on the operation of the political system itself as an explanatory variable helping to account for women's underrepresentation (e.g., Carroll 1985; Darcy et al. 1987).

These researchers demonstrated that the staying power of incumbents and the lack of winnable open seats are major obstacles to women's electoral success in the United States and elsewhere (Andersen and Thorson 1984; Darcy et al. 1987; Carroll 1985; Studlar et al. 1988). Electoral arrangements also are very important (see Rule and Zimmerman 1992; see also Welch and Studlar 1990 for a particularly good review of the research on the impact of electoral arrangements on women's electability). At the state legislative level in the U.S., women run in a greater proportion of multimember than single-member districts, and women who run in multimember districts win at a higher rate than those who run in single-member districts (Darcy et al. 1987, 119; Carroll 1985, 110; Rule 1990). Moreover, when states change from multimember to single-member districts as several states have done during the past three decades, the proportion of women running and winning decreases compared to national trends (Darcy et al. 1987, 119-22). Although differences in electoral arrangements are probably less important at the municipal level, there is evidence that women fare slightly better when cities have at-large rather than district elections (Darcy et al. 1987, 117-8; MacManus and Bullock 1989; Welch and Karnig 1979).[3]

Comparative research has demonstrated that women's representation in national legislatures is greater in countries with proportional representation, especially those utilizing party lists, than in countries like the U.S., which elect representatives on the basis of plurality voting (Randall 1987, 140-2; Rule 1981; Norris 1985; Lovenduski 1986; Haavio-Mannila et al. 1985). The institution and implementation of quotas for women on the party list for election within some European political

parties has further enhanced women's representation in those countries (e.g., Dahlerup 1988, 297; Kolinsky 1991; Phillips 1991, 84-5).

Although this research on the effect of electoral arrangements on women's representation has been conducted utilizing the frameworks, approaches, and techniques of mainstream political science, it nevertheless has important normative implications for behavioral political scientists. Barbara Nelson was quoted earlier in this essay as suggesting, "all political subjects are gendered." Research on the effect of electoral arrangements provides a good example by showing that certain supposedly gender-neutral features of the way the political system operates systematically discriminate against women. Certainly, the existing electoral arrangements of the U.S. are not gender neutral, although the experience of some Western European countries with quota systems suggests that electoral systems can be altered to make them more so. Political analysis that deals with proposed reforms in electoral systems or procedures (e.g., term limits, campaign finance), or that uncritically accepts existing electoral arrangements without explicitly examining the gender implications of those reforms or arrangements, may well end up, however unintentionally, perpetuating women's exclusion from public officeholding.

Empirical studies of women's behavior at mass and elite levels have also posed challenges to what we study as political scientists and how we study it by calling attention to previously unexamined variables and by arguing that in some cases gender-specific models are necessary to better explain political behavior. Empirical studies of women and politics have devoted considerable attention to the ways that women's so-called private lives, especially their responsibilities and connections to partners and children, influence and constrain their behavior. While generally finding family-related factors to be more important for women than for men, some feminist scholars made another important discovery: men have families too! And those families seem to have a significant impact on men's as well as on women's behavior. For example, Diane Kincaid Blair and Ann R. Henry found that family problems are the major factor leading to retirement from office for men as well as women serving in state legislatures; previous research had attributed legislative turnover primarily to low salaries and had failed to investigate the possible importance of family-related variables (Blair and Henry 1981). Similarly, Sapiro found that conflicts between family commitments and public commitments, although resolved differently by the two sexes, were experienced at least as often by men as by women among partisan elites (Sapiro 1982). Thus, the empirical women and politics literature would suggest that the possible influence of so-called private life considerations be given more serious attention in future research on political behavior.

Although much of the research on women and politics has suggested that some variables have a more important influence on women's political behavior while others are more important for men, a few recent studies have taken this argument one step further. These studies have suggested that the *process* and *calculus* of political decision making differs for women and men; some factors are more critical to women's decisions and other factors are more critical to men's. An example of such work is a study conducted by Linda L. M. Bennett and Stephen Earl Bennett examining gender differences in political interest and the impact of apathy on voting behavior. They concluded:

> ...the process leading women to the voting booth differs from that of men. While men are motivated more out of interest and partisanship, age and SES are the prime determinants among women. The top political disposition discriminating among women is citizen duty. In short, while men went to the polls in 1984 mainly because they were interested, to the degree that political dispositions were involved in their calculus of voting, women went to the polls because they thought they ought to go (1989, 119).

There is evidence that women and men employ a different calculus not only in deciding whether to vote, but also in deciding how to vote. For example, Ethel Klein's analysis of 1980 election data indicated that the voting calculus of women and men was very different in that election, with women's rights issues influencing women's votes far more than they influenced men's (1984, 161-2). In a more recent study, Susan Welch and John Hibbing (1992) have demonstrated that economic considerations are less important in women's voting calculus than in men's; moreover, while men are more likely to vote on the basis of egocentric economic judgments, women more often utilize sociotropic economic considerations in making their voting decisions.

In a similar vein, Timothy Bledsoe and Mary Herring have argued that different factors affect the decisions of women and men serving on city councils regarding the pursuit of higher office. They have suggested, "If the situation of women in electoral politics is unique, the decision-making process for these women may be unique as well" (1990, 213). They found that the decisions of women were more likely than those of men to be influenced by the strength of their current political position and their self-perceptions of political vulnerability. However, women's decisions were not

influenced by ambition; in fact, ambitious women were less likely than those without ambition to seek office. In contrast, men's decisions were little affected by their current circumstances but strongly affected by political ambition. In fact, ambition seemed to be the only variable that really mattered for men (1990).

Perhaps Janneke Van der Ros (1987) has carried the argument about differences in women's and men's political behavior and decision making farther than anyone else. She has suggested that gender-specific explanatory models be used whenever relevant in behavioral studies, and she has taken the first steps toward developing such models.

Not all women and politics scholars agree on the desirability of creating gender-specific models of political behavior. Some would rather see the development of nongendered models that would encompass all of human behavior. Others would suggest that we should reject completely the idea of models that try to generalize across all or even half of humanity and move instead in the direction of greater historical, cultural, and situational specificity in our investigation of political behavior. Nevertheless, studies which suggest that we need gender-specific models of political behavior are important because they call into question the universality of our existing knowledge and reveal it to be rooted in men's experience. Although feminist scholars may not be in agreement over the desirability of universals, they most certainly are in agreement that current empirical knowledge does not adequately reflect women's experiences.

Rethinking Traditional Frameworks and Assumptions

In recent years many women and politics scholars and feminist theorists have produced work that more explicitly confronts and challenges the dominant frameworks and assumptions of the discipline. Often influenced by the interdisciplinary perspectives of women's studies, these scholars have raised new questions and introduced new frameworks into the study of gender and politics. Their work suggests that we need to rethink and reconceptualize various approaches, assumptions, and concepts that are central to the discipline.

Empirically oriented feminist political scientists have challenged conventional definitions of politics by adopting an approach to their research that puts the perspectives of women and women's experiences at the center of their analysis. As one example, in contrast to earlier research that documented the effect that women's private lives had on their public lives but nevertheless did not question the basic assumption underlying much of western political thought and practice that life is, and can be, divided into public and private spheres, some recent research has through its focus on women's experiences called into question this assumption of a split between public and private life. For example, examining women state legislators' decisions to run for office and their private life situations, Susan J. Carroll found no evidence of a split between public and private in these women's lives and suggested that we need a new conceptualization. Finding that women's personal life choices had affected public careers and that their public life choices had had an impact on their private life situations, Carroll concluded, "A dualistic conception of public and private as largely separate and mutually exclusive spheres of existence does not adequately portray the reality of these women's lives; rather, public and private in the lives of women officeholders seem to constitute a holistic system of interrelated social relations where any action taken or choice made has repercussions throughout the system" (1989, 63).

Although they have not focused as explicitly on the relationship between public and private, several other scholars have questioned dominant conceptions of politics by using women's perspectives as a point of departure for their analysis of politics. One example is Diane Fowlkes, who interviewed 27 white women activists with diverse political backgrounds. Fowlkes acknowledged that her purpose was "not to test theory but to present and explicate the political worlds of the diverse white women in this study," and that the generalizations that she drew from her interviews were "intended to explicate the various meanings that *these women* give to their political worlds and the various dynamics that shape their political actions" (1992, 27). As part of her study, Fowlkes allowed the women to speak in open-ended fashion about how they defined "the political." The ways in which these activists defined politics were both rich and varied. Some viewed the political as political scientists often do -- as working for candidates for public office, holding office, governing, or advocating issues. However, others viewed the political in ways that suggested a broader or an alternative conception of politics -- as linking the private and public spheres, as developing power to bring about change, and as bringing change through the lives they lead on a daily basis. While most of the women activists' conceptions touched in one way or another on the theme of power, Fowlkes noted that the themes of educating and consciousness-raising, perhaps less often considered to be part of politics by contemporary political scientists, also cut across their conceptions of politics (1992, 184-214). By allowing women to speak from their own perspectives in their own terms, Fowlkes's

work suggests that the discipline of political science must expand its conception of politics if it is to encompass the ways that the women she interviewed think about the political.

Like Fowlkes, Cynthia Enloe in *Bananas, Beaches, and Bases* also used women's perspectives as a point of departure for rethinking politics -- in this case international politics. Enloe noted that she "began this book thinking about Pocahontas and ended it mulling over the life of Carmen Miranda" (1990, xi). She readily admitted that:

> These women were not the sorts of international actors I had been taught to take seriously when trying to make sense of world affairs. But the more I thought about Pocahontas and Carmen Miranda, the more I began to suspect that I had been missing an entire dimension of international politics -- I got an inkling of how relations between governments depend not only on capital and weaponry, but also on the control of women as symbols, consumers, workers and emotional comforters (1989, xi).

In some ways Cynthia Enloe's work does for international relations what the work of Jeane Kirkpatrick and others helped to do for American politics -- it makes women visible. However, Enloe's work differs significantly from that of Kirkpatrick and others discussed earlier in this essay in that Enloe has not adopted the dominant assumptions and frameworks of work in political science; rather, she has set them aside and used instead the perspective of women's experiences, specifically the experiences of Pocahontas and Carmen Miranda, as her point of departure for examining international politics. She has examined some topics (e.g., nationalism, diplomacy, militaries, and international debt) that are familiar to those who study international politics. However, all have been analyzed through the lens of gender and from the perspective of women's experiences with them. Enloe has concluded:

> Conventional analyses stop short of investigating an entire area of international relations, an area that women have pioneered in exploring: how states depend on particular constructions of the domestic and private spheres. If we take seriously the politics of domestic servants or the politics of marketing fashions and global corporate logos, we discover that international politics is more complicated than non-feminist analysts would have us believe. We especially have to take culture -- including commercialized culture -- far more seriously (1989, 197).

Like Carroll, Fowlkes, and Enloe, scholars who have taken women's experiences as a starting point for their analyses but adopted a more historical approach have suggested that we need to rethink and reconceptualize the way we view politics. Much of what women have done historically has been viewed as "philanthropy" or "service" or "disorderly conduct," but it has rarely been seen as politics (Lebsock 1990, 35). In large part, this has been the result of the public/private split; men's activities have been viewed as public activities while women's have been seen as extensions of their domestic roles. Mary Beth Norton has described the transition that took place in the field of women's history, where scholars initially accepted a "male definition of politics," one clearly linked to the public/private split. Historians who focused on women's experiences quickly discovered that this definition was too narrow to encompass women's political activities. As Norton has explained, "Drawing on the feminist movement's insight that 'the personal is political,' women's historians broadened the category to include women's attempts to gain control over their own lives -- both inside and outside marriage -- and to have an impact on the society in which they lived" (1986, 40). Consistent with this redefinition of the "political," feminist historians focused on women's participation in voluntary associations, the temperance movement, the women's club movement, the social settlement movement, and the trade union movement among other activities (Norton 1986; Lebsock 1990). Political scientists might well look to the experience of their colleagues in history for guidance in constructing a more gender-inclusive conception of politics.

Some empirical work has moved in a direction similar to the direction suggested by women's history. One example is *Women and the Politics of Empowerment* (1988), edited by Ann Bookman and Sandra Morgen. This volume includes several case studies of the activism of working-class women. It examines the activities of women who are household workers, social service agency workers, hospital union workers, and clerical workers in an insurance office. Included in the volume are studies of women who organized a clerical union, fought to reopen a prenatal and gynecology clinic, organized to obtain better quality education for their children, and led a campaign to obtain improved neighborhood services. While many of these activities might well be considered outside the domain of politics as traditionally defined, the editors have argued that all are politically relevant. As they have explained:

> The articles on community organizing document the political meaning and breadth of women's efforts to transform urban space and public policy. In contrast to the popular

> view that sees these activities as "voluntary associations" or "mutual aid societies," these cases show women challenging the power of the state and the interests of landlords, developers, and other private institutions. These are certainly political activities (1988, 9-10).

In addition to challenging the ways in which political scientists have traditionally thought about politics, recent empirical work on women and politics has begun to use gender as a category of analysis for studying political structures and processes. This is a relatively new development because, as the previous sections of this essay illustrated, gender has generally been employed as a category of analysis only in studying the behavior of individuals. Perhaps the best example of the application of a gendered analysis to the study of political structures and processes is the recent work on the welfare state and welfare state formation (e.g., Gordon 1990; Sarvasy 1992; Nelson 1990; Diamond 1983). This research has demonstrated that the U.S. welfare state evolved in ways that were clearly gendered (Nelson 1990; Jenson 1990). For example, a two-channel welfare state developed in the U.S., one from the development of Mother's Aid, which was intended for women in the home who had lost their spouses, and the other from the development of Workmen's Compensation, which was intended for men who lost their pay because of work-related disabilities (Nelson 1990). This literature has also demonstrated that the welfare state reinforces inequities based on gender, race, and class at the same time that it provides material aid for those in need (Mink 1990). The welfare state is frequently portrayed as paternalistic and a means of exercising social control over women, but some have argued that it also constitutes a political resource which makes women more secure and less powerless than they otherwise would be (Piven 1990). Regardless, it is clear that the new feminist scholarship on the welfare state has challenged political scientists to think more seriously about the role that gender has played in its formation and operation.

Recent research on women's voting behavior and women in public office, while often employing the same behavioral methodologies that have traditionally characterized research on voting and officeholding, has nevertheless posed a new challenge to the discipline. While much of the earliest empirical research on women and politics worked to counter the image of women as different from men and to convince political scientists that women and men were in most respects politically similar, this new research challenges political scientists to think once again about gender difference. Unlike in its previous incarnation, in its new incarnation gender difference is generally viewed as an asset, not a deficiency; women are seen as bringing perspectives to politics which are currently lacking.

For example, scholars who have examined the so-called gender gap in voting behavior and public opinion have frequently attributed it to differences in women's and men's values and priorities (Conover 1988; Frankovic 1982; Shapiro and Mahajan 1986; Klein 1985; Norris 1985; Mueller 1988). Although they disagree about whether the gender gap is caused by women's greater pacifism, compassion for the needy, nurturance, feminism, commitment to the welfare state, or other qualities, feminist researchers have generally agreed that the gender gap is a manifestation of differing perspectives between women and men (Carroll 1988). Similarly, research on women in public office has found that women public officials give greater priority than men both to so-called women's issues and to issues related to women's traditional roles as care-givers in the family and in society more generally (Dodson and Carroll 1991; Carroll, Dodson, and Mandel 1991; Dodson 1991; Saint-Germain 1989; Thomas 1991; Thomas and Welch 1991). The authors of these studies have suggested that women's greater involvement with policies in such issue areas as reproductive rights, violence against women, child care, health care, children's welfare, and education reflects the different experiences and perspectives that women bring to office with them. Moreover, the addition of these different experiences and perspectives to the policy-making process is viewed as a positive development that will enhance the overall quality of representation. Debra L. Dodson and Susan J. Carroll, for example, have argued:

> ...significant change is taking place -- change that has important long-term implications. As more women enter legislatures, the policy agenda is being reshaped to better reflect the concerns brought into the legislature by women. The end result is likely to be an agenda that is more reponsive not only to the specific needs of women, but also to the needs of a broader cross-section of our society (including, for example, the economically disadvantaged, children and those who lack access to adequate health care) (1991, 94).

Empirically oriented political scientists who have examined the ways that women both as voters and as officeholders are bringing perspectives to politics that are currently underrepresented have been strongly influenced by the work of so-called "gender difference" theorists (e.g., Gilligan 1982; Chodorow 1978; Ruddick 1989) in women's studies. The work of Carol Gilligan on gender differences in moral reasoning has been especially important for many feminist political scientists. Taking

on the scholarship in moral theory from Sigmund Freud to Lawrence Kohlberg, Gilligan challenged the claim that women achieve an attenuated stage of moral development in which "goodness is equated with helping and pleasing others" (1982, 18), and that women have therefore a stunted sense of justice (i.e., the capacity to make impartial judgments according to a set of universal rules). Curiously enough, Gilligan observed, "the very traits that traditionally have defined the 'goodness' of women, their care for and sensitivity to the needs of others, are those that mark them as deficient in moral development" (1982, 18). She drew on the work of Nancy Chodorow (1978) to argue that gender differences in moral reasoning are rooted not in biology but rather in childhood development, specifically in the contrasting relationships that boys and girls have to their primary care-giver, the mother. The abstract rules that govern men's moral reasoning, Gilligan held, reflect the achievement of masculine identity through a radical separation from the mother and the maintenance of firm ego boundaries. The context-based principles that govern women's moral reasoning reflect the achievement of feminine identity through a far less radical form of separation from the mother and the maintenance of empathetic relations to others.

Insofar as the traditional moral standard of impartiality takes for granted the masculine values of separateness and autonomy, wrote Gilligan, "women's failure to separate them becomes by definition a failure to develop [morally]" (1982, 9). Gilligan refuted the notion that the "quality of embeddedness in social interaction and personal relationships that characterize women's lives" was a "developmental liability" (1982, 9). She took that quality as an occasion to advance an alternative moral theory, one that would give legitimacy to women's "different voice":

> When one begins with the study of women and derives developmental constructs from their lives, the outline of a moral conception different from that described by Freud, Piaget, and Kohlberg begins to emerge and informs a different description of development. In this conception, the moral problem arises from conflicting responsibilities rather than from competing rights and requires for its resolution a mode of thinking that is contextual and narrative rather than formal and abstract. This conception of morality as concerned with the activity of care centers moral development around the understanding of responsibility and relationships, just as the conception of morality as fairness ties moral development to the understanding of rights and rules (1982, 19).

The argument that women have a different sense of morality grounded in a sense of everyday life and the web of human relationships (Belenky et al. 1986; Miller 1976; Gilligan 1982) has had a profound influence on the work of many feminist theorists who are concerned with challenging a wide variety of abstract models in the social sciences. As Sara Ruddick has put it in *Maternal Thinking*: "Given the value that is placed on abstraction in academic life, concreteness can become a combative insistence on looking, talking, and asking troublesome questions" (1989, 95).

Ruddick has maintained that "abstraction is central to militarist thinking." Like several other feminists working in the area of peace studies, she has argued that realism, as it is articulated by defense intellectuals, is grounded in universal rules of human behavior which assume that "people and nations will, if they can, dominate and exploit those who are weaker" (Ruddick 1989, 179; Elshtain 1987; Cohn 1987; Enloe 1989). Similarly, Jean Bethke Elshtain has written that the realist model of "professionalized IR discourse" focuses exclusively on the abstract entities called states and ignores the most material facts of human life: "No children are ever born, and nobody ever dies, in this constructed world. There are states, and they are what is" (1987, 91).

Both Elshtain and Ruddick have argued that women's experience, and especially maternal experience, provides the basis for political resistance to authoritarian regimes and for peace activism in democratic countries. Each has insisted that women's role in the maintenance of life offers a vital model of civic participation which treasures "the connectedness of self and other" (Ruddick 1989, 225). For example, the famous resistance of the Madres of Argentina challenges us to rethink conventional notions of peace politics and, not least, of femininity. Ruddick has observed:

> In their protests, these women fulfill traditional expectations of femininity and at the same time violate them....Women who bring to the public plazas of a police state pictures of their loved ones, like women who put pillowcases, toys, and other artifacts of attachment against the barbed wire fences of missile bases, translate the symbols of mothering into political speech. Preservative love, singularity in connection, the promise of birth and the resilience of hope, the implacable treasure of vulnerable bodily being – these cliches of maternal work are enacted in public....They speak a 'woman's language' of loyalty, love, and outrage; but they speak with a public anger in a public place in ways they were never meant to do (1989, 229).

Clearly not all feminists espouse the model of "maternal thinking" or social feminism advanced by Ruddick and Elshtain, and some are intensely critical of it. The lively feminist debate over what is called "gender difference" turns in large part on the skepticism some critics have toward any effort to revalue women's traditional role in the family. Mary Dietz has argued that maternal thinking not only overvalues motherhood and the intimate relations of kin, but also devalues the relations among total strangers or mere acquaintances which are the very basis of large representative democracies. The good mother does not necessarily make the good citizen, and "the bond among citizens is not like the love between a mother and child" (1985, 31; Ferguson 1984). As "citizens are not intimately, but politically involved with each other," writes Dietz, "we look in the wrong place for a model of democratic citizenship if we look to the family" (1985, 31).

Perhaps the most difficult of balancing acts for feminist scholars is how to account for and give value to women's experience without reproducing socially ascribed gender roles. Still, many scholars worry that, so long as gender differences carry social significance, feminists downplay those differences at their peril. Feminist scholarship has clearly shown how the notion that "we are all just people" translates in political science discourse, not to mention in social practices, into "we are all just men." As Susan Moller Okin has argued in her most recent work, *Justice, Gender, and the Family*, gender-neutral language in contemporary political theory effaces the material reality of sexual inequality and reproduces "theories of justice [which], like those of the past, are about men with wives at home" (1989, 13). Like Ruddick, Gilligan, and Elshtain, Okin is critical of theories of justice that offer "some abstract 'view from nowhere'" (1989, 15). Like Dietz, however, she cannot accept the argument that "'justice' and 'rights' are masculinist ways of thinking about morality that feminists should eschew or radically revise, advocating a morality of care" (1989, 15). For one thing, the evidence that women have a different way of moral reasoning is debatable at best; for another, feminists who make such a claim, all disclaimers notwithstanding, risk playing into the familiar "stereotypes that justify separate spheres" (Okin 1989, 15).

The legal theorist Martha Minnow has captured the problem we have been describing as follows: "Both focusing on and ignoring difference risk recreating it. This is the dilemma of difference" (1984, 160). Negotiating the "difference dilemma" has been a particularly delicate matter in the area of feminist legal studies (Eisenstein 1988; MacKinnon 1987; Bower 1991). In an essay on the sex-discrimination suit brought by the Equal Employment Opportunity Council in 1978 against the Sears, Roebuck & Company, the historian Joan Scott usefully summarized the "equality-versus-difference" conundrum as follows:

> When difference and equality are paired dichotomously, they structure an impossible choice. If one opts for equality, one is forced to accept the notion that difference is antithetical to it. If one opts for difference, one admits that equality is unattainable.... How then do we recognize and use notions of sexual difference and yet make arguments for equality? The only response is a double one: the unmasking of the power relationship constructed by posing equality as the antithesis of difference, and the refusal of its consequent dichotomous construction of political choices (1988, 172).

This is a difficult and tall order for feminist scholars. But Scott has insisted that to avoid treating masculinity and femininity as if they were unchanging essences, feminists must ask: "How are differences being constructed" in specific social, historical, and discursive contexts? (1988, 173).

Scott's suggestion that a "power relationship" is concealed in the difference/equality opposition has been forcefully argued by the feminist legal theorist Catherine MacKinnon. In *Feminism Unmodified*, MacKinnon has called for "a shift in perspective from gender as difference to gender as dominance" (1987, 44). What is taken as women's difference from men, she has maintained, is really the difference created through women's social, political, and economic subordination to men. Sexual difference is at bottom an unequal power relationship between women and men, one whose feminist dismantling is not a matter of asserting women's moral superiority; it is rather a matter of politics. Like Scott, MacKinnon has insisted that feminists need to challenge the idea that sexual difference is *the* fundamental social difference, and that they need to refuse the choice between difference and sameness:

> To define the reality of sex as difference and the warrant of equality as sameness is wrong on both counts. Sex, in nature, is not a bipolarity; it is a continuum. In society it is made into a bipolarity. Once this is done, to require that one be the same as those who set the standard -- those which one is already socially defined as different from -- simply means that sex equality is conceptually designed never to be achieved. Those who most need equal treatment will be the least similar [e.g., poor women], socially, to

> those whose situation sets the standard as against which one's entitlement to be equally treated is measured (1988, 44).

So-called "sex equality," according to MacKinnon, is an oxymoron. To be sexed is to be a woman (or rather a wo-man) and to be a woman is to be unequal. As long as sexual difference remains unquestioned, social equality for women will remain impossible.

The only way out of the difference dilemma, these critics have argued, is to refuse sexual difference (man versus woman) and to affirm instead what Zillah Eisenstein has termed "the plurality of differences among women" (1988, 223). In *The Female Body and the Law*, she has called for "a radical pluralist and feminist theory of equality," one that would "recognize the specificity of the female body and the variety of ways this is expressed: individually (as in differences of health, age, body strength, and size) and in terms of a woman's race and economic class" (1988, 222). Eisenstein has taken up the most monolithic of all representations of the female body, the pregnant body, and has radically redeployed it throughout her book to undercut the sameness/difference opposition:

> A middle-class, black, pregnant woman's body is not one and the same as a working-class, white, pregnant woman's body. The pregnant body of a woman in her midthirties is not identical to the pregnant body of a woman in her early twenties. A welfare woman's pregnant body may not be the same as an upper-middle-class woman's pregnant body. [And so forth.] (1988, 222-223).

What Eisenstein has called for is a far more complicated approach to the very category of women and of sexual difference than that which has been advanced thus far in the scholarly literature on women and politics. She has asked feminists, in effect, to question the very terms that they use in their work because those terms tend to reproduce the very thing that feminists want to critique: sexual inequality. If one is a woman, surely that is not all that one is. And those other race and class identities (to name but two) shape how one experiences being a woman and -- this is crucial -- how one is seen by others as a woman. African-American women are not (and have not been) seen as "women" in the same way that white women are seen (hooks 1981; Spelman 1988). Working-class women are not (and have not been) viewed as "women" in the same way that domesticated middle-class women are viewed (Riley 1988; Zerilli 1993). We shall have more to say about the category "women" in our concluding section.

Another problem for feminist scholars who wish to advance a gender-sensitive approach to politics but are wary of monolithic claims about "women's difference" is how to theorize "women's experience." Nancy Hartsock has tackled this problem in her influential work on "feminist standpoint theory." Hartsock wanted to develop an epistemology and a theory of power that would take account of gender differences without universalizing them. She distinguished:

> ... between what Sara Ruddick has termed 'invariant or *nearly* unchangeable' features of human life, and those that, despite being '*nearly* universal' are 'certainly changeable'. Thus that women and not men *bear* children, is not (yet) a social choice, but that women and not men rear children...is clearly a social choice (1985, 223).

Hartsock read women's experience through the lens of Marxist historical materialism. She argued that it is not enough to talk about "women's standpoint," their sense of interconnectedness with others and with nature (*pace* Gilligan, Ruddick, or Elshtain); rather, one must examine the development of a "feminist standpoint" as it emerges in the context of political activism, that is to say, its "achieved character" and "liberatory potential" (Hartsock 1983, 232).

Hartsock's approach has the advantage of thinking through not only how women's experiences shape their political consciousness but also how their political consciousness shapes their experiences as women. It thus broaches the question of how feminine identity is socially constructed and radically challenged through political activism. Rather than treat "women's experience" as if it were a stable and unified category, she has called for a political analysis of how the experience of "being a woman" is given meaning in and through the political practices of feminism.

Some Thoughts on Challenges for the 1990s

We have argued that a persistent problem for feminist scholars has been how to develop alternative theoretical and empirical models that would take into account women's experiences and perspectives, but that would avoid both reproducing socially ascribed gender differences and effacing the social diversity among women. For these reasons, the category of women has come under question in the work of some feminist scholars.

Yet feminists have good reason to worry about throwing into question the very category that has enabled both their collective resistance to oppression and their scholarship. Giving voice to women "as women" in

social life and political practice has been absolutely crucial for contesting women as invisible, apolitical, or simply as the (deficient) other from men. As Elizabeth Spelman has written, "Feminists have long been aware of the levels at which male privilege operates to erase women's lives and perspectives from view" (1988, 4). "Nevertheless," she has added, "our distancing ourselves from the views of blatant sexists keeps us from recognizing the extent to which we may in fact share elements of their views" (1988, 5). That is to say, feminists also often have a blind spot in their research -- one that ignores other categories of social difference (e.g., class, race, and sexual orientation) by working with the category of women *as if* it encompassed the experiences of all women. "This," Spelman has said, "leads us to the paradox at the heart of feminism":

> Any attempt to talk about all women in terms of something we have in common undermines attempts to talk about the differences among us, and vice versa. Is it possible to give the things women have in common their full significance without thereby implying that the differences among us are less important? How can we describe those things that differentiate women without eclipsing what we share in common? (1988, 3).

In our view, Spelman formulates precisely the key dilemma confronting feminist political science today. While enormous strides have been made by feminist scholars, few would doubt that there is still much work to be done. Can that work be accomplished if we treat the differences among women as if they were as significant as what women have in common? How can one make women visible, not to mention challenge the frameworks that render them invisible, if one overly complicates the term "women" that makes the feminist project possible?

There are, needless to say, no easy answers to these questions despite the fact that they are, in our view, some of the most important questions confronting feminist political scientists, and indeed feminist scholars across all disciplines, in the early 1990s. Nevertheless, in this concluding section, we would like to suggest why feminist scholars ought to keep asking these questions, to offer some possible ways of beginning to think through them, and to indicate why we think these questions are important to the discipline.

The first reason why feminists do well to interrogate the category (women) that seems at first glance to be the theoretical foundation of their own research concerns the matter of what is meant by "what women have in common as women." In her book, *Feminist Theory: From Margin to Center*, bell hooks has pondered this very phrase as follows:

> A central tenet of modern feminist thought has been the assertion that 'all women are oppressed.' This assertion implies that women share a common lot, that factors like class, race, religion, sexual preference, etc. do not create a diversity of experience that determines the extent to which sexism will be an oppressive force in the lives of individual women. Sexism as a system of domination is institutionalized but it has never determined in an absolute way the fate of all women in this society (1984, 5).

This "ideology of 'common oppression'" (1984, 8), hooks has argued, is based on a "one-dimensional perspective on women's reality" -- a white, heterosexual, middle-class woman's perspective (1984, 3). When feminists invoke phrases like women's oppression, women's experience, and women's difference, they "conflate the condition of one group of women with the condition of all women and...treat the differences of white middle-class women as if they were not differences" (Spelman 1988, 3). The term "women," in other words, effaces the diversity of its purported social referent; its true referent is a far more partial social reality.

As we argue in more detail below, even when feminists try to take account of women's diversity by factoring into their analyses such variables as race and class, they do not tackle properly the problem of the category of women that we have been describing. Just as feminists have shown that one cannot treat gender as a mere variable, neither can one treat race and class as mere variables. Just as feminists have shown that to make gender an analytic category of their research is to rethink traditional political science frameworks, so do critics like Spelman (1988), hooks (1984), Evelyn Brooks Higginbotham (1992), Patricia Hill Collins (1989), and Julianne Malveaux (1990) insist that when race and class are treated as analytic categories of feminist theory, then existing feminist frameworks themselves must be rethought.

For some feminist political theorists, this rethinking has entailed the development of new strategies for interrogating historical and contemporary inscriptions of sexual difference which examine the lived relation of social subjects to socio-symbolic configurations of masculinity and femininity (Brown 1988; Lorraine 1990; Di Stefano 1991; Zerilli 1991; Zerilli 1993). As Christine Di Stefano has written recently, "gender must be approached as simultaneously 'real' and 'false'; that is, as a set of representations that (in conjunction and tension with other representations [e.g., of race and class]) creates a world of fixed, yet also unstable, meanings, relations, and identities, which simultaneously produce and do violence to specific subjects in specific ways"

(1991, xiv). The challenge for feminists, she argues, is to disrupt cultural representations of an incommensurable sexual difference which organize "the world 'as if' women stood in a derivative (but also opposed) relation to 'man'" (1991, xiv), and, we would add, "as if" that relation were not only immutable but also more socially significant than the relations of race and class.

The instability of the category "women" and of naturalized sexual difference, as demonstrated in the works of feminist theorists, has important implications not only for political theorists, but also for more empirically oriented political scientists. Although these implications will become clearer as the debate over the category "women" continues to play itself out over the next several years, we can put forth a few preliminary observations from the vantage point of the early 1990s.

The work of feminist theorists suggests that researchers in the 1990s should be more cautious and self-reflective about their use of the category "women" than they have been in the past. Most empirical work focusing on women or gender (or even including sex as a control variable) has assumed that the category "women" has an inherent political meaning, that there is something politically relevant, most commonly "interests" (see Sapiro 1981 for a discussion of issues involved in defining women's interests), that all women share. Most empirical researchers believe that by controlling statistically for variables such as race, age, income, and education, they can isolate and measure the effects due to gender. However, applying statistical controls to isolate the effects of gender is not sufficient to deal with the problem of the category "women" as it is posed by feminist theorists. Rather, feminists who have interrogated the category "women" question whether such gender effects, free from the influence of other confounding variables, exist at all -- whether there is any commonality, any essence, or even any interests that all women share after variations in their race, age, education, income, and the like are taken into consideration. Instead, they suggest that race, class, and gender, for example, are so intertwined that all work together to shape identity (or, in this case, political identity). Women exist in an historical and cultural context, and to erase or ignore this context is to gloss over important differences among women.

What does this actually mean for those who do empirical research? First, it means that we should be less concerned with comparing women with men and more concerned with examining how different subgroups of women in different contexts behave politically. If we take the critiques of the category "women" seriously, our research is likely to become more contextual and more historical. It will make visible those women (e.g., women of color, poor women) whose experiences are often erased in the pursuit of scientific generalizations that purportedly apply to all women. Taking seriously critiques of the category "women" will likely move us in the direction of reducing our knowledge claims and our pursuit of universal truths. It will make us less likely to generalize across women (or men) of differing classes, races, cultures, nationalities, ethnicities, or generations. However, what we sacrifice in our ability to generalize (which those who theorize about the category of women would suggest has led only to false or misleading generalizations anyway) will be more than made up for in the depth and richness of our analysis and our understanding.

While empirical researchers who are sensitive to critiques of the category "women" may well become more reflective and self-conscious in their use of the term and more contextual and culturally specific in their approach to research, there are, as we noted earlier, good reasons for researchers not to abandon fully the category that has enabled their work. By using the category "women," feminist political scientists have been able to call into question some of the central assumptions and frameworks of the discipline. The concerted focus by feminists on women and women's experience has helped us as a discipline to see the biases and the blinders that characterized pre-feminist work and to improve our knowledge base by correcting for these biases and removing the blinders. In the same way, the current feminist interrogation of the category "women" may lead to research that will expand and improve our disciplinary knowledge base through greater historical and cultural specificity and more attention to heretofore neglected segments of the population. A major task for women and politics scholars in the 1990s will be to work through the many questions surrounding the category "women," perhaps finding some middle ground between uncritical acceptance and total abandonment of the category. In doing so, feminist scholars are likely to continue to pose new and important challenges to the discipline of political science.

Notes

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1. More recent research has suggested that we should not be so quick to discount the impact of childhood socialization in producing politically relevant differences between girls and boys. (See for example, Bennett and Bennett 1989; Owen and Dennis 1988.)

2. More recent research suggests the relationship between working outside the home and political participation may be less straightforward than once thought. For example, McDonagh (1982) has argued that social status variables are more important than employment per se, and Andersen and Cook (1985) failed to find a short-term impact from entry into the paid work force although their findings pointed to the possibility of a long-term impact.

3. In contrast to the results for women, multimember districts and at-large voting systems have been found in many cases to disadvantage African Americans and Latinos. See Persons (1991) and Fraga (1991) for reviews of this research. Unfortunately, most of the research on women and minorities has not examined the effects of electoral systems on women of color. Notable exceptions are: Karnig and Welch (1979) who found that African-American women do about as well in at-large as in district municipal elections, indicating that the findings for women (dominated by whites) and minorities (dominated by men) cannot be assumed to be true for minority women; Welch and Herrick (1992) who found that African-American, Latino, and white women all fare slightly better with at-large municipal electoral systems, although other factors are more important than electoral structures in explaining the representation of all three groups of women; and Rule (1992) who found that multimember state legislative districts benefit both African-American and white women.

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4

Formal Rational Choice Theory: A Cumulative Science of Politics

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Formal political theory is commonly described as the analysis of rational choices and their aggregate consequences in non-market contexts. It resembles economic analysis which is concerned with rational behaviors in market contexts. In other words, the two fields share the same set of assumptions concerning individual choice. Hence they overlap methodologically and differ primarily in the contexts in which the choices are made. Whether the individual is considered as citizen or consumer, he is viewed as consistent in his behaviors. *Homo politicus* has rejoined *homo economicus*.

Early contributions to the area were made by researchers trained outside political science.[1] William Riker (1958, 1962) was one of the first in our discipline to recognize the importance of the approach for the understanding of politics. Increasing numbers of political scientists followed his lead. For more than a quarter century, the body of formal results and their empirical tests have grown steadily. A number of Nobel prizes awarded for work in the field have brought publicity and encouragement.[2] By now, the field of formal theory (a.k.a. positive theory, public choice, and collective choice) has grown into a major area of research in our discipline. Leading journals such as the *American Political Science Review*, the *American Journal of Political Science*, and the *Journal of Conflict Resolution* devote a substantial proportion of their pages to formal analyses of political processes.[3] The number of textbooks available at the graduate level has increased substantially over the last decade; many deal specifically with political applications.[4]

We recognize that a record of growth and success is not a sufficient recommendation; it certainly does not substitute for careful evaluation. In this essay, we try to describe in what ways formal theory has influenced our thinking about politics.

Our presentation is intended as an introduction for the sophisticated student of political science who may be untrained in deductive methods but who is nonetheless willing to handle abstractions. Our goal is to give the unfamiliar reader a set of simple yet firm intuitions about how formal theories advance our understanding of politics. We do not attempt to cover the field in its entirety. Social choice theory, for example, is not discussed explicitly. Rather, in the interest of portraying the methodological and epistemological factors which have contributed to the field's success, we have chosen to construct this essay around three major substantive areas: voting, collective action, and coalitional stability. Each is discussed separately, even though much of the analysis is in fact interrelated. This is not to say that the discussion is limited to the three areas. We also touch upon the uses of the rational choice approach in other areas such as institutional design, public policy analysis, and political philosophy.

The Distinctive Nature of Formal Theory as a Field

As compared to many fields of specialization within political science, formal theory is unusual. Formal theorists work neither on a particular set of institutions nor on a substantively defined set of political problems. Instead, they often purport to know something specific about what is going on in each of the widely ranging institutional contexts throughout the discipline.

A second distinctive feature of rational choice research is that it is often mathematical. What is more, over the decades much of the mathematics has been refined in its content and reified in its presentation. Today, even for the mathematician, a great deal of technical skill may be needed to comprehend papers in the field. And even with the mathematician's technical advantages, a great deal of reflection is required to comprehend the "political" content of the formulations and their solutions. Unfortunately, these are barriers to entry to *anyone* wishing to appreciate what has been produced in the field.

To understand why the field has these distinctive features, we begin by examining the epistemological goals of the practitioners and the general structure of the arguments they put forward.

The Goals of the Practitioners

The Reasons for a Formal Theory

The concept of formal theory in political science refers to a deductive method of theory construction. The deductive method, common throughout the sciences, guarantees that theories are consistent. Consistency, however, is only a prerequisite for the cumulative growth of knowledge; it does not, in itself, guarantee progress. Progress requires that our theories are in some way descriptive of real world processes. Thus theoretical constructions should lead to testable hypotheses. In short, then, formal theorists endeavor to develop testable, deductive theories of political behavior and institutions.

Formal theory is thus defined more by the *method* of the theory construction than by the *content* of its theories. This important point is taken for granted by the practitioners, but it is often missed by critics. As a method, deduction can be used to construct theories based on *any* set of assumptions, as long as this set does not include inconsistent statements. Deduction, when applied correctly, guarantees the logical validity of all of the derived propositions and, hence, the consistency of the entire theory. Formal theories are consistent in just this fashion. In most sciences the name "theory" is reserved for such constructs.

Necessary Truths or Testable Conjectures?

In any theory, some propositions are fundamental to the system, others are derivative. The fundamental propositions which form the premises of theories are referred to as assumptions or axioms. For the purposes of our discussion it is useful to distinguish between these two concepts. As in mathematics and normative theory, axioms are initial propositions assumed to be truths not correctable by the world of facts. Assumptions we will understand here as empirical conjectures. As such, premises are, in principle, testable. From these assumptions, other propositions are deduced. Such a description is open to misunderstanding, because the notions of "logically" and "derived" are often used neither carefully, nor consistently.

The use of logic, the set of rules which preserve the truth of an argument, guarantees that an argument is consistent. An argument can be thought of as a set of statements or propositions. Consistency among a set of statements is the property that all the statements can be true simultaneously. That is, there exists some logically possible state of the world in which all the statements are true. An argument built with inconsistent statements can not possibly describe *any* state of the world. Such arguments are *necessarily* false. Hence consistency is an essential property of proper argument.

The relationship of logical derivation, or deduction, in argument is a relationship between two sets of statements, such as between premises and conclusions. A deductively valid argument requires that if the premises are true, then the conclusion *must* be true. If the premises are indeed true, then the statement of the deductive relationship between the premises and the conclusions *is* tautological. This is not a problem, however, if the premises are treated as conjectures.[5] In this case, the whole set of statements derived from the assumptions -- the entire theory -- constitutes a set of empirical conjectures. Thus, many of the criticisms of the work in formal theory as tautological are to some extent misplaced.

Normative or Descriptive Theories?

A further ambiguity fundamentally affects one's view of a theory's testability. Many theories can be perceived either as normative or descriptive. These perspectives imply different procedures when theoretical predictions do not conform well with the facts. In short, a normative theory addresses how an individual *should* behave (say to maximize profit), while a descriptive theory addresses how one *does* behave. Intuitively, only the latter, the "does," is testable.

For those who view a theory of rational action as normative, the fundamental propositions of the theory are indeed axiomatic. Testing the theory is irrelevant to their concerns. Facts would then only be important to assess how people deviate from what they should be doing. For those who view the same theory as descriptive, observation and experimentation play a fundamental role in theory development through the assessment of the theory's explanatory value. Because some researchers have treated some of the research as normative (e.g., Von Neumann and Morgenstern's expected utility theory), the theories were at times insulated from empirical tests. But we do not believe this is the mainstream use of formal theory in political science.

When a theory is treated as empirically testable, deductive methods of theory construction yield a great advantage. To the extent that the assumptions are true, all propositions derived from them are *detachable* from these assumptions and presumptively true.[6] This implies that each of the derived conclusions can be used to test the truth of any set of (possibly true) premises which implies them. Hence, the many testable conclusions

derived in the theory are indirect tests of the assumptions. This is of fundamental importance when assumptions are difficult to test directly, as is often the case when modeling political phenomena with theories of choice. Those theorists interested in the truth value of the premises used in their arguments will be interested in generating and testing new conclusions from their premises. And they are -- regardless of the substantive field in which the inferences lie. Hence the intrusive quality of the field.

The Use of Mathematics

This discussion of the nature of theory, and its ties to logic can explain the general use of mathematics in the field. Because mathematics contains within it all the assumptions and techniques of logic, anything which has been shown to follow mathematically has been shown to follow logically.[7]

Mathematics or symbolic notation is by no means necessary to deductive theories. The theories of Downs (1957), Schelling (1963) and Coase (1960), for example, were developed practically without symbols and mathematics. But when performed in natural language, proper deduction is very difficult and full of traps. Definitions of terms can slip and improper inferences can be drawn. Symbolic notation and formalization guard against such problems.

While symbolic or mathematical arguments are often thought of as difficult and complex, we should recall that the very reason for formal modeling is precisely the opposite. The language of symbols and the use of mathematical tools are used because they provide the simplest form of precise description and the easiest method for carrying out complex deductive arguments. Thus the very point of formalization is to clarify, not to obfuscate.

The Role of Experimentation

Theoretical claims made about reality are assessed according to their correspondence with the facts and their coherence with our understanding of the facts.[8] For any correspondence to be replicable, the nature of the observations must be made with considerable care to reflect the details specified in the theoretical argument. In the physical sciences this led to the development of an experimental tradition. This tradition has also taken root in formal theory, and the results from many of these experiments are reported in the current issues of our leading journals.[9]

The core aspirations of the field, then, are logically inferred, theoretical arguments, tested in a particular research paradigm. That paradigm is characterized by a consensus regarding what counts as theory, and a rather intricate, often formalized and mathematical argumentation, to generate testable conjectures of substantive and theoretical interest. And the testing of the conjectures tends necessarily to mirror the intricate and careful nature of the theory, to insure against improperly accepting or rejecting the work.

The reader can now infer from the stipulated goals a number of the characteristics of formal theory. For example, the use of symbolic notation and mathematical methods, emphasis on testing through experimentation, the use of a small set of general assumptions in theory construction are obvious elements. But what of its substance? Since many of the behavioral assumptions used in formal theories come from a different discipline -- economics -- the reader may find it useful to get an overview of some of the specific content of the premises at the foundations of the theory before considering the areas in which they have spawned useful or insightful applications.

Foundational Premises

Rationality

The field's major premise is that politics is inextricably involved with the act of choosing. Theorists try to explain political phenomena by using the behavioral conjecture that actors (individuals, states, organizations, etc.) make purposive, goal-seeking choices based on their own preferences -- they are rational. It is assumed that an individual is able to rank alternatives from best to worst. Such a ranking will have the property of transitivity. Transitivity is the condition that if alternative a is better than alternative b and b is better than alternative c, then it is certain that a is better than c. Actors are thought to choose according to what is best for them given their *own* preferences or tastes.

This does not mean, though it is often misconstrued, that rationality precludes altruistic behavior. We can have a rational choice theory which helps explain the behavior of an actor who is exclusively concerned with helping others. Mother Teresa, considered as a symbol of perfect generosity, may well both calculate how best to accomplish her altruistic goals and behave rationally. Rationality, in itself, has nothing to say about whether the desires, or preferences, of an individual are benevolent or evil. Theories of rational action do not explain where preferences come from. In its most general form, rationality as a behavioral statement takes the goals of an individual as given and both describes how those goals would be attained efficiently and stipulates a consistency between preferences and actions. Individuals assess their available

options and choose those which they expect to best achieve their goals. The silence of the rationality axioms on tastes can be seen in many results that make no restrictions on individual preference orders.[10] Typically, an actor's preferences are represented in the theory as a *utility* function which registers not only the order of an individual's preferences but their intensity as well.

Preferences and Expected Value

Quite often a course of action (let us say inserting money into a vending machine and pressing the Coke button) is not sufficient to guarantee a desired outcome. It may lead to numerous outcomes (let us say the wrong product, or nothing at all). When a course of action leads to a set of possible outcomes, it can be portrayed as a gamble and expose the decision maker to the risk that something less than the desired outcome will eventuate. Obviously, any successful theory, or even a description of choice, must take into account not only the utilities of the possible outcomes but their probabilities as well. Thus, expected utility theory is a form of contingency analysis that explicitly evaluates a decision in terms of both the desirability and the likelihood of the scenarios a course of action may lead to. The worst-case scenario is analogous to the down-side risk. It is not so disturbing to the decision maker when it is neither too bad, nor too likely. If a decision maker with values for the outcomes can assign a probability to each outcome occurring, then the value of the entire lottery can be expressed as an *expected utility* by summing the value of each outcome weighted by its probability of occurring.[11] Various lotteries can then be compared in terms of their expected utilities in order to make the best choice -- the course of action expected to return the greatest welfare.

Self-Interest and Other Value Constraints

Further conjectures are often added to the utility maximizing assumption which narrow (at times considerably) the types of interests actors are presumed to hold. These may be very general conjectures, or rather narrow ones for special actors, within a particular model. Such specification allows us to conjecture actors' preferences (or utilities) over a set of possible outcomes. By far the most widely employed is an assumption of *self-interest*. Self-interest is generally understood to mean that individuals do not directly care about the welfare of others.

Like the rationality conjecture, self-interest certainly is fundamental in economics, and has stood the test of time well in market contexts as applied to profit maximization. Its verisimilitude in other contexts may be looser. But the theory of self-interested behavior is particularly suited for deductive simplicity. After all, it assumes that *i*'s preferences are not determined by *j*'s preferences. To consider dropping or changing that assumption leaves us with a problem: If not self-interest, what? Beyond self-interest lies an infinity of alternative conjectures.

Other values are often imputed to classes of actors in models dealing with specific sorts of repeated choices. In economics it is frequently stipulated that suppliers are motivated only by pecuniary interests. For example, a firm is conjectured to maximize profit. In political models, Downs (1957) conjectured that political parties and politicians faced with competition in a plurality election system would seek to maximize votes to gain office. Frohlich et al. (1971) conjectured that politicians were interested in maximizing profit; Niskanen (1971) assumed that bureaucrats were interested in maximizing their budgets, and so on. It is through such conjectures that some abstract models yield substantive hypotheses about empirical phenomena. Of course, such conjectures are not without controversy. Does a candidate care only about election? Perhaps candidates have personal attitudes about the issues in their platforms. If so, perhaps the model will be less than perfectly predictive.[12] Nonetheless, we should not be too hasty in disregarding a model simply because we wish to include a more expansive notion of preferences or self-interest. Models are to be learned from. For example, models that postulate election (or reelection) to office as the politician's motivation can teach us something about the strength of the ballot as a tool for voters to control their representatives.

Main Features of Applications

Levels of Analysis

A hallmark of rational choice theories is the predominant (though not exclusive) focus on the *individual* as the actor making the decisions. This focus stems directly from two things. First, the foundational conjectures are obviously formulated with individual actors in mind, and many questions are specifically about how individuals behave in given contexts. Second, even if we assume that individuals can make rational choices, we cannot guarantee that groups will.[13] An example of how individual rationality can lead to group irrationality is discussed in the next section. Strong assumptions must be made in order to guarantee group rationality. Nonetheless, we still might ask how a system would operate *if* such strong assumptions were met. Thus, collective actors such as states, social classes, unions, etc., continue to be used as decision agents in the

literature of formal theory. Some of these will be discussed further below, but to illustrate the range of collective actors in rationality models, we draw the reader's attention to a few examples.

Applications to international relations often begin with some notion of welfare maximizing states. One of the earlier ones (Olson and Zeckhauser 1966) was criticized from this point of view (Oppenheimer 1979b), but it clearly yields interesting, testable results regarding the likely distribution of benefits and costs from alliances and other international organizations. Similar assumptions about rational aggregate actors have been used to develop conditions which generate a stable international balance of power (Niou and Ordeshook 1986; Wagner 1986). Still others, interested in strategic choices leading to or away from interstate war, have confined their analyses to state decisions not involving collective decision making (Lalman 1988; Bueno de Mesquita and Lalman 1992).

Other formal theorists have developed models built on Marx's notions of class conflict. They assume that classes make choices to maximize the aggregate welfare of their members. These include Przeworski and Wallerstein (1982) who show that Marx's predictions of inevitable warfare, rather than compromise, was logically wrong. Indeed, arguments relying on class rationality were an early target of formal theorists. For example, Olson (1965) argued that, in the absence of other incentives, one cannot expect the members of any class to struggle for their collective interests to any significant degree. On the other hand, even Olson (1982) used a similar collective rationality assumption on the part of interest groups to develop arguments regarding constraints on economic development.

Institutional Constraints and Questions of Stability

Institutional contexts matter. Individuals acting rationally can arrive at different outcomes in different institutional settings. These institutions, then, act in some way as constraints on actions. Or, as Plott (1978, 207) puts it:

> "Institutions determine the rights and powers of individuals. In the game theory jargon institutions determine the acts available to players at the time they act as well as the consequences which result from any pattern of acts taken by players. Clearly, the consequences, in terms of the social choice from among the feasible alternatives depend upon both the preferences of individuals and the institutions which define the process. In the game jargon, we simply say that the outcome resulting from the game depends upon the set of feasible outcomes, individuals' preferences, and the rules of the game. These rules, which govern, or at least influence the outcome of the overall game, form the basic institutions of the society."

The study of institutions and their effects on policy outcomes is, of course, a traditional concern of political scientists. However, we find that the prediction of a single, stable, good outcome, which is so often reached in economics, is rarely to be hoped for in political settings.

The outcomes produced from political institutions may not be a strict function of individual preferences and the rules of the institution. Indeed, policy alternatives may not be stable, in spite of the fixed preferences of the individuals. To understand this troubling aspect, let us define a concept of *stability.* An outcome would be stable if *no* group of actors has both the ability and the desire to change it. These outcomes are undominated by other outcomes, and they are commonly known as the *core.* In economics it has been shown that the general market outcome is, under a wide variety of conditions, such a stable outcome. That is, it is a member of the core (see Newman 1965, chap. 5). In contrast to this, many non-market decision problems can be shown to have no stable outcomes in the sense just described.

We can illustrate political instability with the classical example of the voters' paradox. The simplest form of the voters' paradox can arise with three individuals $\{1, 2, 3\}$ using the simple majority decision method to select a winner from three alternatives $\{a, b, c\}$. Each voter can rank the alternatives from best to worst. Now suppose that the preferences of the voters are:

Ranking of Outcomes	**Voter 1**	**Voter 2**	**Voter 3**
First	a	c	b
Second	b	a	c
Third	c	b	a

In a vote between a and b, a wins, supported by the coalition of voters *1* and *2*. Between b and c, b is the majority winner, supported by the coalition of voters *1* and *3*. We might think that since a wins a majority over b and b wins a majority over c, that a would be able to win over c. Such would be the case if majority rule guaranteed transitive outcomes. But, paradoxically, we see that the coalition of voters *2* and *3* decides for c over a -- a defeats b, b defeats c, c defeats a.... As a group, decisions on the outcomes are cyclic. The three who could individually rank the alternatives from best to worst are unable to do so as a group. We can also see that the

core is empty in this situation. For any of the three outcomes there is a winning coalition interested in overturning it. There is no equilibrium. And without an equilibrium, predictions in politics are tenuous, at best.

Although we have illustrated the lack of stability and the emptiness of the core with a specific example of a voters' paradox, we hope to make clear below that this deficiency is far more general.[14] Stability in political institutions is an elusive prey.

Major Findings

Voting Theory

The theory of rational voting in general, and the theory of majoritarian voting in particular, are probably the best developed theoretical results in all of political science. Even casual reading in the area is likely to impress one with an awareness that a substantial set of questions have been well formalized and answered. Specifically, a large number of questions on how and when majority rule can (or cannot) aggregate the preferences of voters in a coherent fashion, have been studied. Further, the normative values associated with the majority voting method have been identified and well formalized. Of course, research continues, but the research is ever more focused.

Majority rule is surely not the only voting method used in political gatherings. But it is the first to come to mind, and it is hard to think of a legislature, a corporate board, or even a social club that has not acted as a committee and used majority rule to decide an issue. Contained within the logic of the rule, though not immediately obvious, are a number of problems, including the potential for the cycle that arises from the voters' paradox. In such situations an alternative may well win but victory will not stem only from the preferences of the voters and the voting rule. Something beyond these factors would be required to stop the cycling and lead to a winner. To see this, and a number of other interesting aspects of the problem, we begin with a very simple case: one without cycles.

The Median Voter Theorem: A Case of Stability

In the late 1940s Duncan Black (see Black 1958) began investigating majority rule as a decision-making method. He asked what we might expect if

1. a group of individuals are to vote on policy alternatives represented as points on a line
2. each of the voters has a most preferred, or ideal, point (policy), and
3. for each voter, alternatives farther to the right (or left) of the ideal point are increasingly less preferred.[15]

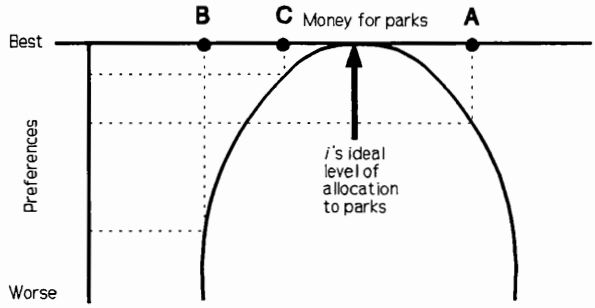

Figure 1: Displaying a Single-Peaked Preference

We begin the analysis with the utility function displayed in Figure 1. The horizontal axis represents allocations for a parks budget. The utility function represents the preferences of an individual who feels that one size of budget is preferred over all others -- an ideal policy. To spend less than the ideal is increasingly less preferred for smaller and smaller budgets. (In Figure 1, the individual's preferences are symmetric around the person's ideal point.) Also, to spend more than the ideal is increasingly less preferred. Such preferences are referred to as "single-peaked," for fairly obvious reasons. Of course, this individual will vote for the ideal point over any alternative opposing it. This individual's preferences are well defined across any pair. For example, budget *C* is preferred to budget *A*, which is in turn preferred to budget *B*. Notice also that the preferences are transitive across all budgets.

Now let us use this notion to illustrate the workings of majority rule under the circumstances stipulated by Black. Consider that the parks budget is to be decided upon by a committee of five voters $\{1, 2,\ldots,5\}$, each with single-peaked preferences. Figure 2 illustrates the preferences of these individuals. What might interest us in such a situation? Imagine that voting leads to an outcome somewhere along the line beyond the ideal points of the most extreme voters *1* and *5*: say alternative x. Any such outcome would be quite a poor choice for the group. After all, there are alternatives that *every* voter prefers over those such as x. Within the bounds of the extreme voter ideal points there would always be *some* point *everyone* could agree to over any point outside this set. This desired property is called *Pareto optimality*, and sets with this property are *Pareto sets*.[16] We see that the Pareto set for all the voters is the line segment connecting their ideal points, and against any outcome not in the set, some point in it could be supported by unanimity. For example, voter *5*'s ideal

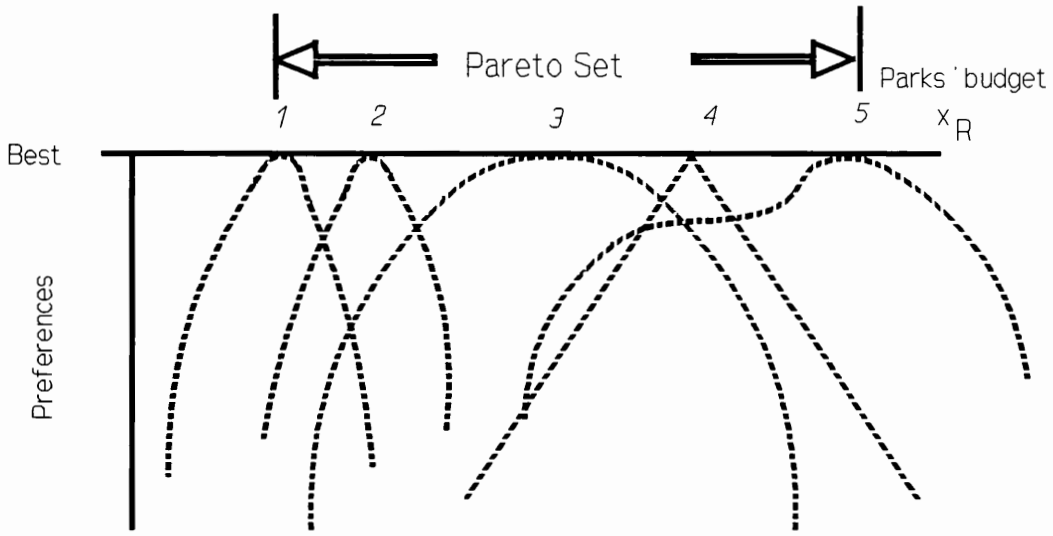

Figure 2: Pareto Set Given N, Single-Peaked Preferences, 1 Dimension

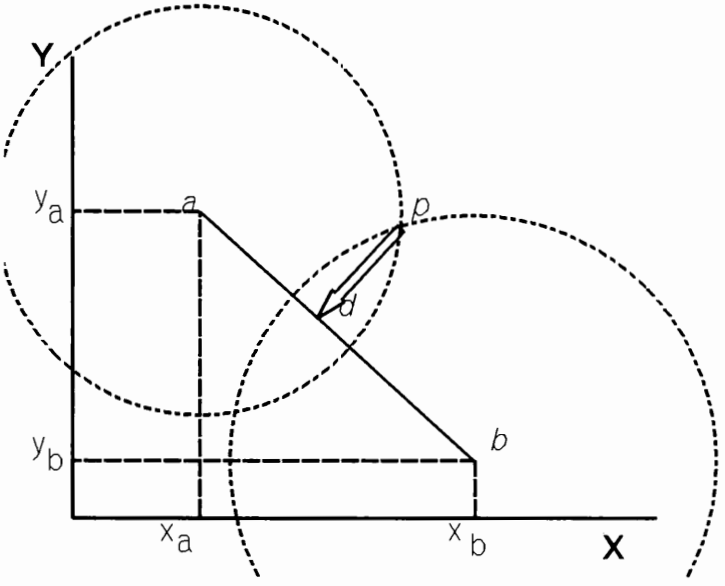

Figure 3: The Ability of 2 Voters to Agree to a Point on the Line Connecting Their Ideal Points, Rather Than a Point off It

alternative is supported unanimously against any alternative to the right of it, such as x. Similarly, there is unanimous support for voter *1*'s ideal alternative over any point to the left of it. But in a contest comparing any two points lying between the ideal points of voter *1* and voter *5* we could not get unanimity. So, in this simple case, majority rule supports Pareto optimality. This may not seem like a lot, but it is certainly a minimum which we would hope could be achieved. After all, why use a method that cannot deliver an outcome which everyone could agree upon if they but junked the procedure?

With this in mind, we might wonder what other properties majority rule delivers when it is employed in this simple, one-dimensional space. Does it lead to moderate or centrist policy outcomes within the Pareto set, or to extreme policies, for example? Under the circumstances of single-peaked preferences on a single dimension, Black showed that the *median voter,* who has an equal number of voters on either side (in this case, voter *3*), is in a privileged position. Just as we saw that the ideal policy of voter *5* is unanimously supported over policies to its right and *1*'s ideal is unanimously favored over motions to the left, there is *majority* support for the ideal policy of the median voter over any motion away from it. The median voter can form a coalition with voters *1* and *2* to vote down alternatives to the right. Motions to the left are defeated by the coalition of voter *3* together with voters *4* and *5*. We see then that the ideal point of the median voter is the equilibrium outcome under majority rule. This is Black's median voter theorem, a fundamental result in formal political theory.

Let us shift our focus and consider questions of coalitions on this committee. Analogously to the arguments above, a coalition's members can *unanimously* agree to restrict their proposals to the points which connect the extreme ideal points of its members. Notice that the median voter's ideal point is part of the line segment connecting the ideal points of any majority set of voters (i.e., the median voter is a member of all winning coalitions). The median voter's ideal point is therefore a member of all the possible proposals which winning coalitions would put forward. Indeed, it is the only such member. So if it were passed, there would be no possible winning subgroup of committee members who could agree to back any other alternative. No coalition would have the power and the desire to change the outcome. No other point can get the support of all winning coalitions, and therefore the ideal point of the median voter is in, and is the only element in, the core. Majority rule goes further (when there are single-peaked preferences along one dimension) than selecting the Pareto set. It selects the median ideal point as the equilibrium from the elements of the Pareto set.

Beyond One Dimension and the Median Voter

Black's median voter theorem gave us a description of conditions under which majority rule aggregates the preferences of voters to produce a stable, "moderate" outcome. In demonstrating the theorem he also gave us the spatial framework for analyzing voting. But Black went beyond this to research the consequences of majority rule when, though the preferences of the voters are single-peaked, the policies under consideration have more than a single attribute. An example would be voters considering not just the size of a single budget, but the allocation of a budget across multiple programs, such as money for parks and money for police. In these situations, one finds that we have reason to be less sanguine about majority rule (Black and Newing 1951).

Now to represent these policies we need more than points along a line. We must add dimensions to form a multi-dimensional policy space. Policies are now points in this space. Do we still get a stable outcome? Is it still the case that majority rule yields moderate results? If we are outside of the Pareto set, do we have reason to expect movement toward it? Do we have results in the core? These are typical formal theory questions, and each can be answered.

To illustrate these results, consider a simple example. Figure 1 represents two individuals whose ideal levels of **X** and **Y** are represented as the points a and b. For the sake of greater simplicity, assume each individual ranks all policies strictly by how close that policy is from her ideal point. It does not matter what the direction from the ideal point may be; greater distances are less preferred.[17] Notice that such preferences are still single-peaked. The analytical convenience of the restriction is that such an individual is indifferent among points that are equidistant from the ideal -- a circle with the center at the ideal point. Points within a circle are closer to the ideal point and hence more preferred; those outside are more distant and less preferred.

Our two individuals in Figure 1 agree that compared to the point p there are points they both would approve of. Constructing the indifference set (circle) for each person through p creates a lens-shaped area where they overlap. Points within this lens are mutually agreeable. For any point p off the line segment connecting a and b there is a point on the line segment (referred to as the *contract curve)* that both prefer.[18] That is, the line segment ab constitutes the Pareto set for the two individuals.

Consider now the situation with additional voters, in this case three: a, b, and c. In Figure 4, each has circular indifference curves. Analogously to the argument just made, they could unanimously agree that a

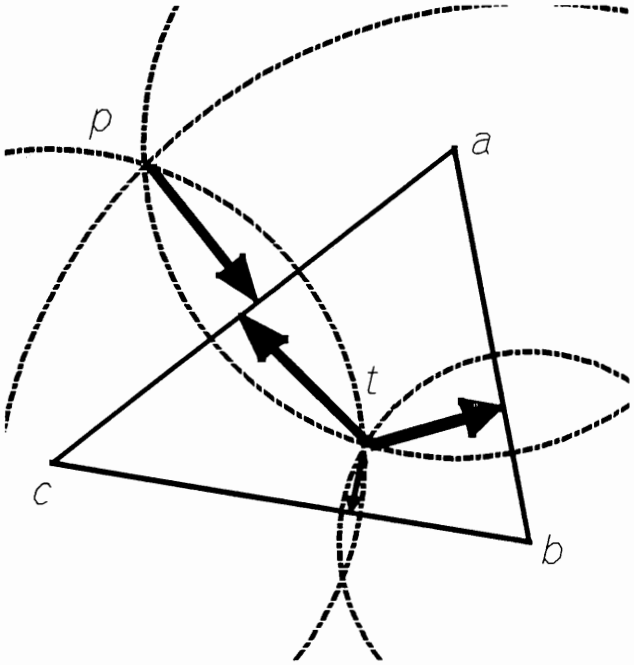

Figure 4: 3 Persons, 2 Dimensions. A Core Exists for Unanimity, but Not for MR

point outside the triangle made by their ideal points is not as good as *some* points on or inside the triangle. Thus the triangle and the points within it are the Pareto set. And, as must be the case for a point in the Pareto set (see above), when we consider points within the triangle there is no point able to garner unanimous support over any other point.

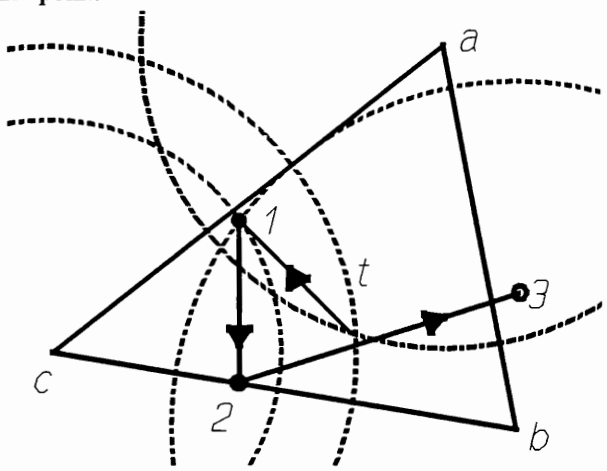

Figure 5: A Voting Trajectory out of the Pareto Set

With majority rule, are there any stable points analogous to the median voter's ideal point in the one-dimensional case? The preceding paragraph showed that we should search for any such point within the Pareto set. So consider a point inside the triangle *abc* such as *t* in Figure 4. We see that a variety of majority coalitions can upset any such point. There are three lenses containing the alternatives that represent possible improvements over *t* for each of the two-person, winning coalitions. Since with majority rule, any outcome obtained by a winning coalition can be outvoted by another coalition, *there is no core in this situation.*[19] In contrast to the median voter theorem, *t* and other such centrist policies are *not* stable. Also note that each of the lenses includes points outside the triangle *abc*; the lenses include non-Paretian, or *Pareto-dominated,* points. In Figure 5 we have duplicated the situation, but drawn one of many possible voting paths representing a series of three votes starting at *t*. *t* is unstable relative to the points in any of the lenses, and these points are in turn unstable themselves. Consider the majority voting path we have constructed from *t* to *1*, to *2*, to *3*. The coalition *ac* obviously prefers point *1* to *t*; *bc* prefers point *2* to *1*; and *ab* prefers *3* to *2*. This shows that majority rule can lead to a Pareto-dominated outcome. So majority rule in more than one dimension, even with single-peaked preferences, does not guarantee stable outcomes. Nor does it restrict outcomes to the Pareto set. We find that by relaxing just the restriction that the policy is one-dimensional, majority rule loses its nice, well-behaved properties. Indeed, majority rule appears grossly unstable. Outcomes can wander about, even if the decision rule and the preferences of the individuals remain fixed.

In one dimension, single-peaked preferences force a symmetry in the voting situation. At the median voter's ideal point, the voters to the left have the same voting strength and are diametrically opposed to those on the right. When the median ideal point is threatened, the median voter can turn either coalition from losing to winning. Such symmetry is missing in the two-dimensional case of Figure 4. This lack of symmetry is typical when there is more than a single dimension. The voter who is the median in one dimension is not generally the median voter in the other dimension. Indeed, in two dimensions there are an infinite variety of directions to go from any point, and in each direction we could have a different median. With this variety goes our majority rule equilibrium.

Figure 6 contrasts the condition of symmetry with that of asymmetry. The ideal points of five voters $\{a, b, c, d, e\}$ with Euclidean preferences are contrasted with a very similar 5-person set $\{a, b, c, d, e^*\}$. For each of the three-person winning coalitions, the optimal

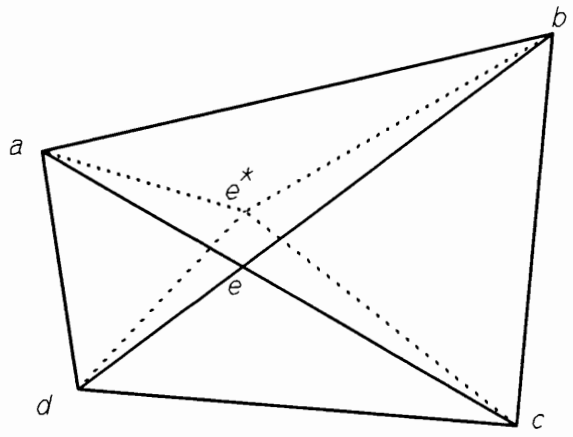

Figure 6: 5-Person, Two-Dimensional Majority Voting: With, and Without, an Equilibrium

set of outcomes is the points on or in the triangle constructed with their ideal points. In the first set, *e* has an ideal point at the intersection of the lines connecting the diagonal pairs of voter ideal points. Now *e* is in each of the winning coalition's set of optimal points. And, there is no three-person coalition which can agree to form in order to upset the policy *e*. Thus, *e* is the core, the equilibrium outcome in this example.

Notice how different the situation is if the central ideal point, *e*, is replaced by a just slightly different ideal point, e^*. Now e^*, though still near the center of the diagram, is no longer in equilibrium. Two of the three-person coalitions, *bcd* and *acd*, have an incentive to form to upset e^*. e^* is not part of an optimal set for those coalitions, since it is not in the triangle associated with each.

Plott (1967) formalized a set of symmetry conditions in a spatial context of any finite number of dimensions which leads to a majority rule equilibrium. Plott demonstrated that when there is a single point which is a median in *all* directions, much like the hub of a bicycle wheel, an equilibrium will exist. Note that *e* is such a hub, but e^* is not. We have no reason to expect such a demanding condition to be satisfied in the real world. The apparent unlikeliness of these conditions shows the fragility of majority rule. When there is no radical symmetry, there is no stable outcome. So, in Figure 6, there is no end to the coalitional cycling when e^* is the fifth person's ideal point. Not comforting.

When Plott's symmetry conditions are not met, how big a problem do we have? Still using only majority rule and conditions on preferences, McKelvey (1976, 1979) and Schofield (1978) produced results indicating that even a slight breakdown of the symmetry in preferences leads to intransitivities that are not even limited to any region of the policy space. Surely, for normative theorists this lack of equilibrium is discomforting. Yet, these results may not be the final answer, and there may be reason to suspect some stability in political outcomes in spite of the ubiquity of disequilibrium and cycles. Theorists have shown that political arrangements and institutions surrounding majority rule can reduce the set through which outcomes can wander.

To show that although intransitivities can cover the space, does not lead one to conclude that political outcomes will wander unchecked. For example, imagine a committee with a chairperson. The chairperson might be a strong agenda setter with extraordinary knowledge of the preferences of the other voters. Such an image is not very distant from what we think of as congressional committees. The chairperson can indeed manipulate a cyclical voting situation. But unless those manipulations were made to her own disadvantage (not likely), she would not leave off the process with the outcome outside the Pareto set.[20] So the conclusions of how majority rule is likely to play out must be thought about within the context of our experience. We must ask, what are the institutional parameters of the process under study? Adding institutional and other features brings the models more verisimilitude with politics as we experience it.

But other restrictions modify majority rule instability. And since majority rule usually exists in a richer institutional context, these arrangements are usefully considered. For example, the common legislative practice of separating alternatives on an issue-by-issue basis is a restriction on the way majority rule is employed. The fact that there are conditions under which such issue-by-issue procedures promote stability is clearly of political interest, in that it addresses how political procedures affect outcomes.

Kramer (1972), Shepsle (1979), and Shepsle and Weingast (1981) find that when preferences are *separable* (i.e., preferences on one issue dimension are independent of the outcome on the other), separating the issues can stabilize outcomes and induce a regularity in politics. Notice that such regularities depend upon an elaboration of the majority rule method and a restriction on preferences that is more than a representational convenience.

Prisoners' Dilemma: The Problem of Collective Action, and the Developments of Non-Cooperative Game Theory

One of the main findings of economics is that purely selfish individuals can all profit from engaging in voluntary exchanges.[21] Hence, in a typical market exchange problem a buyer and a seller would contract with each other until there are no further possible gains from trade. The transactions would allocate the available gains among the two parties: neither the buyer, nor the seller, can get more benefits without a decrease in the benefits to the other -- the outcome is both optimal (see above) and stable. In these market exchanges, the object is assumed to belong solely to the purchaser. The nature of goods is sometimes quite different. Not only those who pay, but also those who do not, share the benefits. In such non-market situations, selfish motives no longer produce an optimal outcome. Indeed, this failure to generate satisfactory outcomes might be, as many would argue, the very reason for the existence of political institutions.

The above argument is not new. It has certainly been around since Hobbes. The fact that the common welfare may not be achieved as the consequence of purely selfish calculation underlies the classical call for the necessity of state. One formal model of such situations is the so-called Prisoners' Dilemma game: a specific model in the theory of games. From this perspective, politics is, in no small measure, "the study of ways of transcending the Prisoners' Dilemma" (Elster 1976). What, then, are game theory and the Prisoners' Dilemma game?

In short, the theory of games is an attempt to analyze the problem of choice in situations where choices are interdependent. These are often referred to as strategic situations. The theory of games is traditionally divided into non-cooperative and cooperative branches. In the theory of cooperative games, analysis focuses on situations where players can make binding commitments. There, the main analytical concepts involved are coalitions, side payments, and bargaining. The basic problems analyzed concern which coalitions will form, what payoffs they will obtain, and how the payoffs will be divided among the coalition members. Non-cooperative game theory, on the other hand, focuses on situations in which binding commitments are not possible, and where, therefore, individual actors, rather than coalitions, are central to the analysis.[22] We will take a closer look at the cooperative branch of game theory after finishing with the Prisoners' Dilemma. But first, we will use the case of the Prisoners' Dilemma game to exemplify the methods of and the results in the non-cooperative branch of the theory.

The Main Concepts of the Theory of Games and the Prisoners' Dilemma

The Prisoners' Dilemma (hereafter referred to as PD), together with the voters' paradox, is by far the most widely known and celebrated example of the use of formal theory in political science. The game has been used to seek insights, explain, and describe political interactions in virtually all areas of political science and public policy. Often the PD has been used as a model for the general problem of collective action (also called the free rider problem, or the "tragedy of commons"). Numerous applications (cf., Axelrod 1984) have an impressive range: from international politics and cooperation (Snidal 1985; Oye 1986), to international trade (Laver 1977), to problems of political philosophy (Taylor 1987). Below, we will use PD in yet another capacity -- to exemplify the steps involved in the construction and development of any formal theory. We will try to show what such a theory does and does not contain, how it relates back to the reality, how it generalizes beyond its simplest form, and how it grows into a cumulative body of research.

The name of the PD game is derived from the story told to motivate interest in the dilemma it presents: Two partners in crime are taken prisoner and face alternative prison terms depending on which one of them, if either, chooses to give the state evidence.[23] Between this story and the realization that much of politics is "the study of ways of transcending the Prisoners' Dilemma" lies a theory which needs development to lead to this insight. Like any theory, it begins with abstraction, the heart of any creative analysis.

The genuine insight in science remains in separating, from an infinitely complex web of reality, a small number of crucial factors. The first steps in theory construction involve therefore a generous use of Occam's razor. In the PD story the key components include the **players** (two suspects), their available **strategies** (to defect from each other by confessing, and to cooperate by not confessing), and **payoffs** (preferences on or utilities of outcomes). These three elements indeed comprise the fundamental concepts of game theory. The depiction of PD in the so-called normal or strategic form is presented in the table below. This otherwise simple conceptual and representational framework -- in which the PD strategic interaction is represented -- constitutes one of the main insights of the non-cooperative theory of games. There are the players: *Row* (her strategies are represented as rows) and *Column* (with strategies represented as columns). The outcomes are associated with strategy

choices by the players and are shown as pairs of numbers in the four cells of the table. The players' preferences over the outcomes are further represented there by the two numbers in each of the cells: the first number representing the payoff to *Row*, the second to *Column*. Now, the fundamental assumption of the theory conjectures that players choose strategies to maximize their payoffs.

Table 1. A One-Shot Prisoners' Dilemma Game

	Column Cooperates with *Row*	*Column* Defects from *Row*
Row cooperates with *Column*	2, 2	0, 3
Row defects from *Column*	3, 0	1, 1

Payoffs, Rationality, and Self-Interest

Payoffs are part of the abstraction and they are considerably stripped here of their everyday meaning. In the simplest form of the game, numbers depicted in Table 1 would merely indicate the order of players' preferences on outcomes: from the best (3) to the worst (0). For some applications, however, it may be reasonable to interpret the numbers in the table as units of utility for the players. The utility interpretation is usually more natural and convenient; hence we use it (even though the "preference interpretation" is more general and sufficient to define the PD).

How will a rational player behave facing the PD situation? First note that any player is better off defecting, no matter what the other player does. To see this, consider the choice situation confronting *Row*, for example. *Row*'s strategy choice has to be made independently of that of *Column*. (This assumption can, for simplicity, be thought of as players making simultaneous choices.) The opponent, *Column*, could cooperate or defect. If *Column* were to cooperate, then *Row* could get 3 (**T**) or 2 (**R**) depending on whether she defects or cooperates; clearly, defection gives her a higher payoff. If *Column*, on the other hand, were to defect, then *Row* could get 1 (**P**) for defecting and 0 (**S**) for cooperating. Here again defection gives her a higher payoff. In short, no matter what the other player does, *Row*'s defect strategy yields a higher payoff. Technically, we say that the strategy of defection strictly dominates the strategy of cooperation: it is best under all circumstances. And since the same reasoning applies to the other player (their situations are purely symmetric), rational players would both defect in this game, and each will end up with a payoff of 1. Thus, the "paradox": rationality leads to a suboptimal payoff. Had the players cooperated, they would each have received the payoff of 2.

Part of what seems paradoxical about the suboptimal solution to the PD is related to the assumption (in all non-cooperative games): players can make no binding contracts. While such agreements would obviously be useful to secure the cooperative payoff, enforceable contracts would lead us to model the situation differently and use different tools (those of cooperative game theory) to analyze it. In consequence, solutions to the cooperative form of the game may be different. Indeed, the cooperative version of the PD presents no dilemma at all. The optimal cooperative outcome is the sole solution of the game -- the only element of the core.[24]

From Simple Ideas to Complex Formalizations -- The Point and the Power of Formal Modeling

The Collective Action and Other Empirical Problems as Prisoners' Dilemmas

As a depiction of a real political interaction, Table 1 does not look very compelling. Even if some situations, like an arms race, seem to resemble a PD, the one-shot (no future interactions) assumption is often not a reasonable presumption. Arms races occur over time, with many non-binary decisions being made by parties that alter their responses in a sequence of moves. Likewise, collective action problems may intuitively seem to have a resemblance to a PD, but since they almost always involve many participants, a two-person matrix would be inadequate. Similar concerns are barriers to most direct applications. On one hand, many actual situations seem to share the core properties of the PD; on the other hand, their depiction as a one-shot, two-person, two-option game is inadequate. Applications require changes in the model. But do the changes fundamentally alter the analysis of the situation? Any such change of the model would have to involve, in one way or another, a more general depiction of the PD game. While there are a number of possible ways to define a PD, the following would be the core elements of all such definitions:

> (i) Defection is the dominant strategy (for each of the players); hence the defection of all is the predicted equilibrium outcome.

(ii) The equilibrium outcome is not Pareto optimal; the payoffs from universal cooperation are higher for all players.

With these two definitional features, it should be much easier to see the scope and the generality of the PD problem. Note first that (i) and (ii) do not rely on the assumption of only two players. By generalizing beyond two persons, we are permitted more serious applications -- such as to collective action problems. Consider automobile emissions problems, as an example. Assume, for simplicity, that in a society of similar individuals, each must decide whether to install a catalytic converter in her car (cooperate) or not (defect). Suppose that the benefit of lead-free air (the outcome of all cooperating) is worth more to an individual than what it costs to install a converter. Will individuals install such devices in their cars? No matter what everyone else does (i.e., whether they all install, only some install, or nobody installs), any single person's choice will have but a negligible effect on the overall quality of air. Certainly, the marginal improvement in air quality would not outweigh the cost of the converter. Hence, the logic of the problem is clear: regardless of everyone else's behavior, an individual is better off defecting [property (i)]. Further, when all defect, everyone is worse off than if they all cooperated [property (ii)]. In this generalized PD, all players defect and all end up with an inefficient outcome: the central property of the dilemma.

Properties (i) and (ii) permit us to go further. They lift not only the constraint on the number of players, but also the limit on the number of strategies available to any player. To see this, consider the decision of how much money to contribute to a charity. Strategies in this case are numerous: every different amount of money constitutes a separate option or strategy. With some straightforward assumptions (similar to those used in the problem above), it is easy to conclude that the logic of the problem would result in conditions (i) and (ii). Hence, neither the number of players nor the number of actions available to them is essential for the PD effect; the simple two-person, two-strategy PD can be extended in a natural fashion to cover a large and important class of political phenomena (Hardin 1971).

Indeed, the generic PD problem is an aspect of the provision of all public goods: goods for which the market does not operate as an effective exclusion mechanism for those who do not pay. Such are the problems of cleaning up the environment, working for (or against) a revolution, contributing to a public radio station: goods which, once provided, can be used by all, including those who did not contribute to its provision. In all these cases, the benefits of any contribution are distributed widely, but the costs are incurred privately. This is a generic property of what are called *public goods*. An argument which shows that selfish motives will not lead to the provision of a public good is often referred to as the problem of collective action. This view, called by Olson the "Logic of Collective Action," (Olson 1965) was revolutionary in the 1960s when it was proposed as a general characterization of problems facing political groups. By now it is a part of the foundations of the discipline. Indeed, the general mechanism behind the failure to cooperate turns out to be the same in the collective action problem as it was in the PD game.

Other views of the collective action, and public good, problem have been developed since Olson's classic. The PD is now but one of a set of alternative games developed to deal with the empirical domain of the problem. So, for example, Dawes and Orbell (1982), Frohlich et al. (1970, 1975), and Schelling (1978) model the situation without dominant strategies. Taylor (1987) develops alternative models quite fully and discusses their application to collective action problems. Many of these have since been subjected to extensive experimental tests.

Repeated Games

Above, we established that the essential properties of the PD do not vary as the number of either players or options increases. But there are other important extensions of the basic model. Most real-life situations are not isolated -- they are bound to repeat, often in the same or similar form. Suppose that a PD interaction is being repeated; what would be the predicted outcome? Are rational players doomed to end up with the inefficient equilibrium outcome again, or is there a possibility to do better?

To develop an analysis of repeated games, we must go beyond our simple tool of a dominant strategy to predict the game's likely outcomes. Indeed, a game -- like the one-shot PD -- in which both players have a dominating strategy, is not a good example to use to display the power, and methods, of game theory. After all, no theory is really needed to predict what happens in a one-shot PD -- the outcome is both obvious and robust.[25] When a player has a strategy which dominates all her other options, there is little ambiguity about what she should do. But such games are very unusual. In a typical game, a strategy may be "good" (i.e., maximizing) to play against *some* of the strategies of the opponent, but not all. It is for this more general circumstance that we must ask, how will rational players choose in the game? In all non-trivial repeated games, and certainly in the repeated PD game, there are usually no dominant strategies, and so one must search for an answer to this question.

Toward a More General Solution -- The Nash Equilibrium

A solution concept in the theory of games is simply a prediction of how the game will be played by rational players. Among many solution concepts, the concept of *Nash equilibrium* is the most fundamental. To illustrate it, consider yourself playing against a single opponent. If you choose strategy **A** and your opponent chooses **B**, we would want your choice to be stable (or in equilibrium) once you discovered the choice of your opponent. What does it mean for a pair of strategies, **A** and **B**, to be in equilibrium? Given the assumption of rationality, the reason for you to choose **A** must be that **A** is the best strategy to be used against the strategy chosen by your opponent. To interpret this, ask: "If my opponent chooses **B**, can I do better by choosing something other than **A**?" (The other player can reason analogously.) Now, if neither of you has an incentive (can get a higher payoff) to change your strategy *unilaterally*, then **A** and **B** are a pair of strategies in (Nash) equilibrium. The notion of no *unilateral* incentive to shift is the essential meaning of this equilibrium concept.[26] Nash (1953) has proved that every (finite) game has at least one such equilibrium.

The Possibility of Cooperation -- The Folk Theorems

Equipped with the general solution concept of the Nash equilibrium, we can now explore equilibria in the two-person repeated PD. Repetitions bring a new incentive structure to the game. Through repetitions players can condition their current moves on the past play of their opponent. Defections can be punished and cooperations rewarded. If players care about future payoffs and the game does not have a definite termination point,[27] it would seem that a threat of punishment (for example, a 'grim-trigger' strategy: "if you defect in any period of the game, I will defect forever after") should be sufficient to sustain stable cooperation in every period of the game. This simple intuition is essentially correct. It might be less obvious, however, that a whole spectrum of other behaviors, from pure cooperation to pure defection, can be in equilibrium as well. As it turns out, there are infinitely many possible Nash equilibrium outcomes and they can support any "amount of cooperation" in a stable state. Theorems which establish this have come to be known in game theory as the Folk Theorems.[28]

One important finding of the folk theorems for the general PD problem is that *cooperative equilibria are possible* and so the optimal (cooperative) outcome can be attained without external enforcement.[29] While this is the first good news, at least two related issues should still be addressed: First, which parameters of the model make the cooperative equilibrium possible, and second, what conditions, if any, make cooperation more likely? Folk Theorems, after all, merely state that all outcomes are possible in equilibrium; they do not say which ones are more likely. We now turn to these two questions.

The Importance of the Future

An important assumption in Folk Theorems concerns the expected value of the future payoffs: the theorems hold only if the "future is important enough." This means that only with sufficiently small discounting of the future, and sufficiently large probabilities of continuing the game, can the entire range of equilibrium states -- from pure defection to pure cooperation -- be attained. (If individuals heavily discount future payoffs and/or the probability of continuation is small, mutual defections constitute again, like in the one-shot case, the only Nash equilibrium.[30])

The Blessing of Bounded Rationality

By showing that any outcome is possible in equilibrium, Folk Theorems are practically useless for making predictions. Can further changes to the model make one of these equilibria more likely than others? Suppose that we "assume away rationality" and hypothesize that in a population of players a simple evolutionary mechanism, rather than rational calculation, leads to the adoption or abandonment of certain strategies. The so-called evolutionary theory of games is a very recent direction of research. (It takes its root in biology with the works of Maynard Smith and Price 1973; Maynard Smith 1982). Axelrod's (1984; Axelrod and Dion 1988) highly influential, and ingenious, simulations and findings suggest that cooperation can evolve from anarchy. Bendor and Swistak (1992a, 1992b) prove that the purely cooperative state is the most robust stable state: the easiest to attain and the hardest to disrupt. And so cooperation turns out to be the most likely equilibrium state -- bounded rationality helps in solving the notorious dilemma.

Other Real World Twists

Above, we developed generalizations of the original PD game. Each variation of the conditions of the game has generated its own form of analysis, consistent with the premises of the theory of games. Together they contribute to widen our storehouse of cumulative findings. There are more variations, and more results; some of these variations are nested in fairly autonomous branches of game theory. The listing in Table 2 exemplifies a few directions of research:

Table 2: Variations of Game Conditions and Families of Analysis

Variation of originally stated conditions	Resulting family of game analysis
players move in turns	extensive form games
interactions are repeated in the future	repeated games
players do not know payoffs to others	games of incomplete information
players use constrained maximization	bounded rationality, bounded complexity, and evolutionary games

This list, though far from being complete, symbolizes and reflects the power of formal theorizing: insights into the problem are sought at the most basic level (e.g., the one-shot PD game). But once this is achieved, one can bring back many of the factors which were initially abstracted away. One can then study the influence of these factors on the outcome of the game one at a time. Theoretical developments in formal theories thus resemble a robustness analysis. Even though some theorems may be very difficult to prove and theoretical progress may stretch over decades, each new result contributes a part to the theory as a cumulative endeavor.

Predicting Political Coalition Formation and Stability

As already pointed out, in non-market situations often no outcome enjoys stability. This means that for any coalitional structure, there is always some group whose members can do better by breaking away from the current arrangement and forming a new coalition. Then, no matter what the status quo, some group would have both the ability and an incentive to change it. In the language of cooperative game theory, we would say that there are no outcomes in the core or that the core of the game is empty. We have already shown the core to be usually empty in spatial voting problems. Below, we will see that such instability is a typical, rather that an exceptional, property of many political choice processes when considered without additional institutional and other environmental factors.

Empty Cores Reconsidered

If we agree with Lasswell, that politics consists of non-market deals to determine "who gets what, when, and how," the generic substance of politics can be seen as redistribution. If we now use game theory to address the issue of stability of various redistributional options, the answer, much as in voting games, turns out to be unsettling. In a large and important class of redistributional games, stable outcomes do not exist. More specifically, if we conceive of a problem as a purely redistributive (formally equivalent to a zero-sum) cooperative game (players can communicate and make and enforce contracts), we can show that no such game has outcomes in the core (cf., Luce and Raiffa 1957, 193-96). The simple game in Table 3 illustrates this general problem.

Table 3: Purely Distributional Game Showing Cyclical Coalition Problem

Line #*	Coalition	Payoffs	In the Bargaining Set?
1	123	1/3, 1/3, 1/3	Yes
2	12	1/2, 1/2, 0	Yes
3	13	2/3, 0, 1/3	No
4	32	0, 1/2, 1/2	Yes
5	12	1/4, 3/4, 0	No
6	13	1/3, 1/3, 1/3	Yes

* The first line is arbitrary. Each subsequent line represents a coalition, and proposed payoff structure, which beats the proposal above it.

The game consists of three persons who use majority vote to distribute one dollar. This means that any two (or three) person coalition that can agree to a distribution would also have power to enforce it. With this definition of the game in mind, we can now demonstrate that no distribution is stable in the sense previously mentioned. Table 3 illustrates the main steps in the reasoning. To see why the core of this game is empty, first assume that some group got together and considered a potential distribution. Suppose, to initiate the argument, that all three players agreed on an equal share. This is a situation depicted in line #1 of Table 3 (an analogous argument can be presented for any possible initial distribution). But now a two-person coalition, like the one on line #2, can clearly do better for itself and beat the first coalition. Thus, the coalitional arrangement of line #1 is unstable. Yet any potential two-person majority would not be stable itself, since the individual left out can strike a deal with one of the two coalition members. The outsider can propose something that both

would prefer over the current arrangement (like the coalition of line three). But, this can again be beaten by the arrangement of line four, and so on. There are no incentives in the game to prevent the round robin from continuing.

The logic of this argument extends to the general case of distributive problems. As long as the rules of the game assure that a majority is decisive, the coalition of the whole would not be stable. A smaller group, suffice it constitutes a majority, would profit by forming a coalition and excluding everyone else from sharing in the distribution. Yet, the excluded minority would have incentives to make other deals which would further destabilize the arrangement, and so on. The conclusion generalizes to larger groups. It goes beyond majorities and voting. The general conclusion goes beyond the simple examples: *without further constraints, or further political arrangements, no coalitional struggle over a purely distributional matter has a stable outcome -- it is not possible for all coalitions to be satisfied at the same time.* Hence, no way of structuring the payoffs insures that all players get what they could guarantee themselves *if* they formed some other coalition; there is always a coalition able to improve the payoffs of all its members.

However, the problem of instability goes farther than that: empty cores turn up well outside the class of purely redistributional games.[31] And stability turns out to be an exceptional rather than a common property of cooperative games. Like Arrow's theorem, the common nonexistence of the core[32] helps us realize that many political outcomes and processes do not have the good properties that we would otherwise like them to have. Instability of all outcomes in games with empty cores means that no *single* outcome can be used as a prediction. Needless to say, there is something unsettling about this. After all, most of our theories (in the physical and life sciences, economics, demography, etc.) yield unique outcomes as predictions. Hence, an important insight from these models to political science is showing that political reality is, in this sense, a different type of reality: unique predictions are often not to be expected.

While theoretical models incorporate a relatively small number of variables, a large number of relevant, or potentially relevant, factors is always assumed away. In consequence, results which are obtained in such models are not directly applicable unless some additional methodological assumptions are specified. Most commonly, though rarely stated, the theorist assumes the effect of the factors which are not included in the model is negligible. Often one questions such assumptions (e.g., the negligible effect assumption) to investigate the robustness of a theoretical result. If, for instance, a game has an empty core, would it remain empty if the payoffs were altered by some small (epsilon [ϵ]) amount? After all, payoffs in models are admittedly artificial: they do not include the small effects of the excluded factors.

This leads us to the following question: Can a non-empty core be obtained with a small shift in parameters? If so, a stable outcome can, after all, be empirically observed without contradiction of the theory. Second, if stability is not obtained, we may be able to induce it through small changes in payoffs. This second would be a crucial concern if payoffs can be effectively altered by policy decisions, even if the change would have to be substantial. These two important concerns, one methodological (robustness), the other political (altering payoffs via government intervention) motivate the so called ϵ-core (Shubik 1982, 153 et seq.) -- a generalization of the core concept.

Getting Stability with a Little Help -- The ϵ-Core

If the lack of stability is a serious problem for political arrangements and if the government can affect payoffs to coalitions, can this power be utilized to secure stability? Again, consider the distributional case as an example. Imagine that we imposed a tax on coalition reformulation in the division of a dollar case. If the tax were one dollar, the first coalition to form would be stable -- any reformulation would simply imply the loss of all gains. While the prospect of a 100% tax looks artificial, the point being made is quite practical -- there is always some level of taxation which stabilizes an outcome. A more realistic task would be to find the lowest tax needed to induce a non-empty core, and this may be an important policy problem. Given a fixed level of taxation, all outcomes which are stable under it are elements of the so-called ϵ-core (see Shubik 1982 for a precise definition and various refinements). If the tax which induces a non-empty ϵ-core is small, stability can be obtained cheaply. Of course, all kinds of transaction costs, as well as other forms of government coercion, would have an effect equivalent to that of taxation. So coalitional instability can be solved by external factors like coercion. Formal analysis can thus bring a new understanding to our observation of the pervasiveness of political coercion. Coupled with the empty cores in distributional politics, it can also help us understand a bit of the correlation between redistributive and coercive political regimes.

Other Perspectives

Game theory also points to a possibility of identifying feasible outcomes to the coalitional instability problem by analyzing the coalition formation process. Note that both the core and the ϵ-core predict the stable outcome (or the lack thereof) without modeling a process

which leads to it. The "core-like" solutions were meant to identify coalitional structures with a balanced power among coalitions. Indeed, this is all they do. How a specific outcome is attained is a different type of question which requires a different line of inquiry. Such models will have to address the process of coalition formation in terms of the bargaining and the negotiations which are involved.[33]

Modeling the Process: Bargaining Sets and Competitive Solutions

Perhaps the most useful, and certainly the dean, of the solution concepts in the process oriented category, is the notion of the *bargaining set* (Aumann and Maschler 1964).[34] Aumann and Maschler conjecture that the bickering would start with an objection by one player against another coalition partner. They then ask whether the player being objected to has a compelling counter argument. To illustrate its approach we again use the example in Table 3. Consider the three-person coalition on line #1. Note that as we move to line #2 in the table, player #3 is being excluded from the coalition. Aumann and Maschler attempt to capture the coalitional bargaining which might occur *inside* the coalition, in the attempted dumping of player #3. To illustrate, let us say player #1 raises the following objection to player #3:

> Player #1 objects: I object to your being in the coalition. I could do a lot better without you getting 1/3 of the payoff. So I plan to make an offer to Player #2 that we form a coalition without you. I will offer him 50% of the dollar.
>
> Player #3 could counter: You are not in any privileged position in this argument. I can easily counter your objection. After all, I could do just as well as you by making a similar offer to Player #2, and then where would you be?

Hence the existence of a valid counter objection to player #1's objection might stabilize the situation of the equal payoff. A similar argument can be made for the coalition of line #2. In this arrangement, either player could object to their partner and propose a coalition with the third player which would improve both their payoffs. But any such objection could be met with a valid counter objection: the player being excluded in the proposal can point out that she too can make *as good an offer*, and be better off for it. Again, potential negotiations like that may make the coalition of line #2 stable. It is precisely such balance between possible objections and counter objections which motivates the concept of the bargaining set: we say that an outcome belongs to the bargaining set if all objections can be validly countered.

We can also see now which outcomes *do not* belong to the bargaining set. Consider line #3, for example. With the coalition of players #1 and #3, and the payoffs split 2/3, 1/3, not all objections can be validly countered. If player #3 objects against player #1, the dialogue would start something like this:

> Player #3: I object to your being in the coalition. I could do a lot better without you getting 2/3 of the payoff. So I think that I am going to make an offer to Player #2 that we form a coalition without you. I will offer him 50% of the dollar. That will leave me better off.

Note that player #1 cannot counter this objection; to make a similar offer to the second player, she would have to take a *cut* in her payoff. In consequence, the outcome of line #3 is not in the bargaining set. Nor is that of line #5, for similar reasons. So in these cases, where there is an equality of power between the coalition participants, only the equal payoff patterns in the coalition which forms belong to the bargaining set. More generally, the bargaining set sees stability as bought with the payoffs in a coalition balancing the power of the coalition members to form alternative partnerships.

Given an obvious relevance of the bargaining set to many political problems, the concept has been widely used. Recently, Niou and Ordeshook (1986) have used it to get leverage over the problem of alliances in maintaining a balance of power in international arenas. Earlier, Oppenheimer (1979a) suggested a model of interest group coalitions using the concept, while Schofield (1980, 1982) has used it in analyzing parliamentary coalitions and voting problems. Indeed, the scope of the applications has been quite wide.

The process of negotiations which led to the bargaining set was assumed to have occurred among individual partners of the actual or the potential coalitions. Would we, however, get the same set of outcomes if we were to assume that it is coalitions who have to bid competitively amongst themselves to recruit and retain individual members? This led to the ingenious formulation of the *competitive solution* concept with its different stability patterns (McKelvey, Ordeshook, and Winer 1978). In some studies involving experimental results as well as some with data from actual political processes, the competitive solution predicts better than the bargaining set. For example, Ordeshook (1986, 429) uses data of parliamentary coalitions in Norway to show that the competitive solution is a better predictor. In general, however, the results are ambiguous: which bargaining models do best where is still an open issue. Looking at policy outcomes in coalitional governments,

for instance, both the bargaining set and the competitive solution seem to do well (Ordeshook and Winer 1980). In addition, other considerations (such as communication patterns, equity, fairness, and universalism) seem to impinge on the performance of all the solution concepts, including the core (Miller and Oppenheimer 1982; Eavey and Miller 1984b; McKelvey and Ordeshook 1981). Indeed, it is useful to note that the permeability of all the solution concepts to external factors helps define the research frontiers of both cooperative and non-cooperative game theory.

Other Areas of Application and Success

Above, we presented the central findings of three major fields of formal theory. Yet other findings, which touch upon most corners of the discipline, abound. Although it is impossible to review them comprehensively, we can develop a casual overview by illustrating some of the more accessible applications. This yields a catalogue of some of the more important items. We begin with problems of institutional and policy design and analysis. This leads us to normative questions of political philosophy. Applications of rational choice theory to issues of policy and institutional design have been plentiful. Here we select a few works to illustrate the sort of leverage to be gained from the theory.

Institutional Design and Analysis

Affecting Outcomes with Structures

How structures or institutions affect the choices of individuals is the general question behind the institutional design problem. The problem of consolidation of urban governments serves as an early example of formal theory application to institutional design. Before the 1950s urban planners and other social scientists had serious intellectual arguments regarding the merits of consolidating urban jurisdictions. Planners, in part desiring increased simplicity of organization, advocated the unification of governments in urban areas. They saw the proliferation of municipalities as inefficient and diminishing the cities' abilities to supply services to the urban population. Commuters, as was argued, were "free riding" on the efforts of the city-dweller. Two early strands of formal theory were applied to the problem. First Tiebout (1956) showed that the proliferation of independent municipalities, differentiated by their service and tax programs, increased overall efficiency. Citizens migrate to the town which fits best their values and supplies the mix of goods they most prefer. Tiebout saw governments as competing for citizens, and this process would decrease the inherent difficulty of revelation of the citizen's demand for services and alleviate the expected problems of suboptimal supply.[35]

As newer tools of analysis were invented (specifically, Buchanan's theory of clubs, 1965), the problem was looked at anew. Using the notion of clubs, Ellickson (1973) showed that the game of urban municipality creation and population migration has a stable outcome. Ellickson concluded that the equilibrium would involve the migration of populations to and from towns until there was segregation by income. Poor people would, in the end, cluster in the low service towns. Efficiency was to be gained only at a cost of fairness in distribution (also see Miller 1981). By specifying the problem or urban consolidation in terms of individual behavior, the theorists were able to sort out both the value conflicts and the projected effects of the alternative policies.

Another area which led to accessible and interesting findings stemmed from an apparent contradiction between theory and observation. As we discussed above, voting and democratic procedures have a propensity for instability. Yet, stability is commonly observed: certainly more than one might infer. In the eyes of the casual beholder, voting cycles are not common (cf. Tullock 1967). An interesting and important line of questioning examines which of the existing institutional arrangements induce stability. Trying to discover whether stability could be predicted by institutional details, theorists focused on the structure of voting rules. Romer and Rosenthal (1978) were among the first to show this in a context of a small committee reporting to a larger voting body (i.e., a school board suggesting a policy alternative to a legislature or an electorate). The work was soon replicated, for Congress and congressional committees, by Shepsle and Weingast (1981). As a result, some important political structures were shown to be capable of inducing a non-empty core and hence an equilibrium outcome.[36] Once it became clear that predictions could be generated by focusing more on the details of the institutions, it was only natural that the concept of the core was applied to macro, as well as micro, structures. This, for example, permitted a substantial reanalysis of the relative powers of the branches of the U.S. government (e.g., Miller and Hammond 1987, 1990).

Generating Incentive Compatible Institutions

As a final design concern, note that when individuals have little incentive to reveal their preferences (see note 35), it can be non-trivial to design an institution

which generates acceptable results. Then theoretical applications may be quite practical and useful. One such case was faced by the American Public Broadcasting Network. The PBS affiliates had to express their evaluation of shows being considered for the next calendar period. The existing incentives led stations strategically to understate their evaluation of the most important and successful shows, like Sesame Street. Expecting shows like Sesame Street to gain sufficient support from others (i.e., they could get it as a free ride, without spending their "political capital"), many affiliates concentrated on preserving shows that they themselves produced, or shows which were of special interest to their region. This problem led to a temporary defeat of the most important shows and, in consequence, to the design of a less manipulable system by formal theorists brought in as consultants (see Ferejohn et al. 1979).

Public Policy Analysis and Design

Markets have two desirable properties: they are predictable and efficient, or at least more so than some of the bureaucratic and political environments. Hence, it is in general good news when markets can be created to reach policy goals which were previously sought by other means. Formal theorists have been at the forefront of designing such policy induced markets. A few exemplars of such policies have been accomplished through designs which harness the rational choice theorizing.

Developing Quasi Markets

As an example of the general problem, consider the issue of noxious industrial discharges: We can establish a market for permits of noxious substance discharges, so as to provide firms incentives to cut down their pollution levels. The design can roughly be described as follows. Instead of a "standards" policy, which sets the maximum level of emissions for each firm, the government sets the total level of the waste permitted. By distributing (or selling) a particular number of emission permits (each giving the owner a particular quantity of emissions) allowing permits to be traded, we create a market in which firms have incentives to be low polluters.[37] If, for example, a firm can decrease its emissions for less than the required cost of the permit, it can then sell any extra permits it does not need to others who have not yet efficiently decreased their emissions. Those who are least efficient at solving the pollution problem pay most. Meanwhile the government remains in full control of the market; if it wishes to "tighten up" the standard, it can regulate the quantity of permits on the market (e.g., it can repurchase and retire permits, or if permits expire annually, it can decrease the number of new permits issued). Of course, the inspections, to insure compliance, would not be different than in the previous regime.

More generally, this sort of shift in the design of an institution provides an incentive for the actor to work with the goal of the group or the society. Such policies are designed to be flexible in implementation at the individual level. They give the actor an incentive to adopt the most efficient response possible. Hence the general call for what we call *incentive compatibility* in the design of institutions. The price to be paid for the lack of such institutions may be very substantial. Plott (1983), for example, conducted an experimental study which showed very significant differences in efficiency levels and goal attainment under regulatory forms with and without incentive compatibility.

Deregulation and Efficiency

One of the most prominent economic policies in the Reagan decade was that of deregulation and for this general direction the theoretical work of Ronald Coase was not without influence. In what came to be known as the *Coase Theorem,* Coase showed (1960) that a situation involving externalities[38] does not need government intervention and can be left to the parties involved to settle; the optimal outcome would ensue as a result of bargaining. For example, a polluter in a residential area would either have to pay the damages to the community, or would receive payment from the community to stop the noxious activity. In either case, an efficient solution would occur. Who would pay whom would only depend upon the initial distribution of property rights.[39] A number of experimental studies confirmed this relationship (Hoffman and Spitzer 1986, 1982). However, Aivazian and Callen (1981) demonstrated a problem with a possible generalization of Coase's theorem. When the externality problem involves more than two parties the core may be empty and so, no stable bargain might ensue. If the bargaining cycle goes through suboptimal points, the efficiency of the outcome is threatened. Most recently, experimental work shows that Coasian solutions to cases with suboptimal points in a bargaining cycle cannot be expected to generate Paretian results (Guyton, Blake, and Leventhal 1991).

Political Philosophy

Given the leverage gained over the properties of democracy, voting, and institutional design, we would expect rational choice theory to have serious implications for political philosophy. It does.

Utilitarianism, Pareto, and Distributive Justice

The uncovering, by formal theorists, of some relationships between normative properties of collective outcomes, like Pareto optimality, and the assumptions of individual rationality was reported above. These findings are relevant to debates in political philosophy. To see this, consider that Pareto optimality is all that is left of utilitarianism if we remove the assumption of interpersonal utility comparisons (Soltan 1982). Further, Pareto optimality, all by itself, is insufficient to generate *any* distributive judgment (Sen 1973, chap. 1). We are left with the interesting conclusion that modern utilitarianism is insufficient to generate distributive judgments. Hence, the field of distributive justice must be motivated by other values.

Harsanyi (1955) was among the first to apply the tools of rational choice theory directly to problems of distributive justice. Later, however, it was mainly Rawls' work on justice which (while gaining a prominent position in political philosophy) attracted the attention of formal theorists (Rawls 1971). Two specific concerns regarding Rawls' conception of justice have produced interesting and important results. The first was the proof that his justice conditions implied a dictatorship (Plott 1978). The second line of work, by Howe and Roemer (1981), showed that only under extreme assumptions regarding risk aversion was the Rawlsian outcome, of maximizing the welfare of the worst off, stable (i.e., an element of the core).[40] Traveling down this path, we see that by taking utilitarianism, in its modern guise, and adding values regarding risk, we generate a family of possible notions of distributive justice (for a very nice exposition of this the reader may want to consult Wittman 1979). In addition to these, other notions of justice, based on yet different additions to the rationality stew, have been developed. They are discussed in some detail by Sen (1973). Finally, as came to be typical of the work in formal theory, experiments have been run to generate empirical evidence regarding what principles would be chosen behind a veil of ignorance (see Frohlich and Oppenheimer 1992).

Democracy

Given what we have said above, it may look like the lessons for democratic political philosophy are all negative. This is not quite so. Some limited, but quite attainable, positive properties of democracy have been identified. Thus, for example, if Schofield (1978) and McKelvey (1979) force us to let go of the hope that majoritarian democracy alone results in a maximization, or even an improvement in the welfare of the populace, we can still ask: "What are its other properties?" And onto this, the findings of Kramer (1977) and Miller (1980, 1983a, 1983b) cast light. Although Schofield and McKelvey show majoritarian systems can lead us to wander anywhere in the policy space, Kramer and Miller show the dynamics of the path to be less debilitating: when the trajectory of voting leads from the Pareto set, it must turn around and come back toward it.

Yet another response to the lack of central tendency in majority election research has been proposed. It is based on a different behavioral model of the voter which assumes that voter's choice is probabilistic (first advocated by Luce and Raiffa [1957, appendix] and Hinich 1977; it has now been explored more fully by Coughlin et al. 1988). The model assumes voters to have different probabilities to vote for different candidates and the probability of voting for a candidate increases as the candidate gets "closer" to the voter's ideal point. With simple assumptions regarding the function which translates distances from ideal points into probabilities of voting for alternatives, Coughlin et al. have developed powerful results. Competing candidates, each interested in maximizing the expected value of the number of votes they receive, are shown to end up in equilibrium seeking relatively "central" positions in the utility space. The resulting equilibria turn out to be not only efficient (cf. Mueller 1989, 201-203) but also surprisingly similar to the sorts of aspirations of the utilitarian philosophers (Coughlin 1984, 1988).

Liberalism and Personal Responsibility

The notions of liberalism and human rights have been important in political philosophy. And the analysis of such rights (both alienable and inalienable), and how they can conflict with other values, has been aggressively developed by formal theorists (Sen 1970a, 1970b). Take, for instance, our discussion of how structure can induce equilibria (see note ?). We reported that some institutional structures were capable of stopping the nefarious cycles. These gains, however, are often obtained by giving particular institutions (legislative committees, for example) disproportionate rights or power to design policy on specific issues. Such a devolution of decision authority can lead to suboptimal outcomes. Indeed, the general problem is identical to that of the Liberal Paradox (Sen 1970b) -- a theorem which shows that conditions of optimality and liberalism are not compatible. In other words, optimality can, at times, be attained only by constraining liberty, and preserving liberty results in a potential loss of efficiency.[41]

A final illustration concerns our image of the responsible citizen. The collective action problem was shown often to leave individuals without incentive to take political action. Unable to greatly affect the outcome,

citizens are left without incentive to stay informed regarding their political choices (Downs 1957). Such legal and philosophical concepts that link what we can hold people responsible for with their psychological state at the time of choice (e.g., *mens rea*) will be affected by showing that what we can reasonably expect from rational individuals is ignorance and lack of disposition to be involved. Hence, this generates arguments for a lessened individual ethical responsibility with regard to political matters (Oppenheimer 1985). If one can only marginally affect the outcome, then (given the modern philosophical adage "no ought without a can") the contours of the modern political philosophy scene must flow around the findings of the rational choice theories just as they flow around the findings of other modern sciences.

Concluding Comments

Our extended (though admittedly partial) tour of the findings of formal theory in political science puts the reader in a position to assess the current state of the field and some of the controversies within, and surrounding, the field. We temporize a bit about this task because controversies do get resolved and, in a developing field, the current state is passed before one can comment on it. We also offer some prognosis for the future of a science of politics and in what way formal theory conforms to the scientific standards we alluded to above.

Possible Limitations and a Prognosis for Further Contributions

If we conceptualize the field of formal theory as the combination of deductive methods and the basic conjectures of rationality, we might wonder how far such a thin reed can be pushed. Work continues at the foundational level and in expanding the range of applications. One of the healthy signs that the field will continue to grow and develop is the displayed willingness of its practitioners to explore the possibility of reformulating the foundational assumptions. These reformulation efforts have so far been pushed in two areas: choice under risk and self-interest.

Preferences, Expected Value

Over the last fifteen years or so, how people behave in the face of risk has become a topic of intense experimental research. Major, replicable discrepancies to the (von Neumann-Morgenstern) expected utility assumptions have shown up in the laboratory. These negative findings have implications, not only for the status of the expected utility theory as a valid descriptive theory of choice, but also for the behavioral consequences of many formulations of rationality. Although a number of serious theoretical reformulations have been proposed, they have proved to be neither a reasonable normative, nor a reliable descriptive, alternative to the standard expected utility theory.[42]

Self-Interest

Like the rationality conjecture, narrow self-interest certainly is part of the foundational conjectures of economics, and has stood the test of time well in market contexts. On the other hand, the display of pure self-interest seems to be context dependent. In non-market contexts, significant non-self-interested behavior such as altruism has often been observed.[43] Such displays, often thought to have some political role, call for a theoretical explanation.

The theory of rationality does not restrict the values which an individual may have and which generate his or her preferences. But many models of rational behavior do stipulate narrowly self-interested preferences, assuming that preferences of an individual are independent of the preferences of others. And to consider dropping or changing that assumption leaves us with a problem. If preferences are not independent, then what is the specific form of their interdependence? Beyond self-interest lies an infinity of alternative conjectures. Behaviorally, although at times significant, this "deviant," non-self-interested behavior has not been found to be predominant (see the experiments reported in Frohlich et al. 1984). Simple "other-regarding" alternatives to theories of self-interest, such as a specific form of altruism, have theoretical problems themselves (see Margolis 1982) even though they do cover a considerable amount of the observations of non-self-interested behavior. Further, there is some evidence that individuals who hold these other-regarding values are quite consistently acting in a rational way (Frohlich and Oppenheimer 1984; Goetze and Galderisi 1986). Trying to understand the role of altruism has, however, led to the development of both non-self-interested models and tests of such arguments.

A Cumulative Record of Contributions

Perhaps most central to the evaluation of the field should be its record in generating a trail of claims to knowledge -- claims that are made so as to be correctable. Not only is it clear that a large number of topics central to the concerns of the discipline are now theoretically "understood," but the knowledge claimed about these topics is increasing. The field of formal

theory has made indelible marks in such widely scattered areas of political interest as

A. the level of participation and information we can expect of voters,
B. the level of political organizing we can expect endogenous to a group,
C. the expectations we can have of simple democratic processes,
D. how to design better public policy and institutions,
E. how to harness the power of markets for the use of policy goals.

We have not discussed the many non-experimental, empirical studies either conducted on or informed by formal theory. This too is an important part of the record of the field. Repeatedly, formal theory models have led to better specification of statistical models, even to the point of specifying the functional form to be estimated. Many new (at times non-obvious) relationships have been uncovered through the use of formal methods. What is important, of course, is that rational choice provides a unified theory of political behavior. Applications to different areas do not require a new theory. New applications are new contexts and as such are introduced as constraints on the basic theory. Though it continues to be improved and refined, the theory remains vibrant. As it leads to more and better knowledge claims in seemingly disparate areas of political life, our confidence in the theory improves.

By now, the field of formal theory has established itself. Its accumulated knowledge claims are relevant to practitioners who would wish to study disparate areas of the discipline. Increasingly, students of politics must be able to read, understand, and even manipulate the theoretical arguments of this field to understand the substantive claims of political analysis.

A Science of Politics

We have attempted here to present formal theory in a way that would make further comment unnecessary. But in the fear we have failed, we offer the following coda. The essence of any science is the explanation of the world around us. The motivations for seeking these explanations range from simple curiosity to pragmatic considerations of how to manipulate better our surroundings. The simply curious may be more attracted to the structure of the abstractions created than the more practically oriented. Good science is rigorous in its argumentation and in its testing, because it is the rigor that is most responsible for the truth value of the theory. For practical matters, more than a bit of skepticism in matters of evidence is also well advised.

Formal theory, we have tried to indicate, has these features. The theory is constructed and interpreted to be correctable. Imprecise postulates, faulty proofs, ambiguous links between the theory and the real world all get challenged -- not on the basis that we do not like the answers so much as that we do not believe that the methods of science were used properly. After all, science is not an answer so much as it is a method of obtaining answers, tentative as they may be. In this way, science has the power to change what we believe about the world. The present authors agree on very little about the state of formal theory, or where it is going, but they do agree that rational choice theory has fundamentally changed how the discipline ought to proceed in studying politics and training students.

Notes

The authors wish to thank Michael Alberty, Norman Frohlich, John Guyton, and Kenneth Shepsle for their very timely and solid critiques of the first sketches of this paper.

1. These include John von Neumann and Oscar Morgenstern's game theory (1944), Duncan Black's rediscovery of the voter's paradox and his analysis of committees and elections (1958), Kenneth Arrow's general possibility theorem (1963), Anthony Downs's spatial model of party competition (1957), James Buchanan and Gordon Tullock's evaluation of constitutional forms (1962), and Mancur Olson's theory of collective action (1965).

2. These would include (the year in brackets is the year in which the Prize was awarded) Paul Samuelson [1970] and Kenneth Arrow [1972], who both received the Nobel Prize for a variety of contributions. Their respective contributions to the theory of social choice (see Arrow 1963, 1977) and public goods (see Samuelson 1955, 1954) were conspicuous, however. James Buchanan [1986], another Nobel laureate, was given the prize explicitly for his work in the public choice area (see Buchanan 1965, 1968; and Buchanan and Tullock 1962). Ronald Coase [1991] (see Coase 1960) and Gary Becker [1992] are the latest to receive the prize for work which most would consider in this field. Some would also include Herbert Simon [1978] and Maurice Allais [1988].

3. Other journals are devoted specifically to publishing results in formal theory. *Public Choice* (published since 1966) the pioneer journal in the field, has been joined in the last decade by *Social Choice and Welfare* (since 1981), *Rationality and Society* (since 1988), *The Journal of Theoretical Politics* (since 1988), and numerous others. Important articles in the field are scattered across journals in economics and the other social sciences, as well as a number of interdisciplinary journals. Barry and Hardin (1982, 387-390) provide a dated but useful guide to the classic readings and standard periodicals in the area.

4. Enelow and Hinich (1984) provides an introduction to spatial models of elections, Mueller (1989) gives an overview of non game-theoretic rational choice theory, Ordeshook (1986) explains game theory and how it can be used to explain political phenomena. Other texts in game theory include Fudenberg and Tirole (1991), Kreps (1990a, 1990b), Myerson (1991), Owen (1982), Rasmusen (1989), Shubik (1982), and Van Damme (1987). We should also note that the classic textbook by Luce and Raiffa (originally published in 1957) has

been brought out of retirement and republished in 1985. Undergraduate offerings in the area include Bonner (1986), Brams (1975, 1978, 1980, and 1985), Davis (1983), Elster (1989), Frohlich and Oppenheimer (1978), and Hamburger (1990). A growing number of departments have begun to include these materials in their undergraduate curriculum.

5. Some economists have regarded the fundamental assumptions about human action as non-correctable axioms protected from test (Friedman 1953; Von Mises 1957). Nagel (1963) has shown this position to be epistemologically flawed. In political science, it is common to view the fundamental propositions of theories of action as assumptions and not axioms. As a consequence these theories are regarded as falsifiable.

6. If we presume the premises to be true, we are also making the testable presumption that the conclusions are. The real strength of this property of deductive argument becomes clearer when it is properly compared with the nature of inductive arguments and statistical explanation (see Hempel 1965, 53-79, 394-406). Conclusions of inductive, statistical arguments are virtually never detachable from the informational content of the premises which are used to generate them.

7. The relationship between mathematics and logic is, itself, an interesting question. We refer the reader to Quine (1970, 98-101) for discussion of the relationship.

8. The issues we put aside here concern the presumptive nature of truth and the theory of truth that underlies efforts to test theories. White (1967) and Prior (1967) are good sources on this subject, and they have good bibliographies.

9. One of the leaders of this tradition in economics is Vernon Smith. Smith (1976) and Plott (1979) discuss the basic methodological issues surrounding experimentation.

10. Of course, some models include substantive claims about these values.

11. This notion of utility was developed by Von Neumann and Morgenstern (1944, chap. 2). Also see Luce and Raiffa (1957, chap. 2).

12. Alternative models have often been developed to deal with variations in precisely these sorts of conditions. For an example involving politicians who are motivated by policy as well as votes, see Wittman (1977).

13. Although space prohibits a full discussion of this central area of formal theory, there are a number of good texts on aggregation problems. For example, see Sen (1970a), Arrow (1977), Bonner (1986), and Schwartz (1986).

14. Our discussion focuses on majoritarian voting rules, but we should point out here that Arrow's General Possibility Theorem instructs us that this problem in aggregating preferences to form a social decision is not restricted to such rules.

15. Enelow and Hinich (1984) provide an excellent treatment of this representation of policies and preferences. We will usually further stipulate that the preferences are symmetric about the voter's ideal point. This is obviously a rather restrictive assumption, but the restriction is useful. Symmetric preferences over spatial options can be evaluated in terms of simple Euclidean distances.

16. A Paretian outcome is one from which no improvements can be made for some people without hurting others. In other words, if x is a Paretian outcome, then even if there are some people who prefer y, others prefer x to y and would be against such a move.

17. Unequally weighted distances would be important in many real-world applications, such as the evaluation of the siting of point-sources of air pollution given prevailing winds, etc. Extensions of the material in this manner are straightforward (see Enelow and Hinich 1984).

18. That the contract curve in this example is a straight line is an artifact of the fact that we have assumed circular indifference sets.

19. If the rule were unanimity, the Pareto set, points in and on the triangle, would constitute a core. Everyone in the group agrees that points in the triangular area are better than those outside it, and no movement from a point within it is unanimously supported.

20. An interesting, even amusing, study of the impressive power of a chairperson in a small club is reported in Levine and Plott (1977).

21. Schwartz (1981) proves that an element of coercion may be relevant for the stabilization of all trades.

22. In reality there often is a very thin line between cooperative and non-cooperative situations. Thus the distinction between the two branches of game theory remains fuzzy. One attempt to get around the problem, called the Nash program, was to reformulate all of game theory as a theory of non-cooperative games. While, in principle the reformulation is possible, in most cases it complicates rather than clarifies the problem.

23. In greater detail: two suspects for a major crime are taken into custody and separated. The police have enough evidence to convict them on a minor offense. The district attorney, believing correctly that they have committed the serious crime, lacks the evidence necessary to convict them. To strengthen her case, she offers each of the suspects: defect from your partner and confess, and we will be lenient. If you confess and your partner does not, you will be released (the best outcome -- the Temptation payoff: T) and your partner will get a maximum sentence (the worst outcome -- the Sucker's payoff: S); if the two of you continue to cooperate with each other, and neither of you confesses, you will both be convicted of the minor crime (the second best outcome -- the Reward payoff: R); if you both confess, you will both go to jail but the judge will be lenient (the third best outcome -- the Punishment payoff: P). What we have leads us to postulate that both prisoners (*i* and *j*) share preferences such that: $T >_{i,j} R >_{i,j} P >_{i,j} S$. This ordering of payoffs, the same for the two players, defines what is called a one-shot two-person PD game. Note that a different order of payoffs can be reasonably postulated (e.g., if not only jail terms but other factors matter). If this were the case, the game depicted would no longer be the PD (Rapoport 1992; Swistak 1992).

24. The PD is developed as a cooperative game in Ordeshook (1986, chaps. 7 and 8). The conclusion that the cooperative outcome is the sole element of the core requires an additional constraint: $T + S <_{i,j} 2R$, often included in the characterization of the PD.

25. For example, that neither information nor communication conditions matter when you play the PD game. Whether you know the payoffs to the other player, or not, you will defect. (Knowing your own payoffs is sufficient.) Similarly, players may negotiate, threaten, and promise, but if no binding commitments are possible, when it comes to the actual decision, no one has an incentive to keep their promise.

26. If we consider the Nash equilibrium as a reference point to compare with actual behavior, it often fails to predict accurately. A few reasons account for most of its failures. Two follow straight from the definition of the Nash solution: players fail to predict correctly, or they fail to maximize with respect to a prediction. Observations of these deviations have led to new directions of research and brought about many new solution concepts. The most important of these are reviewed and developed at length in any of the newer textbooks on game theory (see note ?). The analysis of these developments in non-cooperative game theory goes beyond the scope of the current essay. We should also note that there are justifications of Nash's notion which are not based on the correct prediction of the opponent's strategy. Concepts like minimax introduce some other constraints which get around the requirement of prediction as an essential aspect of rational calculation (see Frohlich and Oppenheimer 1978, chap. 3). Of course, Nash can fail as a predictor if a game, modeled as a non-cooperative situation, turns out to be a cooperative one. Then, as was the case with PD, a different outcome may result from the non-cooperative and the cooperative analyses.

27. These are often referred to as infinite-horizon games. They are modeled as infinitely repeated games with a "weighting"

parameter on future payoffs which may be interpreted as a probability of continuing the game in the next period, a discount parameter on future payoffs, or a parameter reflecting a joint effect of both.

28. If we define the "amount of cooperation" as an expected frequency of cooperative moves in a game, the folk theorems essentially state that any percent of cooperative moves, from 0% to 100%, can be attained in equilibrium. Folk Theorems derive their name from the fact that many game theorists believed them to be true before any formal proofs were published. They are reported to have been known (Aumann 1987) since the late 1950s or early 1960s. The plural form "theorems" is often used to indicate that the same proliferation of equilibria effects holds under various assumptions (e.g., Rubinstein 1979; and Fudenberg and Maskin 1986).

29. Alternative routes to a similar conclusion regarding the collective action problem can be seen in Hardin (1982) and Taylor (1987).

30. It should be noted that when we remove uncertainty about the future interactions and the exact termination period becomes known to both players, the possibility of cooperation is lost again. Mutual defections in all periods is the only equilibrium outcome of the game (cf. Luce and Raiffa 1957). The proof is based on the so-called backward induction argument, which hinges on a very demanding assumption of "common knowledge" (I know that he knows that I know that he knows ....). (Results related to common knowledge may be among the most important and consequential recent developments in game theory. The first formulation of the "common knowledge" concept is due to Lewis (1969). The first formalization was given by Aumann (1976).) While this deductive result was consistently in contrast with experimental observations, some recent theoretical results shed light on this discrepancy. In general, the "defect in all periods" solution turns out to be very vulnerable to small changes in the assumptions of the model. If, for example, the game is known to be finite but players do not know which period will be the last, the cooperative outcome is again a possible equilibrium (Basu 1987). Cooperation can also arise as an epsilon-equilibrium (Radner 1980; Fudenberg and Levine 1986) and as a result of a bounded complexity of strategies which players use (Neyman 1985). In short, the bad news here turned out not to be robust. The unique deficient equilibrium does not hold under small changes in the model.

31. To see how rare is the existence of the core, i.e., how demanding is the stability condition, consider a more general, yet still simple to analyze, class of games -- the so-called symmetric games. A game is called symmetric if the payoff to a coalition depends only on its size. This means that the payoff to all different but equal-size (e.g., two-person) coalitions is the same. In a symmetric game the core exists if, and only if, the payoff to the individuals in the equal share outcome is larger in the coalition of all than it is in any smaller coalition (Shubik 1982, 147). In other words, the core exists only if the coalition of all has the maximal per capita payoffs. For example, for any symmetric three-person game to have a core, a two-person coalition can get less than 2/3 of the payoff to the coalition of all three players. This is a very severe constraint on the payoffs. It is obviously not met in purely distributional games like the one in our example. The fact that the empty core shows up in all zero sum games, and in all symmetric games of the sort noted, sheds light on some more general mechanisms behind the difficulty with voting (i.e., that the problem does not stem from the spatial analogues). This, in turn, allows us to understand more fully the breadth and depth of what has come to be called the Arrow problem (again, see Shubik, *op. cit.*, for an interesting discussion).

32. Von Neumann and Morgenstern (1944, 260 et seq.), anticipating the problem of an empty core, adopted the so-called "stable set" as a solution concept in cooperative games. While stable sets usually exist, their problem is one of abundance, not scarcity: typically a game has many stable sets and, hence, the concept serves as a "weak" predictor of the outcome. Stable sets are often more useful for the purpose of seeing which outcomes will *not* be obtained. In the divide-a-dollar game example, (1/2,1/2,0), (1/2,0,1/2), (0,1/2,1/2) are the only elements of the stable set. The reader who is intrigued why (1/3,1/3,1/3) is not predicted as a feasible outcome should consult any of the textbooks in the area for the definition and the discussion of this important solution concept (in Shubik 1982, cf. 157 et seq.).

33. One other important line of research was proposed by Shapley (1953), and is typically referred to as the Shapley value. It associates a unique payoff structure with every cooperative game. All the major value solutions are based on considerations of weighting and combining the values of possible outcomes.

34. Although Aumann and Maschler introduced this solution concept, they did not use it to predict which coalitions would form. Rather, they tried to predict which payoff structures would stabilize each of the possible coalitions. It took later work (Peleg 1963) to note how this can be extended to predict coalition formation. We should also note that the bargaining set is often large; such is the case in our example. Consequently, some refinements of the bargaining set have been developed. These include the kernel (a subset of the bargaining set), and the nucleolus (a single point in the kernel).

35. It should be noted that revelation of an individual's demand for a public good is a major problem: in general, individuals do not have an incentive to reveal their true preferences. When a person is thirsty and can buy a Coke, her choice to purchase the Coke is a revelation of her demand for the good. The same is not true, however, for public goods. The lack of voting for a candidate, for example, does not have to reflect the lack of desire for the candidate's victory; the voter might have simply conjectured that her preferred candidate will win regardless of her vote. In general, Samuelson (1954) argued that there was an inherent problem in getting a truthful or undistorted measure of individual demand for a public good. This problem was greatly embellished by a number of authors who were quite successful in inventing partial solutions (cf. Mueller 1989, chap. 8).

36. Such structurally induced outcomes are constrained by the status quo and the sets of preferences of the members of both groups. Formal analyses like the ones cited help to explain why membership on committees tends to be by those with the more intense interest in the issues governed by the committees. In experiments, Eavey and Miller (1984a) showed that the predicted distortion (toward the median voter of the committee and away from the median of the legislature) might not be as pronounced when there is solid bargaining between the two bodies.

37. Since some of the policies of the U.S. and foreign governments are now built on these ideas, discussion is often available in the popular press; see Weisskopf (1989). Some early work on the logical structure of the problem of using charges to manipulate environmental quality can be found in Kneese and Bower (1968, Part II).

38. Externalities are effects of market transactions which are not reflected in the market price. As such they can often be thought of as public goods.

39. Coase's theorem works under the assumption of negligible bargaining costs. It is often emphasized that "negligible bargaining costs" mean that Coase's theorem does not imply massive deregulation. But at least when the parties could, and would, be compensated for the costs of bargaining, and when these costs were less than those of regulation, Coase's result does imply deregulation.

40. It should be noted that any deductive proofs involving Rawls' conditions require specific formalization of his normative conditions. Since Rawls' own argument is a bit verbose, it cannot be known whether his argument is fully consistent with its formalization by Plott or Howe and Roemer. Regarding results on the attitudes towards risk, it is interesting to note that the core was shown to include Harsanyi's (1955) notions of maximizing the average when there was no risk aversion among the individuals.

41. The dilemma here may sound similar to the one in the Prisoners' Dilemma game. There too, the fact that each individual had

the right to choose led to the suboptimal result. Indeed, Bernholz (1976) and Miller (1977) show Prisoners' Dilemma to be a type of liberal paradox -- yet another unexpected connection in the complex labyrinth of results discussed here (also see the more general argument in Breyer and Gardner 1980).

42. Some major discrepancies between the axioms of the expected utility theory and the experimental data were found quite early (1950s and 60s). These early findings are usually associated with the so-called Ellsberg and Allais Paradoxes (see Schoemaker 1982 for an excellent review). A host of experimental studies followed in the 1970s and 80s (e.g., Lichtenstein and Slovik 1971; Grether and Plott 1979). Popular treatments of this technical material are available in Quattrone and Tversky (1988), Kahneman and Tversky (1982), and Tversky and Kahneman (1981). For some alternative directions of research see Kahneman and Tversky (1979), Machina (1989), Cox and Epstein (1987), and Holt (1986).

43. Experimental results in this area are varied. The main context in which a residual of non-self-interested behavior has continually shown up is that of Prisoners' Dilemma experiments (cf. Marwell and Ames 1981). But dissents are heard on this in later, better controlled experiments by Isaac et. al (1984 and 1985) and most recently, by Iwakura and Saijo (1992). Other studies like McKelvey and Palfrey (1991), show that in some non-market contexts one can generate a great deal of altruism.

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5

The Comparative Method

David Collier

Comparison is a fundamental tool of analysis. It sharpens our power of description, and plays a central role in concept-formation by bringing into focus suggestive similarities and contrasts among cases. Comparison is routinely used in testing hypotheses, and it can contribute to the inductive discovery of new hypotheses and to theory-building.

The forms of comparison employed in the discipline of political science vary widely and include those contained in statistical analysis, experimental research, and historical studies. At the same time, the label "comparative method" has a standard meaning within the discipline and in the social sciences more broadly: it refers to the methodological issues that arise in the systematic analysis of a small number of cases, or a "small N."[1] This chapter examines alternative perspectives on the comparative method that have emerged over roughly the past two decades. Although the primary focus is on discussions located in the fields of comparative politics and international studies, the application of the comparative method is by no means restricted to those fields.

The decision to analyze only a few cases is strongly influenced by the types of political phenomena under study and how they are conceptualized. Topics for which it is productive to examine relatively few cases include revolutions, particular types of national political regimes (e.g., post-communist regimes), or particular forms of urban political systems. This focus on a small number of cases is adopted because there exist relatively few instances of the phenomenon under consideration that exhibit the attributes of interest to the analyst. Alternatively, some analysts believe that political phenomena in general are best understood through the careful examination of a small number of cases. In the field of comparative and international studies, the practice of focusing on few cases has achieved greater legitimacy in recent years in conjunction with the rise of the school of "comparative historical analysis," in which small numbers of countries are studied over long periods. This close scrutiny of each country limits the number of national cases a scholar can consider.[2]

Choosing to study few cases routinely poses the problem of having more rival explanations to assess than cases to observe, or the quandary of "many variables, small N" (Lijphart 1971, 686). Elementary statistics teaches us that as the number of explanatory factors approaches the number of cases, the capacity to adjudicate among the explanations through statistical comparison rapidly diminishes. This problem has stimulated much discussion of how most productively to analyze a small N.

The late 1960s and early 1970s saw a boom in writing on comparative method (e.g., Merritt and Rokkan 1966; Kalleberg 1966; Verba 1967; Smelser 1968; Lasswell 1968; Przeworski and Teune 1970; Sartori 1970; Merritt 1970; Etzioni and Dubow 1970; Lijphart 1971; Vallier 1971; Zelditch 1971; Armer and Grimshaw 1973). This literature established a set of norms and practices for small-N research, proposed alternative strategies for conducting such analyses, and created a base line of understanding that has played an important role in the ongoing practice of small-N studies. This chapter assesses the issues of comparative method that have been debated in the intervening years and considers their implications for ongoing research. The point of departure is Arend Lijphart's (1971) article "Comparative Politics and Comparative Method." Among the studies published in that period, Lijphart's piece stands out for its imaginative synthesis of basic issues of comparison and of the relation between comparative method and other branches of methodology.[3] It therefore provides a helpful framework for examining, and building upon, new developments in the field.

A central theme that emerges in the discussion below is that refinements in methods of small-N analysis have substantially broadened the range of techniques available to comparative researchers. The most fruitful approach is eclectic, one in which scholars are willing and able to draw upon these diverse techniques.

Synopsis of Lijphart

Lijphart defines the comparative method as the analysis of a small number of cases, entailing at least two observations, yet too few to permit the application of conventional statistical analysis. A central goal of his article is to assess the comparative method in relation to three other methods--experimental, statistical, and case-study--and to evaluate these different approaches by two criteria: 1) how well they achieve the goal of testing theory through adjudicating among rival explanations, and 2) how difficult it is to acquire the data needed to employ each method (see Figure 1).

The experimental method has the merit of providing strong criteria for eliminating rival explanations through experimental control, but unfortunately it is impossible to generate appropriate experimental data for most topics relevant to political analysis. The statistical method has the merit of assessing rival explanations through the weaker but still valuable procedure of statistical control, but it is often not feasible to collect a sufficiently large set of reliable data to do this form of analysis.

The case-study method has the merit of providing a framework in which a scholar with modest time and resources can generate what may potentially be useful data on a particular case. Unfortunately, opportunities for systematically testing hypotheses are far more limited than with the other methods. Yet Lijphart (pp. 691-93) insists that case studies do make a contribution to testing hypotheses and building theory, and he offers a suggestive typology of case studies based on the nature of this contribution. He distinguishes among *atheoretical* case studies; *interpretative* case studies (that self-consciously use a theory to illuminate a particular case); *hypothesis-generating* case studies; *theory-confirming* case studies; *theory-infirming* case studies (that, although they cannot by themselves disconfirm a theory, can raise doubts about it); and *deviant case* analyses (that seek to elaborate and refine theory through a close examination of a case that departs from the predictions of an established theory). Lijphart emphasizes that "certain types of case studies can even be considered implicit parts of the comparative method" (p. 691), and to the extent that the assessment of hypotheses does occur in some case studies, it is often because the case studies are placed in an implicit or explicit comparative framework. Yet even within this framework, he emphasizes that findings from a single case should not be given much weight in the evaluation of hypotheses and theory (p. 691).

The comparative method, as defined by Lijphart, has an intermediate status in terms of both his criteria. It provides a weaker basis than the experimental or statistical method for evaluating hypotheses, due to the lack of experimental control and the problem of many variables, small N. Yet it does offer a stronger basis for evaluating hypotheses than do case studies. Despite the constraint of addressing more variables than cases, the comparative method allows systematic comparison that, if appropriately utilized, can contribute to adjudicating among rival explanations.

Although the data requirements of the comparative method may be much greater than for case studies, Lijphart argues that they are less demanding than for experimental or statistical research. He therefore views the comparative method as most appropriate in research based on modest resources, and he suggests that studies using the comparative method might often serve as a first step toward statistical analysis.

> If at all possible one should generally use the statistical (or perhaps even the experimental) method instead of the weaker comparative method. But often, given the inevitable scarcity of time, energy, and financial resources, the intensive comparative analysis of a few cases may be more promising than a more superficial statistical analysis of many cases. In such a situation, the most fruitful approach would be to regard the comparative analysis as the first stage of research, in which hypotheses are carefully formulated, and the statistical analysis as the second stage, in which these hypotheses are tested in as large a sample as possible. (1971, 685)

Lijphart also proposes solutions to both sides of the problem of many variables, small N (1971, 686 ff). With regard to the small number of cases, even if researchers stop short of a statistical study, they can nonetheless try to increase the number of cases used in assessing hypotheses. With regard to the large number of variables, he suggests two approaches. First, analysts can focus on "comparable cases," that is, on cases that a) are matched on many variables that are *not* central to the study, thus in effect "controlling" for these variables; and b) differ in terms of the key variables that *are* the focus of analysis, thereby allowing a more adequate assessment of their influence. Hence, the selection of cases acts as a partial substitute for statistical or experimental control. Second, analysts can reduce the number of variables either by combining variables in a single scale or through theoretical parsimony, that is, through developing a theory that focuses on a smaller number of explanatory factors.

Thus, Lijphart provides a compact formulation of the relationship between the comparative method and

Figure 1. Situating the Comparative Method as of 1971: Lijphart's Scheme

Case Study Method

Merit: Permits intensive examination of cases even with limited resources

Inherent Problem: Contributes less to building theory than studies with more cases

Types of Case Studies:

1. Atheoretical
2. Interpretive
3. Hypothesis-generating
4. Theory-confirming
5. Theory-infirming (i.e., case studies that weaken a theory marginally)
6. Deviant case studies

Comparative Method

Defined as: Systematic analysis of small number of cases ("small-*N*" analysis)

Merit: "Given inevitable scarcity of time, energy, and financial resources, the intensive analysis of a few cases may be more promising than the superficial statistical analysis of many cases" (Lijphart, p. 685)

Inherent Problem: Weak capacity to sort out rival explanations, specifically, the problem of "many variables, few cases"

Potential Solutions:

1. Increase number of cases
2. Focus on comparable cases
3. Reduce number of variables
   a. Combine variables
   b. Employ more parsimonious theory

Experimental Method

Merit: Eliminates rival explanations through experimental control

Inherent Problem: Experimental control is impossible for many or most topics of relevance to field of comparative politics

Statistical Method

Merit: Assesses rival explanations through statistical control

Inherent Problem: Difficult to collect adequate information in a sufficient number of cases, due to limited time and resources

other methodologies, and he offers solutions to the characteristic dilemmas of the comparative method.

Further Perspectives on Small-N Analysis

The two decades following Lijphart's study have seen the emergence of new perspectives on small-N analysis, as well as a renewed focus on methodological alternatives already available before he wrote his article. Though many of these innovations appear in work explicitly concerned with the comparative method, conventionally understood, others appear in writing on the experimental, statistical, and case-study methods. The result has been an intellectual cross-fertilization of great benefit to the comparative method. Figure 2 provides an overview of these innovations.

Innovations in the Comparative Method

Innovations in the comparative method can be discussed in terms of the issues introduced above, encompassing the goals of comparison, the justification for focusing on few cases, and the problem of many variables, small N.

Goals of Comparison

A central and legitimate goal of comparative analysis is assessing rival explanations. However, as Theda Skocpol and Margaret Somers (1980) argue, comparative studies should be understood not merely in terms of this single goal, but in terms of three distinct, yet ultimately connected, goals.[4] The first is that considered above: the systematic examination of covariation among cases for the purpose of *causal analysis*.[5] The second is the examination of a number of cases with the goal of showing that a particular model or set of concepts usefully illuminates these cases. No real test of the theory occurs, but rather the goal is the *parallel demonstration of theory*. This use of comparison plays an important role in the process through which theories are developed. The third type of comparison is the examination of two or more cases in order to highlight how different they are, thus establishing a framework for interpreting how parallel processes of change are played out in different ways within each context. This *contrast of contexts* is central to the more "interpretive" side of the social sciences and reflects yet another way that comparison is frequently used.

In addition to providing a more multifaceted account of the goals of comparison, Skocpol and Somers suggest the intriguing idea of a research "cycle" among these approaches (pp. 196-197). This cycle arises in response to the problems that emerge as scholars push each approach up to -- or beyond -- the limits of its usefulness. For example, a "parallel demonstration" scholar might introduce a new theory and show how it applies to many cases. "Hypothesis-testing" scholars, wanting to specify the conditions under which the theory does not hold, could make further comparisons with the goal of discovering these conditions. Hypothesis-testing studies that too brashly compare cases that are profoundly different might, in turn, stimulate "contrast of contexts" scholars to examine more carefully the meaning of the differences among the cases. It is thus useful to look beyond an exclusive focus on the role of comparison in broad causal analysis, to an understanding that encompasses the different elements in this research cycle.

This is not to say that assessing hypotheses does not remain a paramount goal of comparison, and many scholars insist that it is the paramount goal. Yet this broader perspective offers a valuable account of how comparative work proceeds within a larger research community, pointing usefully to the interaction among different goals of comparison.

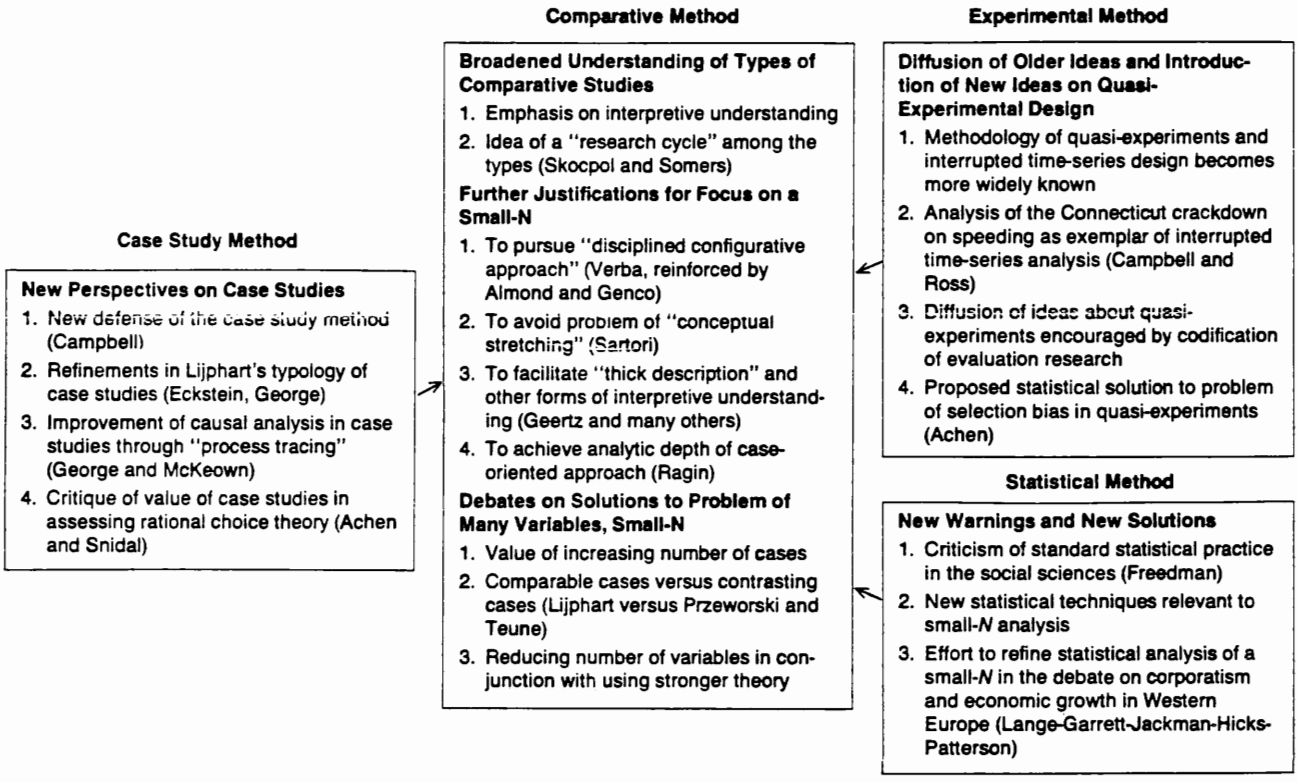
Justification for Small N

A second trend is toward a more elaborate justification of a focus on relatively few cases. Lijphart's rationale seems in retrospect rather modest, in that it emphasizes only the problem of inadequate resources and treats the small-N comparison as a way station on the route to more sophisticated statistical analysis.

A very different defense of working either with a small N or with case studies had previously been available in arguments favoring a "configurative" approach (Heckscher 1957, 46-51, 85-107), and this perspective was elaborated a few years before the publication of Lijphart's analysis in Sidney Verba's (1967) review essay advocating the "disciplined configurative approach." In evaluating Robert A. Dahl's *Political Oppositions in Western Democracies* (1966), Verba points both to the sophistication of the hypotheses entertained in the book and to the difficulty of assessing them adequately, except through a close command of the cases, leading him to advocate this disciplined configurative mode of research. Verba's formulation is appealing because he is concerned with systematic hypothesis testing and theory building. At the same time, he links this priority with a more explicit appreciation of the difficulty of testing hypotheses adequately and the value of properly executed case studies in providing subtle assessments of hypotheses.

It might be claimed that the difficulty of adequately testing hypotheses ultimately derives from the

Figure 2. Innovations Relevant to the Comparative Method



problem of limited resources discussed by Lijphart. If enough talented researchers worked long and hard, they could carry out a *Political Oppositions* study for many dozens of countries. Yet the problem here is somewhat different from that emphasized in Lijphart's initial formulation. It is not so much that resources are limited, but that constructing adequate comparisons has proved more difficult than had often been thought in the 1960s and early 1970s, in the initial days of enthusiasm for comparative statistical research. Among these difficulties, that of the valid application of concepts across diverse contexts has been especially vexing.

Within the literature on comparative method, a key step in elucidating these problems of validity, and thereby strengthening the justification for a small N, is Giovanni Sartori's (1970) classic discussion of "Concept Misformation in Comparative Politics," the basic themes of which are elaborated in his later book *Social Science Concepts* (1984). Sartori suggests that the application of a concept to a broader range of cases can lead to conceptual "stretching," as some of the meanings associated with the concept fail to fit the new cases. The concepts that can most easily be applied to a broad range of cases are often so general that they do not bring into focus the similarities and contrasts among cases that are essential building blocks in worthwhile comparative analysis. Consequently, a study focused on concepts that are carefully adapted to this "finer slicing" of a given set of cases should be extended to other cases only with great caution. From this perspective, it may be argued that the most interesting studies will often be those that focus on a smaller number of cases.

With regard to the problems of increasing the number of cases under study,[6] Adam Przeworski and Henry Teune's *The Logic of Comparative Social Inquiry* (1970) is a major source of insight. Although they argue that achieving a high level of generality should be a basic goal of social science, their framework is centrally concerned with the difficulties that can arise in generalizing beyond an initial set of cases. With regard to problems of validity, they advocate the use, when necessary, of "system-specific" indicators that serve to operationalize the *same* concept in *distinct* ways in different contexts (pp. 124-130). For the scholar seeking to move toward a larger set of cases, the potential need for system-specific indicators necessitates the close examination of every new case.

Przeworski and Teune also address the problem that as the analyst incorporates more cases into a study, distinct causal patterns may appear in the new cases. To deal with this problem, Przeworski and Teune advocate "replacing proper names" of social systems by identifying those systems in terms of the explanatory factors that account for why causal relations take a particular form within each system (pp. 26-30).[7] This approach makes the invaluable contribution of providing a theoretical, rather than an idiosyncratic and case-specific, basis for analyzing differences in causal patterns. However, extending an analysis to additional cases on the basis of this procedure again requires a painstaking assessment of each new context. Thus, Przeworski and Teune provide a valuable tool for adequately analyzing a larger number of cases, but their approach again shows that this must be done with caution.

Since 1970, the renewal of a Weberian concern with interpretive understanding, i.e., with deciphering the meaning of behavior and institutions to the actors involved, has also strengthened the justification for advancing cautiously with one or very few cases. Clifford Geertz's (1973) label "thick description" is commonly evoked to refer to this concern,[8] and this focus has appeared in many guises relevant to political research, including Gabriel Almond and Stephen J. Genco's analysis of "Clouds and Clocks" (1977) and Skocpol and Somers' "contrast of contexts" approach, which encompasses studies that use comparison to richly contextualize research findings. Charles C. Ragin's *The Comparative Method* (1987) explores another facet of this concern in his analysis of the "holistic" orientation of what he calls "case-oriented" research and the complex problems of "conjunctural causation" -- that is, causal patterns that vary according to the context -- to which configurative scholars are typically far more sensitive.

Finally, the intellectual success in recent years of the school of comparative historical analysis has played an important role in legitimating a focus on a small N. This approach was pioneered in works such as Reinhard Bendix (1964), Barrington Moore (1966), and Lipset and Rokkan (1967), and more recent works include Rokkan (1970), Tilly (1975), Paige (1975), Bendix (1978), Trimberger (1978), Skocpol (1979), Bergquist (1986), Luebbert (1991), Goldstone (1991), Collier and Collier (1991), and Rueschemeyer, Stephens, and Stephens (1992). Methodological statements focused on this tradition include Skocpol and Somers (1980), Skocpol (1984), Tilly (1984), and Ragin (1987).

The particular form of analysis in these studies varies considerably, as suggested by Skocpol and Somers' typology noted above. In varying combinations, these studies employ both rigorous qualitative comparisons that extend across a number of nations, and also historical analysis that often evaluates each national case over a number of time periods.[9] This tradition of research thus combines well-thought-out comparison with an appreciation of historical context, thereby contributing to an effort to "historicize" the social sciences.

Although the uses of comparison in this literature are diverse, as Skocpol and Somers emphasized, it may

be argued that a major consequence of the growing importance of comparative historical studies is to further legitimate the approach that was Lijphart's original concern: the assessment of rival explanations, based on systematic, qualitative comparison of a small number of cases. In light of a spectrum of studies from Barrington Moore's (1966) pioneering analysis of the emergence of alternative forms of modern regimes, to Skocpol's (1979) study of revolution, to Luebbert's (1991) analysis of the emergence of liberalism, fascism, and social democracy in interwar Europe, it is evident that this literature has given new legitimacy to the use of broad historical comparison for systematic causal analysis. Efforts to codify procedures for assessing hypotheses in this type of analysis, such as that in Ragin's *Comparative Method* (1987), further reinforce the plausibility of insisting on the viability of small-N analysis as a middle ground between case studies and statistical studies.

Solutions to the Problem of Many Variables, Small N

The evolving debates on comparative method have suggested further refinements in Lijphart's original three solutions for the problem of many variables, small N, i.e.: 1) increasing the number of cases, 2) focusing on matched cases, and 3) reducing the number of variables.

1. *Increase the Number of Cases* At the time Lijphart wrote, it was believed in some circles that comparative social science would increasingly be oriented toward large-N comparative studies, based on extensive quantitative data sets and rigorous statistical analysis. Today there can be no question that, for better or worse, quantitative cross-national research in the subfield of comparative politics, and quantitative international politics in the subfield of international relations, have not come to occupy as dominant a position as many had expected. Within these two subfields, they hold the status of one approach among many.

Various factors have placed limits on the success of large-N research based on quantitative data sets, among which is certainly the renewed concern with closely contextualized analysis and interpretive studies. Broad quantitative comparison may have been set back as many scholars discovered how extraordinarily time-consuming it is to construct appropriate data sets, often out of proportion to the professional rewards that seem to be forthcoming. This is particularly a problem when the focus of analysis extends beyond the advanced industrial countries to regions for which it is often extremely difficult to develop reliable data. In addition, the quantitative-comparative approach has probably been hurt by the publication of too many studies in which concepts are operationalized with dubious validity and which employ causal tests that are weak, unconvincing, or inappropriate (Ragin 1987, chap. 4).

Yet the fact that broad quantitative comparison has not become a predominant approach should not lead scholars to overlook what has been accomplished. Robert Jackman (1985) insists that comparative statistical research has had more success than is recognized, and Lijphart's own recent work moves in this direction (1990). The failure to seize good opportunities to do quantitative research could certainly be viewed as being as much of a mistake as premature quantification, and the fruitful debate on corporatism and economic growth in Western Europe discussed below is one of many examples of how statistical methods can effectively address interesting analytic issues. Further, the availability of new statistical techniques (also discussed below) has made it far more productive to do quantitative analyses with as few as ten to fifteen cases. Consequently, the option of increasing the "N" at least to that level is still worth pursuing, and it should probably be pursued more often.

2. *Focus on Comparable Cases* The recommendation that analysts focus on carefully matched cases has been both reinforced and challenged. In a discussion published in the mid-1970s, Lijphart (1975) explores further the trade-off he noted in 1971 between the goal of increasing the number of cases and the goal of matching cases as a substitute for statistical control. Obviously, if a researcher is to select cases that are really similar, however that similarity is defined, the number of appropriate cases is likely to become limited. In the face of this trade-off, Lijphart opts in favor of the more careful matching of fewer cases, and he goes so far as to restrict the application of the term "comparative method" to analyses that focus on a small number of carefully matched cases. This emphasis parallels a much earlier perspective on the comparative method referred to as the method of "controlled comparison" (Eggan 1954). Arthur Stinchcombe's (1978) advocacy of the methodology of "deep analogy," i.e., the comparative analysis of very few, extremely closely matched, cases pushes this approach even further.

A contrasting strategy is advocated by Przeworski and Teune (1970, 32-39) and Przeworski (1987, 38-41). They suggest that even with careful matching of cases in what they label a "most similar" systems design, there remains a problem of "overdetermination," in that this design fails to eliminate many rival explanations, leaving the researcher with no criteria for choosing among them. They prefer instead a "most different" systems design, based on a set of cases which are highly diverse and among which the analyst traces similar processes of change.[10] Przeworski suggests that the strength of this design is in part

responsible for important advances in the literature on democratization, such as the work of O'Donnell, Schmitter, and Whitehead (1986). Przeworski maintains that this literature addresses such a broad range of cases that analysts are forced to distill out of that diversity a set of common elements that prove to have great explanatory power.[11]

This discussion can be placed in perspective by recognizing that cases that are closely matched from one point of view may contrast sharply from another. My own recent work (Collier and Collier 1991) combines the two strategies by starting with a set of eight Latin American countries that are roughly matched on a number of broad dimensions. Among the eight countries, the analysis focuses on pairs of countries that are nonetheless markedly different. The overall matching assures that the contexts of analysis are analytically equivalent, at least to a significant degree, and the paired comparison places parallel processes of change in sharp relief because they are operating in settings that are very different in many respects.

In conjunction with the debate over the merits of most similar and most different systems designs, it is important to recognize that in many studies, the conclusions reached in the overall comparison of cases are also assessed -- implicitly and sometimes explicitly -- through within-case analysis. In the section on case studies below, the discussion of "pattern matching" and "process tracing" suggests some of the forms this takes. It is no coincidence that within the school of comparative historical analysis, findings are often reported in books, rather than articles. Part of the reason is that the presentation of detailed information on each case serves to further validate the conclusions drawn from comparisons across cases.

These within-case comparisons are critical to the viability of small-N analysis. As Stanley Lieberson (1991, 312-315) has correctly insisted, taken by themselves, comparisons across a small number of cases, using either a most similar or a most different systems design, provide a weak basis for causal inference. However, if one considers the role of these internal comparisons, the "N" is substantially increased, thereby strengthening causal analysis.[12]

This use of within-case comparison can also help protect the analyst from a problem that arises in the most different systems design, in which countries are matched on the dependent variable and differ in terms of a series of background variables. Barbara Geddes (1990) has shown that if cases are selected on the basis of scores on the dependent variable, which is how most different systems designs are often carried out, the lack of variance on the outcome to be explained introduces a "selection bias" that can greatly weaken causal inference. One way of mitigating this problem is to introduce greater variability through internal comparison.

The ongoing debate on most similar versus most different systems designs has implications for the status of area studies. Dankwart Rustow (1968) argued some time ago in favor of moving beyond an area studies approach, and many scholars agree that cases should be selected in response to the analytic requirements of particular research projects, rather than on the basis of a geographic proximity that at best is often a poor substitute for the analytic matching of cases. Recent "cross-area" studies on successful export-led growth and on democracy suggest that this alternative perspective is gaining ground.[13]

However, the area studies approach is a booming business today for a variety of reasons, including the impressive funding of area studies by U.S. foundations in recent decades, as well as institutional momentum. In fact, from the point of view of the theoretically oriented small-N comparativist, this is not a bad outcome. The country case studies produced by area specialists are crucial building blocks in most comparative work, and without them cross-area studies would be on far weaker ground. It is essential to recognize that these case studies benefit greatly from the intellectual leverage gained when individual scholars develop, over many years, a cumulative and well-contextualized understanding of a particular region. Particularly in light of current concerns that broad comparative studies should be attentive to the context of analysis, the contribution of area specialists is essential.

3. *Reduce the Number of Variables*: The third solution to the small-N problem is to reduce the number of explanatory factors, either through combining variables, sometimes referred to as "data reduction," or through employing a theoretical perspective that focuses on a smaller set of explanatory factors. One of the promising sources of parsimonious explanatory theory is the "rational choice" approach that has gained increasing attention among political scientists. Rational choice modeling offers a productive means of simplifying arguments that contain a multitude of interesting variables, but that may fail to specify the most critical ones. Within the field of comparative analysis, Geddes's (1991) study of administrative reform in Latin America, which models the impact of different electoral and party systems on the incentives of legislators to adopt reform, provides an excellent example of a productive simplification of a complex topic. As such models gain increasing acceptance in the comparative field, analysts will acquire a useful tool for addressing the small-N problem.[14]

More work on concept formation is also needed, notwithstanding the sustained contribution of Sartori

(1970, 1984, 1991, 1993, and Sartori, Riggs, and Teune 1975); the work of authors such as McKinney (1966), Kalleberg (1966), and DeFelice (1980); and also Burger's (1976) invaluable synthesis of the Weberian approach to concept formation. Comparativists do not devote enough attention to thinking through how well or poorly concepts are serving them and therefore may have insufficient ground for knowing whether they are making appropriate choices in the effort to achieve theoretical parsimony.

The field of cognitive science has recently provided insights into categorization that may be useful in refining the concepts employed in comparative studies. The application of these insights is illustrated by George Lakoff's (1987) challenge to frameworks, such as that of Sartori, that employ what Lakoff calls "classical categorization," in which the meaning of concepts is understood in terms of defining characteristics that are seen as giving the concepts well-defined boundaries. This understanding is crucial to Sartori's framework, in that the problem of conceptual stretching which he analyzes hinges on these boundaries. Cognitive scientists argue that in ordinary language, the meaning of concepts derives not from defining characteristics, but from an implicit "cognitive model" that underlies the concept and from "exemplar" cases that serve to anchor the concept's meaning and provide a point of reference for identifying better and worse cases. This perspective provides a different view of the question of boundaries, and hence of conceptual stretching. More work is needed to discover the degree to which these patterns in ordinary language are also present in social science usage, and if so, the implications for the use of concepts in comparative analysis (see Collier and Mahon 1993).

Innovations Suggested by Work on Other Methods

Experimental Method

Although the experimental method itself may be of little relevance to the topics addressed in most comparative research, ideas derived from the experimental method can improve small-N studies. Donald Campbell and Julian Stanley's classic *Experimental and Quasi-Experimental Designs for Research* (1963) shows how the logic of experimental design can be applied to "quasi-experiments," that is, to "observational" studies that include some event or innovation that has a form analogous to an experimental intervention, but that occurs in a "natural" setting. An example would be the initiation of a new public policy whose impact one wishes to assess.

Campbell and Stanley underline the great value in quasi-experiments of the "interrupted time series" design. In this design the analyst looks at a long series of observations over time, so that the values of the observed variable are examined not only at two points immediately before and after the policy change or other innovation (which "interrupts" the series), but also well before and well after. To illustrate the risk of restricting the analysis to these two observations, the authors present several hypothetical configurations of data in which restricting the analysis to two observations leads to a finding of sharp discontinuity, whereas the full time series reveals continuity. Causal inferences about the impact of discrete events can be risky without an extended series of observations. Comparativists employing small-N analysis must heed this warning, since they routinely analyze the impact of discrete events, ranging from wars, revolutions, and military coups to specific public policies.

Donald Campbell and Laurence Ross's (1968) subsequent analysis of the impact on traffic fatalities of the Connecticut crackdown on speeding in the 1950s provides a stunning "exemplar" of the imaginative application of a quasi-experimental design to public policy analysis. Indeed, Przeworski (1987, 31) has argued that methodology is influenced far more by exemplars than by formal attempts to "legislate" correct methods, and the Connecticut crackdown article has certainly played that role.[15]

The case appears to be a simple one. When the State of Connecticut initiated strict enforcement of the vehicular speed limit in the 1950s and traffic deaths dropped sharply, the cause and effect relationship seemed obvious. Yet in evaluating this causal link, Campbell and Ross do an impressive analysis of potential threats to its "internal validity" (was that really the cause in Connecticut?) and its "external validity" (can the finding be generalized?). No sensitive analyst can read this article without acquiring a more sober view of the problems of evaluating policy impacts.

Ideas about quasi-experimental and interrupted time series design have also been disseminated through the large body of writing on evaluation research. This includes studies that apply these ideas to the analysis of political development (Hoole 1978), as well as excellent treatments of experimental design and evaluation research in introductory textbooks on social science methodology, such as Babbie (1992).

Although much writing on quasi-experiments appears to offer helpful guidance and practical advice to small-N analysts, Christopher H. Achen's *The Statistical Analysis of Quasi-Experiments* (1986) may leave them feeling that the methodological challenges posed by this type of design are overwhelming. In studies of the impact of public policy, the core problem is the lack of "randomization" in the application of the policy, which

may result in selection bias. For example, the benefits of a policy are commonly received by some groups and not by others, on the basis of certain attributes possessed by the groups, and it is possible that these prior attributes will themselves reinforce the outcomes that the policy seeks to promote. In the absence of true experimental data, this poses the challenge of disentangling the impact of the policy from the impact of these prior attributes. This causal riddle can be addressed by constructing a model of how citizens are selected to be recipients of the policy. This model then becomes a building block in the analysis of the policy's impact, in that these prior considerations can be "factored out" in assessing the policy. Achen shows that solving the riddle requires a complex form of "two-stage" statistical analysis.

The implications of Achen's book may be discouraging for analysts working with a small number of cases. An adequate solution to the lack of randomization requires a form of statistical analysis which can be applied to an elaborate quantitative data set, but this technique would be hard to apply in a small-N study. A more hopeful view might be that the literature on experiments and quasi-experiments at least provides useful warnings about the perils of analyzing discrete events as if they were true experimental interventions. In the absence of appropriate data sets, the researcher must exercise caution in making causal claims.

Innovations in Statistics

Recent work on statistical analysis has provided both new warnings about the risks of statistical studies and new opportunities for doing meaningful statistical work with relatively modest case bases. The statistician David Freedman has launched a major assault on the use of multivariate quantitative analysis in the social sciences (1987, 1991), which he claims fails because the underlying research design is generally inadequate and because the data employed fail to meet the assumptions of the statistical techniques. His criticism may bring considerable satisfaction to those who have been skeptical about statistics all along and who take comfort in the greater "control" of the material they feel derives from analyzing relatively few cases through more qualitative techniques. It is realistic to expect that we may go through a period of greater questioning of the use of statistics in the social sciences. However, as with the rejection of quantitative cross-national research discussed above, it would be unfortunate if a reaction against quantitative studies went too far.

The emergence of new statistical techniques that are helpful in the analysis of relatively few cases makes such a blanket rejection unwarranted. One example is the development of "resampling strategies" such as the "bootstrap" and "jackknife" (Diaconis and Efron 1983, Mooney and Duval 1992). These techniques use computer simulation to create, from an initial set of real data, a large number of hypothetical replications of the study, which can then be used in statistical tests that are not as vulnerable to violations of distributional assumptions as are more conventional tests. These techniques may be especially useful when there is great heterogeneity among units, as may readily occur in cross-national comparisons.

The development of "robust" and "resistant" statistical measures (Hampel et al. 1987; Hartwig 1979; Mosteller and Tukey 1977) is promising in much the same way. These measures are relatively unaffected by extreme or deviant values and can therefore help overcome the problem in small-N analysis that findings may be seriously distorted by a single observation that is greatly in error.

Another set of techniques concerned with this same problem is "regression diagnostics" (Bollen and Jackman 1985; Jackman 1987). These are tests used in conjunction with conventional regression analysis to assess whether unusual values on particular observations, called influential cases, have distorted the findings. The advantage of regression diagnostics in comparison with robust and resistant statistics is that one can employ them with the more familiar coefficients associated with regression analysis.

The use of regression diagnostics is nicely illustrated in the recent debate on the relationship between corporatism and economic growth in 15 Western European countries (Lange and Garrett 1985, 1987; Jackman 1987, 1989; Hicks 1988; Hicks and Patterson 1989; Garrett and Lange 1989). The starting point of this debate is Peter Lange and Geoffrey Garrett's 1985 article, which presents an interesting and complex idea in a simple form. They argue that the organizational strength of unions in the labor market and the political strength of the left in the electoral and governmental arenas both have an impact on economic growth, but that this impact is shaped by a complex interplay between these two factors, which they represent through an "interaction" term in their regression analysis of the 15 cases.

In a reanalysis of their article, Robert W. Jackman (1987) employs regression diagnostics to examine certain influential cases that he believes distort their findings. In the ensuing discussions among these five authors, an expanded model with further control variables is proposed, this expanded model is both challenged and defended, and Lange and Garrett subsequently defend their original model and call for new data and further tests.

This scholarly debate brings together an important substantive problem, a high level of area expertise and knowledge of specific cases, the inventive

use of a relatively straightforward statistical model, a constructive critique based on regression diagnostics, and a sustained process of cumulative knowledge generation based on the scrutiny of a shared data set. Just as the Campbell and Ross article on the Connecticut speeding crackdown is an exemplar of a quasi-experimental design, this debate should stand as an exemplar of a methodologically sophisticated effort by several scholars to solve an important problem within the framework of small-N quantitative analysis. This debate also shows that although an "N" of 15 might often be approached through qualitative small-N comparison, it can likewise be subjected to statistical analysis, with interesting results.

Another area in which potential problems of statistical analysis are amenable to solution concerns the issue of "average effects" in regression studies. The results of the simpler forms of regression analysis are based on an average of the strength of causal relations across the cases being studied. For the coefficients produced by regression analysis to be meaningful, it is necessary that these causal relations be homogeneous across the cases. Yet Ragin (1987, chap. 4), among others, has forcefully argued that this assumption commonly does not hold, given the complex forms of "multiple conjunctural causation" often encountered in comparative studies. In different contexts of analysis, the interaction among causal factors may vary.

However, solutions to this problem are available. John E. Jackson (1992) shows how it can be addressed with advanced statistical techniques, and the interaction term in the Lange-Garrett regression analysis, discussed above, deals with precisely this problem: that the effect of one explanatory factor varies depending on the value of another explanatory factor. Finally, Przeworski and Teune's procedure of "replacing proper names," also discussed above, takes this problem of causal complexity and turns it into an opportunity to deal more theoretically with the diversity of causal patterns.

Innovations in the Case-Study Method

When Lijphart wrote his 1971 article, he apparently felt some hesitation about including a discussion of case studies in an assessment of the comparative method.[16] Yet the two topics are closely linked, and his helpful typology of the uses of case studies in hypothesis testing and theory building set the stage for refinements in case study analysis later introduced by other scholars.

One of the most suggestive discussions of the case-study method is that of Campbell (1975). He dramatically recants the bold assertion he made in his earlier book with Stanley that "one-shot" case studies are "of almost no scientific value" (1963, 7). He shows instead that case studies are the basis of most comparative research, that they offer many more opportunities than is often recognized for falsifying the researcher's main hypotheses, and that much can be learned from making explicit the comparisons that are often implicitly built into case studies. For example, any given hypothesis about a case has implications for many facets of the case. Campbell uses the label "pattern matching" to refer to the process of discovering whether these implications are realized. The analyst can thereby increase the "N" by multiplying the opportunities to test hypotheses within what may initially have been viewed as a "single" case.

This procedure of pattern matching is helpful in addressing the long-standing concern that case studies are useful for generating hypotheses, but that the same case cannot then be used to test the hypothesis because it offers no possibility of disconfirmation. This is sometimes referred to as the problem of ex post facto hypotheses.[17] The procedure of pattern matching opens the possibility that an hypothesis initially generated by a particular case could subsequently fail to be supported by the same case. Thus, the problem of ex post facto hypotheses can be partially overcome.[18]

Harry Eckstein (1975, 113-123) is likewise concerned with testing, as opposed to generating, hypotheses in case-study analysis, and he argues forcefully that many analysts have greatly underestimated the value of case studies for hypothesis testing. In particular, the carefully constructed analysis of a "critical case" -- for example, one about which the analyst has particularly strong expectations that it will fit the hypothesized causal pattern -- can provide an invaluable opportunity to falsify the relevant hypothesis.

Alexander George and Timothy McKeown (1985), building on George (1979), present a helpful synthesis of two key building blocks in the process through which hypotheses are tested in case studies. The first corresponds to the conventional approach to placing a case in comparative perspective, which they call the "congruence procedure." The scholar examines the values of an hypothesized independent and dependent variable for a given case and determines, in light of explicit or implicit comparison with other cases, whether these values are consistent with the predictions of the hypothesis under consideration (pp. 29-30). The second is "process-tracing," through which the researcher engages in a close processual analysis of the unfolding of events over time within the case (pp. 34-41). The goal is to assesses whether the dynamics of change within each case plausibly reflect the same causal pattern suggested by the comparative appraisal of the case in relation to other cases. Process tracing may be seen as a specific instance of Campbell's pattern matching, and as with

pattern matching the analyst makes a series of within-case observations against which the hypothesis can be further assessed.

Overall, these articles, along with works such as Robert K. Yin's *Case Study Research* (1984), offer a systematization of case-study procedures that provide a valuable point of reference for scholars concerned with small-N analysis. At the same time, the debate continues on the proper role of case studies in assessing and building theory. An interesting part of this debate, published as a special issue of the journal *World Politics* (1989) focuses on the contribution of case studies to evaluating one application of rational choice analysis, i.e., rational deterrence theory in international relations. The opening article by Achen and Snidal (1989) argues that the case studies employed by many international relations specialists do not adequately address the central ideas of this body of theory, thereby raising an issue perhaps not often enough considered in discussions of the comparative method: How can the *methodological* concern with executing good comparisons be linked to the key analytic issues posed by the particular *theories* that are to be evaluated? Achen and Snidal also note the problem of selection bias in case studies of deterrence theory, that is, the problem that case studies usually focus on deterrence failure, whereas much or most of the time deterrence works. The issue of the journal includes a series of articles by scholars close to the case-study tradition who debate the issues raised by Achen and Snidal. These articles constitute a valuable effort to think through how case studies have functioned in relation to the assessment of a particular body of theory, a line of inquiry that should be taken up more often.

In this debate on deterrence theory, an intellectual tension emerges that has been a recurring theme in this chapter: between analyses that seek to achieve a generic understanding, based on relatively few variables and encompassing many cases, as opposed to analyses that seek to draw out the complexities of particular cases.

Conclusion

Among the diverse approaches discussed in this chapter, three major analytic alternatives stand out. First, new perspectives on the case-study method have strengthened the viability of that approach. Discussions of opportunities for within-case comparisons have in fact begun to blur the distinction between case studies and the comparative method, although the case-study approach does remain a distinct tradition. Interest in case studies has been reinforced by several factors, including the renewed concern with interpretive social science, the continuing intellectual and institutional strength of area studies, and deep skepticism in some circles about the validity of broad comparison.

Second, it is evident that quantitative techniques employing a relatively small number of cases can successfully address important substantive questions. This approach merits attention in light of the new statistical tests suitable for small-N analysis. The opportunity for cumulative scholarly learning provided by statistical studies is nicely illustrated by the Lange-Garrett-Jackman-Hicks-Patterson debate. This debate is also relevant to the issue of linking rival research traditions, because it shows that insights derived from case studies and from more qualitative comparative work can, after all, serve as stepping-stones on the path toward statistical analysis.

The third alternative has been reinforced as well: the systematic comparison of a small number of cases, with the goal of causal analysis, which is the approach that Lijphart originally advocated. In this perspective, broad qualitative comparison is seen as both possible and productive. The growing influence of the school of comparative historical analysis has substantially enhanced the credibility of this approach, and it plays an important role as an analytic middle ground between the case-study tradition and small-N statistical analysis.

All three of these approaches will persist, and a key question is how well they can be linked. The tradition of research on Western Europe provides an encouraging model, in that the findings of quantitative comparative scholars play an important role in general debates in that field.[19] In research on Latin America, by contrast, quantitative comparative work receives considerably less attention from mainstream scholars. Yet the kind of cross-fertilization found in the West European field can make an important contribution to strengthening research. With good communication, country specialists and experts in qualitative small-N comparison can push the comparative quantifiers toward more carefully contextualized analysis. Likewise, the comparative quantifiers can push the country specialists and experts in qualitative comparison toward more systematic measurement and hypothesis testing. A central goal must be to sustain such communication.

The implications for graduate training are clear. If Ph.D. candidates are to be prepared to address these issues of comparison, they should have enough training in statistical methods to evaluate quantitative studies that employ old, and new, methods of statistical analysis and to use such methods when appropriate. Those more oriented toward statistical analysis should have enough background in qualitative small-N comparison and case study analysis to be able to build on the analytic contribution of those approaches. Both groups should have substantial exposure to basic writings on the

philosophy of science and logic of inquiry that can provide the framework for more informed choices about these methodological alternatives.

In this way, the foundation can be laid for an eclectic practice of small-N analysis that takes advantage of opportunities that present themselves on both sides of what could otherwise be a major intellectual divide.

Notes

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1. "N" is used to refer to the number of cases analyzed in any given study.

2. References to representative works of comparative historical analysis are presented below.

3. In his comparison of these methods, Lijphart acknowledges his debt to Smelser's (1968) excellent analysis that employed a parallel framework. See also Smelser (1976).

4. This perspective has been elaborated by Skocpol (1984, chap. 11), and a parallel formulation is found in Charles Tilly (1984, chap. 4).

5. Skocpol and Somers (1980, 181-87) refer to this as "macro-causal" analysis. Yet small-N studies that generate and test hypotheses can have both a macro and a micro focus, and it does not seem productive to exclude from this category those with a micro focus. Hence, this alternative label is used.

6. Although Przeworski and Teune are centrally concerned with issues that arise when additional cases are added to an analysis, the problems they discuss are also more likely to occur if one is dealing with a larger N to begin with.

7. For example, instead of referring to "Venezuela," one would refer to a country in which, due to the impact of massive oil revenues, a particular causal relationship assumes a distinct form.

8. "Thick description" is sometimes mistakenly understood to refer simply to "detailed description," which is not what Geertz intends.

9. Given that these studies often focus on long periods of time within each case, it might be argued that the number of cases could be greatly increased through comparison over time, thereby making them something other than small-N studies. However, since the goal of many studies in this tradition is to explain overall configurations of national outcomes as they are manifested over long periods, these outcomes often cannot be disaggregated into a series of longitudinal observations. Hence, the number of cases cannot realistically be increased through the use of comparison over time.

10. The most similar and most different systems designs correspond, respectively, to John Stuart Mill's (1974) method of difference and method of agreement. Whereas Przeworski and Teune's labels of "similar" and "different" refer to whether the cases are matched, as opposed to contrasting, on a series of *background* variables, Mill's labels of "difference" and "agreement" refer to whether the cases are contrasting, as opposed to matched, on the *dependent* variable.

11. Personal communication from Adam Przeworski.

12. Christopher Achen, personal communication, has long insisted on this point.

13. For example, Gereffi and Wyman (1990), Haggard (1990), Przeworski (1991), and Rueschemeyer, Stephens, and Stephens (1992).

14. For a discussion of strategic choice models (a closely related type of model) that have been applied to the analysis of political reform, democratization, and democratic consolidation in Latin America, and that likewise offer fruitful simplifications of complex phenomena, see Collier and Norden (1992).

15. The reprinting of this article in a reader on social science methodology (Tufte 1970) made it widely available to political scientists, and its influence has been substantial.

16. Personal communication from Arend Lijphart.

17. This problem is routinely discussed in introductory methodology texts, e.g., Babbie (1992, 24-25, 427).

18. Although pattern matching within the same case introduces the possibility of falsifying the hypothesis, it does not overcome all of the problems of ex post facto hypotheses. Thus, pattern matching will probably not overcome a problem of unrepresentativeness which may arise due to selection bias or to the chance selection of an atypical case.

19. See, for example, the debate on interest mediation and corporatism in Western Europe, including Wilensky (1976), Hibbs (1978), Schmitter (1981), and Cameron (1984). The debate started by Lange and Garrett (1985) is a continuation of this line of analysis.

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6

The State of Quantitative Political Methodology

Larry M. Bartels and Henry E. Brady

In the first edition of this volume, Achen (1983a, 69) complained that "political methodology has so far failed to make serious theoretical progress on any of the major issues facing it." In the intervening decade, thanks in no small part to Achen's prodding, the field of political methodology has progressed significantly on several fronts. While political methodologists have still "done nothing remotely comparable" to the invention of factor analysis by psychometricians or structural equation methods by econometricians (Achen 1983a, 69), they have invented, adopted, or further developed an impressive variety of useful techniques for dealing with event counts (King 1989d), dimensional models (Poole and Rosenthal 1991; Brady 1985b, 1990; Enelow and Hinich 1984), pseudo-panels (Franklin 1990), model misspecification (Bartels 1991b), parameter variation (Rivers 1988; Jackson 1992a), aggregated data (Achen and Shively n.d.), selection bias (Achen 1986b), non-random measurement error (Brady 1985a; Palmquist and Green 1992), missing data (Mebane n.d.), and time series data (Freeman, Williams, and Lin 1989; Beck 1990).

Moreover, while the volume and sophistication of basic methodological research in political science have increased significantly in the decade since Achen surveyed the field, the impact of methodological research on empirical work throughout the discipline has probably increased even more significantly. In order to document that fact, in addition to describing recent methodological advances in a variety of areas, we point to innovative and important applications of quantitative methods in every part of political science. To that end, in addition to reviewing every issue of *Political Methodology* (1974-1985) and the four volumes of *Political Analysis* dated 1989-1992 (but published in the next year), we reviewed and classified according to methodology over 2,000 articles in other political science journals, including every article published in *The American Political Science Review* from 1981 to 1991 and every article published in *The American Journal of Political Science*, *Comparative Political Studies*, *Comparative Politics*, *Journal of Conflict Resolution*, and *International Studies Quarterly* from 1984 to 1991.[1]

Despite having cast so wide a net, we must emphasize that, due to inevitable limitations of space and expertise, our discussion of problems and techniques remains selective. Models and analyses based on computational, rather than statistical, logic are gaining a foothold (Alker 1988; Schrodt 1991). Data graphics, inspired by the ideas of Tufte (1983) and Cleveland (1985) and by the power and convenience of computer graphics packages, are becoming increasingly sophisticated. These strands of research and others have added significantly to the vitality and utility of political methodology in the last decade. We omit them here not because we consider them unimportant or uninteresting, but because we prefer to focus our finite attention upon what we consider the mainstream of contemporary political methodology: the armamentarium of techniques developed to relate statistical models to quantitative data of various sorts.

There is no one way to organize the many topics touched upon in our survey of the field. We have chosen to follow a rough logical progression from data collection through modeling to estimation. We begin in Section 1 with data collection, where there has been a notable resurgence in the use of experiments, innovations in survey design, and a proliferation of events data in a variety of research areas.

Sections 2 and 3 are organized around distinctive units of analysis. Time-series research has profited greatly from the development of Box-Tiao methods, vector autoregression, cointegration and error correction models, and the Kalman filter; these developments are described in Section 2. In Section 3 we survey innovations in the use of pooled time-series cross-sectional data, panel data, and auxiliary data sets, as well as the current state of aggregate data analysis.

Another familiar distinction is among nominal, ordinal, interval, and ratio data. Special methods are necessary for dependent variables that are not interval or ratio measures; in Section 4 we discuss methods for analyzing polychotomous "categorized" data such as standard Likert opinion questions, polytomous or multiple choice data such as choice of party or candidate, and

event count and duration data such as the number of governments or wars in a period of time.

No matter what the measurement level, our variables are often measured with considerable error. In Section 5 we discuss some sources and consequences of measurement error. We also note some significant progress in dealing with random and non-random measurement error, sample selection bias, and missing data.

In Section 6 we turn to dimensional models, which have often been used to understand the factors underlying voter and legislative choice. Political scientists initially borrowed methods for dimensional analysis from psychology; but in the past twenty years, in this perhaps more than in any other area, political scientists have led the way in making methodological advances.

In Section 7 we survey some general problems of model specification. We outline some techniques for policing the consequences of specification uncertainty, including sensitivity analysis, cross-validation, and formal specification tests, and we describe some distinctive contributions by political scientists toward understanding and managing the statistical consequences of misspecification.

Finally, once the data are collected and the model specified, parameter estimates must be obtained. Political methodologists have now moved well beyond using ordinary least squares for every problem. In Section 8 we describe advances in estimation, including creative applications of maximum likelihood and other estimation methods.

Our view is that the subfield of political methodology exists to serve the needs of substantive political research and can be successful only to the extent that it facilitates progress in the various substantive subfields of the discipline. Thus, in Section 9 we review some of the many areas of political science in which methodological advances have led directly to important substantive advances in the past decade or so. We focus especially on two areas identified by Achen (1983a) as ripe for methodological contributions: the nature of the survey response and the phenomenon of economic voting.

1. Data Collection Methods

Political scientists' data collection strategies have been diversified and enriched in the past decade. Experimenters have created more realistic experimental settings and married experiments with survey research. Survey researchers have moved from strictly cross-sectional designs to repeated cross-sections, sophisticated panels, and other approaches designed to add time and context to the analysis. Events data have proliferated, and lengthy, high-quality time series involving many nations or other actors are now available for a wide range of phenomena.

While these new sources of data have improved our ability to generalize and to make causal inferences, they have also provided new challenges for political methodologists. Since some of the discipline's major data collection efforts have cost millions of dollars and decades of people's lives, it seems well worth our time and trouble to develop new techniques, and to train researchers in those techniques, in order to extract as much information as possible from these often heroic data collection efforts.

Experiments

In the past decade, political scientists have found many new and exciting ways to do experimental work. Samples are more representative and treatments are more realistic, increasing greatly the external validity of experimental work. Experimental methods have been built into opinion surveys to combine the advantages of both approaches. In addition, there is a strong commitment to using experiments to help develop and test formal theories of collective decision making, political psychology, and other important political phenomena. For example, whereas books on question-wording experiments (Schuman and Presser 1981; Sudman and Bradburn 1982) once seemed to be nothing more than catalogs of odd and even freakish phenomena, recent work has used similar experiments to understand how people think about politics (Tourangeau and Rasinski 1988; Sniderman et al. 1991; Sniderman and Piazza 1993). A new book on experimental research in political science (Kinder and Palfrey 1992) provides an excellent sample of the diverse and sophisticated work now being done in this area.

Although there is a long tradition of using experiments in public opinion research to determine the impact of political messages on people's opinions (Hovland, Janis, and Kelley 1953), these experiments typically involved unrepresentative samples (college students), limited treatments (sometimes just differences in written instructions), and often artificial treatments (hypothetical candidates, countries, or news reports). They showed that some people could be persuaded in some ways under some conditions, but they could not provide reliable evidence about the likely presence or magnitude of those effects in natural settings.

In the 1980s political scientists became more sensitive about the external validity of experimental research and started to develop more realistic experiments done in real time with real people. More representative

samples were recruited from outside the university (Iyengar and Kinder 1987; Iyengar 1987, 1991), sometimes even regionally or nationally representative samples (Sniderman et al. 1991; Piazza, Sniderman, and Tetlock 1990; Johnston et al. 1992). Experiments also used more sophisticated stimuli such as entire news programs carefully edited to include or omit particular stories (Iyengar and Kinder 1987; Sullivan and Masters 1988; Masters 1988; Iyengar 1991). Finally, more complex designs have included re-interviews after a period of time to see whether effects persist (Iyengar and Kinder 1987; Sullivan and Masters 1988).

Another group of experimental researchers has not worried very much about representativeness or realism because they simply wanted to convince skeptics that citizens could make the seemingly complicated and sophisticated calculations required by theories of rational choice, bargaining, and justice, and that the equilibria predicted by rational choice theory would be attained under some conditions. In perhaps the most famous experiments in this tradition, Levine and Plott (1977) and Plott and Levine (1978) showed how committee agendas can be manipulated to reach a wide range of divergent outcomes. Riker (1986, chap. 3) provided a very readable summary of these results.

More recently, McKelvey and Ordeshook (1985) considered a sequence of two-candidate elections under incomplete information in which "voters have no contemporaneous information about the candidate positions, and candidates have no information about voter preferences. The only source of information is historical data on the policy positions of previous winning candidates and contemporaneous endorsement data" (1985, 480). The experimental data provided some support for a mathematical result predicting an equilibrium under these conditions equivalent to what would occur if voters had full information.

In another series of experiments, Frohlich and Oppenheimer (1990) showed that students do not adopt Rawlsian notions of justice in a simulated task, although they do choose a "safety net" for the least able of their members. They also showed that in a production environment where students must live with the distributive rule they have chosen, participation in choosing the rule leads to greater acceptance of it.

In this kind of research, the experimental situations are obviously artificial, but the surprise lies in finding out that relatively naive subjects can act in ways that are very sophisticated. McKelvey and Ordeshook (1990, 140-41) concluded that:

> the experimental research that this essay reviews gives us considerable confidence that the large body of theoretical research into spatial models of committees and elections is not without sound empirical content. Certainly, those models may have to be elaborated before we can apply them directly to the study of, for example, the U.S. Congress, parliaments, and actual elections. But those same experiments, in addition to revealing fruitful avenues of opportunity for the theorist, also suggest how theoretical models can be adapted to environments outside the laboratory.

A recent edited volume (Palfrey 1991) provides another overview of laboratory research in political economy.

Perhaps the most innovative use of experimentation has been the effort of Sniderman and his collaborators to marry experimental research and representative sample surveys. In a series of studies of American racial attitudes and Canadian views of rights and freedoms, they designed variations in question wording to test for the impact of various considerations on people's attitudes. For example, they asked respondents whether certain kinds of people deserve government help, with random changes in the race, gender, age, marital-parental status, and "dependability" of the prospective recipient (Piazza, Sniderman, and Tetlock 1990). This complex experiment is made possible by a computer assisted telephone interviewing (CATI) system that can produce as many different interview "forms" as the researcher can devise.

Sniderman and his colleagues also described other possible experimental and non-experimental methods using CATI. Source attribution experiments vary the credibility of the source associated with a statement. This technique was used by Johnston et al. (1992) in an effort to simulate the rhetoric of the 1988 Canadian election campaign. By asking some randomly selected respondents about the "Canadian" free trade agreement and others about the "Mulroney Government's" free trade agreement, they found that Prime Minister Brian Mulroney was a rhetorical liability during the early part of the campaign but became an asset as his popularity increased towards the end of the campaign. Johnston et al. also used a "challenge" methodology advocated by Piazza, Sniderman, and Tetlock (1990) to determine which arguments were most likely to move public opinion about free trade as the campaign progressed.

These techniques and others like them provide new ways to do public opinion research, allowing researchers to learn about people's attitudes and beliefs by challenging them, by employing varying stimuli in unobtrusive ways, and by using experiments to test what really matters to people.

Survey Designs

Survey research now profits from having a history. There are more than 50 years of Gallup polls at almost monthly intervals, 40 years of American National Election Studies done every two years, 20 years of the annual General Social Survey, and many other valuable surveys. The chasm between time-series analysts using aggregate data and cross-sectional analysts using survey data has been filled over time with mountains of survey data. The limitations of the standard cross-sectional survey, discussed in detail by Mebane (1991), can now be overcome with a time series of cross-sections. Researchers can also use these surveys as time series to follow the dynamics of public opinion over long time periods (Mueller 1973; Carmines and Stimson 1989; Page and Shapiro 1992; Stimson 1991a; Mayer 1992). And mystifying differences between the results produced by time-series and cross-sectional analyses can be resolved by analyzing surveys over time, as we document in Section 9 below for the case of economic voting (Kramer 1983; Markus 1988).

Survey researchers have also developed new designs for surveys in the past 25 years. The 1978 and 1980 National Election Studies produced representative samples of around 20 people in each of 108 congressional districts, allowing researchers to study the linkages between public opinion, congressional elections, and the roll-call votes of members of Congress (Bartels 1991a). The 1988, 1990, and 1992 Senate election studies produced even larger samples of about 75 respondents in each of the 50 states, providing rich opportunities to study temporal, geographical, and candidate- and election-specific variations, often with the "campaign" as the relevant unit of analysis (Franklin 1991).

The 1980 National Election Study combined repeated cross-sections with a panel beginning in January and continuing through the election year (Markus 1982) to provide the most complete study of campaign dynamics since the classic Columbia studies (Lazarsfeld et al. 1944; Berelson et al. 1954). The 1984 National Election Study completed 70 interviews a week from January to November, facilitating "rolling-cross-section" analyses of election season dynamics (Brady and Johnston 1987; Allsop and Weisberg 1988; Bartels 1988). The 1988 Canadian Election Study used daily samples of 70 people to produce a study of the dynamics of a parliamentary election (Johnston et al. 1992).

The House, Senate, and rolling cross-section studies all provide small numbers of interviews by some discrete unit -- a congressional district, a state, a week, or a day. These units vary on a variety of contextual dimensions, including the degree of competitiveness in the election, media coverage, and autonomous events. The challenge for political methodology is to make full use of the information contained in time- or place-specific samples that, treated individually and naively, are too small and error-laden to be of much use.

Events Data

Events data vary greatly in their substantive focus (alliances, cabinet dissolutions, presidential vetoes), units of observation (the international system, nations, presidencies), and time intervals (days, months, years). Yet they share a common structure, recording whether a particular event has occurred in each time interval in each relevant unit, and, if it has, recording attributes of the event, the unit, the time interval, or all of these.

The prototypical events data collections in political science are those dealing with war (Sorokin 1937; Wright 1942; Richardson 1944, 1960). Among the best known of these is the Correlates of War project of Singer and Small (1972; Small and Singer 1982), which includes data from 1816 to 1980 on various features of wars, civil wars, and interstate disputes. More recently, Levy (1983) has developed a similar data set for wars involving great powers from 1495 to 1975. In the 1980s, the Singer-Small, Levy, and other data sets have been extended as part of a National Science Foundation project on Data Development for International Research (DDIR); the interstate conflict databases funded by this project were described by Cioffi-Revilla (1990), and an overview of international data sets was provided by McGowan et al. (1988).

Many other forms of events data are available in political science. Within international relations, the term "events data" is often reserved for daily codings of newspaper reports on international interactions, such as Charles McClelland's World Events Interaction Survey (WEIS) and Edward Azar's Conflict and Peace Data Bank (COPDAB).[2] Several volumes have been devoted to methodological issues arising from these and similar data (e.g., Azar and Ben-Dak 1975; Burgess and Lawton 1972; Kegley et al. 1975), and a 1983 symposium in *International Studies Quarterly* (Howell 1983; Vincent 1983; McClelland 1983) raised questions of comparability between the WEIS and COPDAB time series. Goldstein and Freeman (1990) justified their use of events data to analyze conflict and cooperation among the United States, the USSR, and China in part by demonstrating convergence in their findings across three different sets of events data (WEIS, COPDAB, and Ashley [1980]).

Events data have also been gathered to study patterns at the national level. The best known and most widely used of these data sets (McGowan et al. 1988) is the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983a, 1983b).[3] Banks (1971)

provided yearly attribute and events data, sometimes dating back to the early nineteenth century, for most of the countries of the world. Morrison, Mitchell, and Paden (1989) provided extensive data on events in African countries. Freedom House provides yearly ratings of the level of freedom in countries around the world (Starr 1991). Bienen and van de Walle (1989) have data on 165 African leaders from independence through 1987; Jackman (1978) has data on African coups from 1960 through 1975; Strom (1985) has extensive data on governmental coalitions in Western parliamentary democracies between 1945 and 1982; King and Ragsdale (1988) have data on the American presidency, including presidential vetoes, press conferences, and many other events.

The analysis of events data is not straightforward. There are significant measurement problems and complicated analytical issues arising from unreliability in the classification of events and the complexities of inherently multidimensional phenomena. This is most clearly seen in the difficulties of defining wars (Levy 1983, chaps. 3 and 4; Small and Singer 1982, chaps. 2 and 3; Levy 1985; Thompson and Rasler 1988), democratic transitions (O'Donnell and Schmitter 1986, chap. 2), episodes of deterrence (Huth and Russett 1988; Lebow and Stein 1990), or coups in Africa (Jackman et al. 1986). Even defining something as apparently simple as the duration of a government can raise difficulties (Lijphart 1984).

In response to difficulties like these, Collier and Mahon (n.d.) suggest a need for multidimensional conceptualizations of events and for analytical techniques allowing for multivariate dependent variables. Another, albeit little used, approach to measurement problems in events data is to employ multiple indicator methods borrowed from psychometrics (Achen 1987). For example, Faber (1987) used confirmatory factor analysis to study the measurement properties of the COPDAB data.

Analytical problems also arise in deciding whether to analyze events history, sequence, or counts data,[4] and in developing models appropriate to each alternative (Tuma and Hannan 1984). Sociologists (Tuma and Hannan 1984; Allison 1984), statisticians (Cox 1972), economists (Heckman and Singer 1982), epidemiologists (Gross and Clark 1975; Elandt-Johnson and Johnson 1980), and engineers (Kalbfleisch and Prentice 1980) have developed sophisticated methods for analyzing these different types of events data for marital, employment, and health statuses (including the reliability of machines and products). The challenge for political scientists is to adopt these methods when they are useful and to develop new ones to meet the special needs of our discipline.

2. Time-Series Analysis

Time-series data have come to play an increasingly prominent role in political science in the last decade, especially in empirical work at the intersection of politics and economics. The time-series methods first introduced by Hibbs (1974), including generalized least squares and autoregressive integrated moving average (ARIMA) models (Box and Jenkins 1976), are now adopted routinely to model a wide variety of trends, autoregressive errors, and moving average processes in time-series data.[5]

The econometric literature continues to be the major source of new techniques for analyzing time-series data.[6] But political scientists have become increasingly sophisticated in their efforts to adopt and adapt these techniques to studies of the political business cycle (Beck 1987), presidential popularity (Beck 1992; Ostrom and Smith n.d.), arms races (Williams and McGinnis 1988), and other political phenomena. Four techniques in particular have seen both rapid theoretical development and fruitful application in political settings: Box-Tiao intervention models, vector autoregression, cointegration, and the Kalman filter.

Box-Tiao Intervention Methods

Box-Tiao intervention methods (Box and Tiao 1975), introduced to political scientists by Hibbs (1977), have been used to show that the ideology of governing parties influences unemployment rates (Hibbs 1977), although world economic conditions also matter (Alt 1985); that wars influence economic growth rates, government expenditure, and taxes (Rasler and Thompson 1985a, 1985b); that bureaucratic interventions and outputs change with such political factors as legislation, appointments, and control of the presidency (Wood 1988; Wood and Waterman 1991); and that partisanship responds to presidential popularity and other political trends (MacKuen, Erikson, and Stimson 1989).

The results from these studies are fascinating stylized facts, but the interventions they consider (wars, political events like Watergate, and changes in the ideology of a governing party) are often very complex "treatments" for which simple causal claims ("wars increase taxes") seem dangerous. In addition, Box-Tiao methods are usually employed to estimate simple "reduced" forms with few covariates and little attention to causal sequence. More researchers should employ causality tests (e.g., MacKuen, Erikson, and Stimson 1992) to lend credence to their arguments, and more attention should be devoted to developing structural models. For example, Hibbs and Dennis (1988, 478), rather than simply following Hibbs' earlier (1977) lead

analyzing how political partisanship affects income equality, built a structural model in which "unemployment, inflation, transfer spending, and inequality are determined endogenously, and they are driven politically by the party controlling the White House and the partisan balance in Congress."

Vector Autoregression

Vector autoregression is due primarily to Sims (1977, 1980). The excellent exposition by Freeman, Williams, and Lin (1989) is aimed at political scientists and includes an illustrative empirical analysis of government expenditures in Britain. Williams and McGinnis (1988), McGinnis and Williams (1989) and Williams (n.d.a) provided additional political applications.

The main feature of vector autoregression is that each variable in an equation system is regressed on a large collection of past values of all the variables in the system. The resulting parameter estimates may be thought of as reduced-form parameter estimates for some unspecified structural model including the same set of variables. This approach has two significant attractions. First, the problem of model specification is solved by simply including lagged values of all the variables in the system.[7] And, second, since current disturbances are uncorrelated by assumption with everything in the past, the problem of estimation has a simple solution: ordinary least squares is always the appropriate estimator.

But this simplicity has its price. The unrestrictive approach of vector autoregression to the selection of regressors entails a corresponding cost in the precision of the resulting reduced-form parameter estimates. Indeed, parameter estimates from vector autoregressions tend to be so imprecise that it is conventional not to interpret them individually but to test the significance of *sets* of parameter estimates for one or more variables simultaneously using an F-test or modified likelihood ratio test (Freeman, Williams, and Lin 1989, 845).[8]

Statistical tests on whole sets of parameter estimates can produce results that seem puzzling to analysts with specific ideas about the causal processes at work. For example, in Williams and McGinnis's vector autoregression of U.S. and Soviet military spending levels from 1949 to 1981 (1988, 984), F-tests failed to reject the null hypotheses that the two series are mutually exogenous ($p=.29$ and $p=.55$), despite the fact that the single parameter estimate most plausibly representing the reaction of U.S. spending to observed Soviet spending (with a three-year lag, as suggested by Ostrom and Marra 1986, 828) was large (.81) and clearly different from zero by conventional statistical standards (with a standard error of .37, even in a regression with only 22 degrees of freedom, $p<.025$).

Since individual parameter estimates from vector autoregressions are not directly interpretable as estimates of structural effects, analysts commonly depict the dynamic relationships among variables by simulating the effects of random shocks, or "innovations," in one variable upon the future time paths of all the variables in the system. However, the results of this "innovation accounting" sometimes depend crucially upon the analyst's assumptions regarding the causal relationships among the contemporaneous innovations themselves. Typically, in order to interpret contemporaneous correlations among residuals from different equations, the analyst "must choose an ordering of the causal chain of contemporaneous effects, an ordering that should be given some theoretical underpinning" (Freeman, Williams, and Lin 1989, 847). When the pattern of responses in the dynamic system depends crucially upon such choices, the strong assumptions about causal structure that are supposed to be eschewed in vector autoregression must be let back in by a side door.

Nevertheless, proponents of vector autoregression argue that their approach reflects a more realistic attitude toward the uncertainties of model specification than does the conventional approach to specifying and estimating simultaneous equation models. As Freeman, Williams, and Lin (1989, 853-54) put it:

> SEQ [conventional simultaneous equation] and VAR [vector autoregression] modeling rest on different assumptions about what we presently know and what we can know about the nature of social reality. SEQ modelers presume that our theories are relatively well developed, or that we know quite a bit about social reality. For example, our theories tell us exactly how variables are interrelated, including the functional forms of these interrelationships. This knowledge is reflected in the strong, zero-order restrictions that SEQ modelers impose on their equations, for example, in the exogeneity and lag specifications in their equations. . . . VAR modelers assume, in contrast, that our theories are relatively undeveloped, or that our understanding of social reality is severely limited. . . . Only a weak set of restrictions about what variables to include in the model and about the contemporaneous relationships among variable innovations are theoretically justified, in their opinion.

One way to mitigate the imprecision of the results of vector autoregression is to incorporate prior

beliefs about the structure of lagged relationships within a Bayesian framework. The use of informative priors in Bayesian vector autoregression (BVAR), especially to improve the precision of forecasts, has been advocated in economics by Todd (1984) and Litterman (1984), among others, and in political science by Williams (n.d.a; McGinnis and Williams 1989). Indeed, there is some reason to hope that the adoption of a Bayesian approach to vector autoregression may encourage the use of Bayesian methods by other analysts as well, perhaps contributing eventually to the sort of integration of complementary approaches envisioned by Zellner (1979).

Cointegration and Error Correction Models

Much economic theory and some in political science makes rich use of the notion of "equilibrium." Error correction models are methodologically attractive primarily because they can represent quite directly both theoretical expectations about equilibrium relationships and real-world deviations from those equilibrium relationships. For example, Ostrom and Smith (n.d.) distinguished between a set of "quality-of-life outcomes" (unemployment and inflation) expected to have "consistent and enduring effects on presidential approval" and a set of "dramatic political events" (foreign crises, scandals, health problems, and the like) expected to produce transitory "bumps and wiggles" in long-run trends of presidential approval. An error correction model captures the notion that dramatic events may make presidential approval at any given time somewhat higher or lower than is warranted by the underlying equilibrium relationship between quality-of-life outcomes and approval but recognizes that these transitory discrepancies will tend to erode over time as a political system returns to equilibrium.

In the past several years, time-series econometricians have made rapid strides in the development of error correction models, and of techniques for addressing the closely related phenomenon of "cointegration." The seminal article by Engle and Granger (1987) on cointegration has spurred a booming econometric literature (Phillips 1988, 1991; Engle and Granger 1991). Applications in political science include Beck (1992) and Ostrom and Smith (n.d.) on presidential approval, and Ostrom and Smith (1991) on defense spending.

A series of variables is said to be cointegrated if some linear combination of those variables is of lower order than the variables themselves. For example, two (or more) series X_{jt} may each be non-stationary (exhibiting mean drift), but with some cointegrating vector α producing a linear combination $Z_t = \alpha'\mathbf{X}_t$ that is itself stationary (with constant mean and finite variance). The relationship of cointegration implies "that both series individually have extremely important long-run components but that in forming Z_t these long-run components cancel out" (Granger and Newbold 1986, 225). As Granger and Newbold also pointed out (1986, 226), Z_t can be thought of as a short-run (stationary) deviation from an underlying equilibrium relationship $\alpha'\mathbf{X}_t = \mathbf{0}$; "many economic theories are expressed in terms of equilibria of just this sort. Now Z_t can be interpreted as the equilibrium error, that is, the extent to which the economy is out of equilibrium."

The Granger Representation Theorem (Engle and Granger 1987) shows that the cointegrating relationship $\mathbf{Z}_t = \alpha'\mathbf{X}_t$ can be transformed into an equation relating current changes in any one variable to lagged changes in all the variables in the system, plus a moving average of current and lagged disturbances, minus some fraction of the previous equilibrium error Z_{t-1}. This transformation provides explicit connections not only between cointegration and error correction (since it allows for direct estimation of the error correction mechanism through the coefficient on Z_{t-1}), but also between cointegration and vector autoregression (since the transformed equation is essentially a vector autoregression in first differences, but with the additional regressor Z_{t-1}).

Engle and Granger (1987) proposed a sequential technique for estimating both the long-run equilibrium relationships among the variables in the system and the short-term dynamics of the error correction mechanism. The first step in this sequential technique is to estimate the long-run equilibrium relationship among variables by regressing levels of each variable on levels of all the other variables in the system. Thus, for example, Ostrom and Smith (n.d.) began by regressing monthly levels of presidential approval for the eight years of the Reagan administration on levels of inflation and unemployment (plus dummy variables for the 1981 assassination attempt and the Iran-contra scandal), and by regressing levels of inflation and unemployment on levels of the other variables as well. The second step is to test for cointegration in the relevant series. In the case of presidential approval, it is neccesary to establish that, although presidential approval levels themselves are not stationary, the residuals from the first-stage regression of approval on other variables in the system are stationary.

The third step in the Engle-Granger technique is to estimate the error correction model, which is essentially a vector autoregression of first differences but with an additional variable (a residual from the corresponding first-stage estimation) representing Z_{t-1}, the extent to which the system was in disequilibrium in the previous period. The parameter estimate associated with this lagged residual will lie between 0 and -1 if the error correction model makes substantive sense and can be

interpreted as a measure of the strength of equilibrating forces in the system. For example, Ostrom and Smith (n.d.) estimated a coefficient on lagged residual popularity of -.58 (with a standard error of .09); the implication of this estimate is that shocks to Reagan's approval rating from dramatic political events had a half-life of about a month.

The econometric literature on cointegration continues to advance at a rapid pace. While this advance is itself impressive, the speed and sophistication with which political scientists have begun to apply these techniques are also impressive. Some recent developments are reflected in the exchange among Durr (n.d.), Beck (n.d.), Williams (n.d.b), and Smith (n.d.) in *Political Analysis*, and in a further exposition by Smith (1992).

The Kalman Filter

The Kalman filter was invented in engineering (Kalman 1960) and developed in economics by Harvey (1981), Watson and Engle (1983), and others. Harvey's (1989) book-length treatment covers both the basic theory and a variety of extensions. The expository survey for political scientists by Beck (1990) includes an accessible theoretical discussion, illustrative analyses of presidential approval and government investment data, and help with programming in GAUSS.

The Kalman filter is a tool for estimating a "state space model" combining a transition equation for latent endogenous variables with a measurement equation relating latent variables to observable indicators. For an Lx1 latent state vector γ_t, the transition equation can be written as:

$$\gamma_t = \Phi_t \gamma_{t-1} + B_t x_t + \Psi_t \epsilon_t ,$$

where x_t is a Kx1 vector of observed exogenous variables, ϵ_t is an Mx1 vector of stochastic disturbances, and Φ_t, B_t, and Ψ_t are matrices of parameters to be estimated.[9] An Nx1 vector of observed data y_t is related to the latent endogenous variables γ_t (and to the observed exogenous variables) by the measurement equation

$$y_t = \Lambda_t \gamma_t + \Xi_t x_t + \Omega_t \delta_t ,$$

where δ_t is an Nx1 vector of measurement errors and Λ_t, Ξ_t and Ω_t are matrices of parameters to be estimated. Thus, a useful way to think about the Kalman filter is as a structure-cum-measurement model for time-series data parallel to the general model of covariance structures developed by Jöreskog and others for cross-sectional data (Engle and Watson 1981).

The main attraction of the Kalman filter is its flexibility. For example, since all the parameters in the measurement model employed in the Kalman filter may in general be time dependent, missing data can be handled by assigning large measurement error variances to interpolated values (Harvey and McKenzie 1984). Beck (1990, 141-44) described an analysis of Reagan approval based on *New York Times*/CBS Poll data for which about 40% of the monthly data had to be interpolated. Although parameter estimates derived from the Kalman filter (with an assumed error standard deviation of four percentage points in months with interpolated data and zero in months with actual poll results) were generally similar to least squares estimates with the interpolated data treated at face value, the least squares analysis underestimated the (negative) effect of the lagged error term by about 50%.

In addition, the time dependence of the structural parameters in the transition equation make the Kalman filter suitable for estimating models with time-varying parameters (Beck 1983); indeed, Beck (1990, 135-36) noted that this has "probably been the principal application of the S[tate]S[pace]F[orm] in econometrics."

Since careful attention to measurement, time dependence, and error processes have all become increasingly important elements in sophisticated methodological work, the Kalman filter -- a powerful and flexible technique for dealing with all three -- seems destined to achieve increasing prominence in time-series research. As Beck pointed out (1990, 147), "The Kalman filter comes into its own when we actually care about the error process. There is no other technique that apportions the total error variance into measurement error and other sources of error. For evaluating theories such as rational expectations, which make strong predictions about error processes, the K[alman]F[ilter] is invaluable."

3. Time Series of Cross-Sections, Panels, and Aggregated Data

Aggregated Data and Ecological Inference

Political science data are often aggregated over time or units, and these data can present special problems for analysts. When aggregate outcomes can reasonably be thought of as having been produced by "unitary actors," it may be unproblematic to treat them as macro-level data. In cases where aggregate outcomes arise from more complicated individual-level processes, as with votes for political parties, aggregate presidential popularity, or political protests, aggregate-level analyses of covariation are less obviously appropriate, although

nevertheless quite common. This has led some researchers to propose individual-level causal models, aggregate them, and develop techniques for estimating the underlying causal relationships. Both approaches have their place, but researchers are not always clear about which tack they are taking, and this can lead to confusion about whether they are testing individual-level causal theories or describing aggregate-level relationships.

One of the clearest examples of macro-level analysis of aggregated data is MacKuen, Erikson, and Stimson's (1989) article on "Macropartisanship," which proposed that, "Just as party identification is the key concept in studies of the individual voter, its aggregate -- what we term macropartisanship -- is central to theories of party system and voter alignment" (1989, 1125). MacKuen, Erikson, and Stimson examined the determinants of macropartisanship using a long time series constructed from individual-level survey data. Presidential popularity (Mueller 1973; Hibbs 1987), party vote share (Clubb, Flanigan, and Zingale 1980), seats in a legislature (Grofman 1983; King 1989b), and many other aggregate characteristics of political systems have been treated in this way. In some cases, the theoretical framework amounts to little more than a regression equation. In other cases, a macro-level model is proposed, as with Taagepera's (1986) generalization of the "cube law" relating seats to votes in legislative elections.

A related form of macro-level modeling involves using survey data to develop aggregated measures for geographical units. For example, Achen (1977, 1978) and others used survey data to study the relationship between the policy preferences of congressional constituencies and their representatives; Jackson (1990) developed an errors-in-variables method for estimating models using small area data; and Wright, Erikson, and McIver (1987) measured state partisanship and ideology using survey data.

In still other cases, a micro-level model of behavior is developed and then aggregated over individuals to suggest what the corresponding macro-level relationships should be. In these cases, the resulting models can be put to diverse uses. For example, King (1989b) developed a stochastic model of the relationship between seats and votes that relaxes the usual assumption of uniform partisan swing, allowing him to estimate a variety of characteristics of electoral systems. Although King also analyzed the impact of redistricting, his major goal was simply to characterize electoral systems in a useful and parsimonious fashion. In the political economy literature, Kramer (1983) took a different tack. He was fundamentally concerned with making individual-level causal inferences, and he developed an argument as to why aggregate data might provide a better estimate than survey data of the impact of economic conditions on the vote.

Of all these enterprises, Kramer's best reflects the classic problem of "ecological inference," in which individual-level relationships are estimated directly from aggregate-level data. Most of the literature on ecological inference flows directly from Robinson's (1950) article on "the ecological fallacy" and Goodman's (1959) advocacy of "ecological regression." As Erbring (1990, 235) put it, "Ever since Robinson (1950) first shocked a whole generation of social scientists with his demonstration of the 'ecological fallacy,' much has been written about alleged fallacies, biases, pitfalls, and hazards of one kind or another lurking behind aggregate data and about strategies for circumventing them."

On one hand, researchers have attempted to clarify problems of ecological inference by starting from micro-level models and analyzing the difficulties that arise from various forms of aggregation. Hanushek, Jackson, and Kain (1974) identified aggregation bias as a specification error, clarifying in a familiar framework how ecological regression sometimes goes wrong and how aggregation bias can be reduced through careful attention to issues of model specification. Although their results have sometimes been interpreted to mean that ecological regression is consistent if the corresponding individual-level regression is properly specified, Achen (1986a, 2) showed that "coefficients in properly specified micro-regressions are generally biased when estimated with aggregate data." Erbring (1990) further clarified the relationship between specification bias and design effects, emphasizing the role of variance partitioning in aggregation and the connection between the problem of ecological inference and more general problems of research design. Finally, Freeman (1990) described some analogous statistical difficulties arising from temporal aggregation of time-series data, with references to the relevant econometric literature and an example focusing upon empirical studies of superpower rivalry in international relations.

A second approach has been to devise new techniques for mitigating the problems of ecological inference. Often, work of this sort begins with a critique of Goodman's ecological regression technique and then proposes modifications of Goodman's approach. For example, Ansolabehere and Rivers (1992) proposed a way to combine survey and aggregate data to get better estimates of racial voting patterns. Achen (1984) showed that ecological regression applied to constituency-level British election returns yields results that are wildly at odds with the results of individual-level analysis of survey data and proposed a new "quadratic" technique for ecological regression that produces results much closer to the individual-level results. Achen (1983a, 1983b, 1984,

1986a) and Shively (1969, 1974, 1985) have been leaders in efforts of this sort, and their forthcoming book (Achen and Shively n.d.) will no doubt serve as a valuable compendium of techniques for ecological inference.

We now have a better understanding of the problems involved in making inferences from aggregated data, as well as some workable, if imperfect, techniques for dealing with those problems. Achen's (1983a, 85) suggestion still holds: "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." In the meantime, the fact that no technique can compensate fully for the absence of relevant individual-level data should not prevent analysts from utilizing the best available techniques for mitigating aggregation problems; nor should the ultimate intractability of ecological inference prevent us from continuing to strive for understanding in areas beyond the reach of ideally suitable data.

Time-Series Cross-Sectional, Panel, and Pseudo-Panel Methods

Time-series cross-sectional (TSCS) data vary both over units such as government agencies, nations, or individuals and over time. For example, Fischer and Kamlet (1984) studied budgetary allocations for three government agencies over a 27-year period, Pollins (1989) considered U.S. trade with 24 different partners over 24 years, and typical election panel studies (Markus 1982; Conover and Feldman 1989) include from two to five interviews with each of 1,000 to 2,000 people.[10] These designs increase the number of observations, vary both time and context, and provide the opportunity to control for idiosyncratic behavior through the inclusion of dummy variables or lagged endogenous variables. At the same time, they pose some significant methodological challenges.

Consider the most general TSCS linear regression:

$$y_{it} = a_{it} + x_{it}\, b_{it} + e_{it},$$

where y_{it} varies over I units indexed by i and over T time periods indexed by t; a_{it} is a set of intercepts varying by unit and time; x_{it} is a vector of J independent variables; b_{it} is a parameter vector varying by unit and time, and e_{it} is an error term. This model is woefully underidentified, with only IxT observations available to estimate IxT intercepts, IxJxT slopes, and at least one variance for characterizing the error term.[11] Additional assumptions are necessary, and the TSCS literature provides a smorgasbord of possible restrictions and tests for these restrictions. If we assume, for example, that the slopes are constant over time and units, $b_{it} = b$, that the intercepts can be expressed linearly as $a_{it} = c_i + d_t$ to allow for idiosyncratic unit and time effects, that there is no serial or spatial dependence in the error, and that heteroskedasticity results from the varying ability of the independent variables to explain each unit's behavior, then only one slope, I+T intercepts, and I variances must be estimated -- a number less than IxT observations for modest values of I and T. Although TSCS methods have been developed almost exclusively by economists (Hsiao 1986; Judge et al. 1985, chap. 13), political scientists can now refer to the excellent overviews by Stimson (1985) and Sayrs (1989). Stimson's article has been widely cited in the literature, in part because it provides a useful distinction between time-serial dominant (T > I) and cross-sectional dominant (I > T) data.

Many other complications arise with TSCS studies. It often makes sense to include a lagged endogenous variable to control for prior opinions (Markus 1982), last year's budgetary decisions (Fischer and Kamlet 1984), or prior levels of economic development (Alvarez, Garrett, and Lange 1991; Bradshaw and Tshandu 1990). A lagged endogenous variable produces biased parameter estimates if it is measured with error (McAdams 1986), if there is autocorrelation in the equation (Hibbs 1974), or if the adjustment process is not Markov and depends upon more than the last value of the endogenous variable (Beck 1992).

Another difficulty can arise when there are interdependencies across units. In budgetary studies, for example, the sum of the allocations to all agencies is the total budget, so one agency must be dropped from the analysis when the total budget is also being modeled (Kamlet and Mowery 1987). Interdependencies also arise from diffusion or contagion across units. Diffusion and contagion can be considered nuisances and treated as a form of spatial autocorrelation, but they can also be addressed directly by including additional explanatory variables in the analysis (Ross and Homer 1976). Schneider and Ingraham (1984) used this strategy to study social policy innovations across nations. More generally, interrelations or interactions among units can create simultaneities. When the number of units is small, as with Fischer and Kamlet's (1984) two-category budget model, a simultaneous equation system can be estimated in which the dependent variable for each unit enters into the equation for every other unit, but when the number of units is large, as with most cross-national studies, the problem appears very difficult.

The most sophisticated TSCS models are found in the budgeting literature and in panel studies. Starting with theories of incremental budgeting, a great deal of attention has been paid to budgetary decision making (Berry and Lowery 1990), with simultaneous equation

(Fischer and Kamlet 1984) and switching regressions (Kiewiet and McCubbins 1988) used to model the annual budgetary battle between the president and Congress.

In panel studies, simultaneities (Markus 1982) and errors in measurement (McAdams 1986) have been staples of concern, and sophisticated models have been developed to study opinion change (MacKuen 1984; Bartels n.d.). For example, Bartels (n.d.) used panel data from the 1980 American National Election Study to estimate the political impact of media exposure in a presidential election campaign. In addition to using the panel design to estimate and adjust for the effects of measurement error, he developed an estimation strategy in which opinion reports at two or more points in time could be used in the context of a simple Bayesian model of opinion change to characterize the "distinctive messages" received by those exposed to television news and newspaper coverage of the campaign in the intervening period. In this case, as in other cases where individual opinions are quite stable over relatively short periods of time, the availability of direct, individual-level measurements of prior opinion greatly increases our ability to relate opinion changes to individual differences in media exposure, political predispositions, physical location, and other factors.

Perhaps the most exciting opportunity for innovation by political scientists lies with analyzing repeated random cross-sections. Unlike standard time series of cross-sections or panel studies, in these survey designs different individuals are interviewed in each time period. The resulting data can be analyzed by simply taking aggregate statistics for each cross-section and treating them as a time series, as in Allsop and Weisberg's (1988) study of macro-partisanship over the course of the 1984 presidential campaign. Of course, this approach throws away a great deal of information that might be better exploited in a pooled TSCS analysis, but it is often hard to know how to use that information to full advantage. In their study of the 1988 Canadian campaign, Johnston et al. (1992, Appendix B) proposed two different TSCS methods for using this additional information.

More work is needed to develop methods that utilize the full power of this design. However, one such method has already been developed: Franklin's (1990) "two-stage auxiliary instrumental variables" estimator. In Franklin's approach, pseudo-panels are constructed by using common variables in two or more separate data sets. Suppose, for example, that we want to know the relationship between party identification in 1960 and 1964. We have no panel of respondents covering this period, but we do have separate cross-sections in the two election years. Using the 1960 data, we can estimate party identification as a function of a set of instrumental variables (demographic variables and other fixed characteristics) available in both data sets. Then, using the coefficients from the 1960 equation for party identification, we can construct estimates of 1960 party identification for each person in the 1964 cross-section. Finally, we can regress 1964 party identification on estimated 1960 identification to determine, for example, whether a realignment occurred in the intervening period. Franklin (1990) showed the conditions under which this procedure makes sense and also derived the standard errors of the resulting parameter estimates.

4. Techniques Tailored to Measurement Properties of Data

Polychotomous Data and Polytomous Choice

With the admonitions of Aldrich and Nelson (1984) in mind, political scientists confronted with dichotomous dependent variables now routinely use logit or probit, make Goldberger corrections for the linear probability model, or at least apologize for the limitations of ordinary least squares. In practice, these three methods often produce very similar results, and the OLS model has the great virtue of being easy to estimate and to interpret (Achen 1986b). In fact, given the difficulty of interpreting probit and logit estimates (Nagler 1991; Denk and Finkel 1992), there is a great deal to be said for OLS.

Dichotomous variables, of course, are only the simplest form of polychotomous or polytomous choice data. Jacoby's (1991) review of measurement theory surveys many other possibilities. Economists (Maddala 1983) and psychometricians (Muthen 1984; Rasch 1980) have developed a variety of methods for analyzing these kinds of data. Political scientists have begun to adopt some of the methods developed by economists: Tobit for truncated variables like the amount of welfare paid by local communities (Sharp and Maynard-Moody 1991), nested logit for multistage decision processes of international agencies and voters (Hansen 1990; Born 1990), switching regressions to capture presidential and congressional interaction over budgetary matters (Kiewiet and McCubbins 1988), and multivariate logit (Franklin and Jackson 1986; Jackson and King 1989) and probit (Ostrom and Simon 1988) for simultaneous equations. Techniques developed by psychometricians are not as widely known or used.

In addition to borrowing methods from other disciplines, political scientists have developed some innovative and important techniques of their own for analyzing polychotomous data. Most notably, McKelvey

and Zavoina (1975) extended the familiar probit model to deal with ordinal, rather than simply dichotomous, dependent variables. The ordinal probit model has been used to study the partisanship of Latinos and Asian Americans (Cain, Kiewiet, and Uhlaner 1991), opinions on abortion (Franklin and Kosaki 1989), and many other political phenomena.

In an especially interesting application, Krehbiel and Rivers (1988) synthesized the ordinal probit model and an ideal point model of congressional roll-call voting. In Krehbiel and Rivers's approach, the ordinal information contained in a series of congressional roll-call votes, in combination with a set of observed covariates -- characteristics of legislators and districts -- makes it possible to obtain consistent estimates of individual legislators' ideal points in just the same way that ordered probit can be used to estimate scale values.[12] Krehbiel and Rivers presented both unidimensional and multidimensional variants of their method, and they developed approaches both for the case where there is some auxiliary information on the locations of bills (for example, the wage levels proposed in each of a series of minimum wage bills) and for the case in which no such information is available. Bartels (1991a) used Krehbiel and Rivers's model to estimate the impact of constituency opinion on congressional defense appropriations votes in the first year of the Reagan defense buildup, using the levels of spending proposed in each of a series of alternative appropriations bills to calibrate the spending preferences of members representing congressional districts with varying levels of public support for increased defense spending.

Several other innovations by political scientists are also worth noting. Rivers (1988) developed a method for estimating models with parameter heterogeneity using rank-preference data. Brady (1989, 1990) showed that standard factor analysis applied to interpersonally incomparable ordinal rankings produces misleading results and developed a model for the dimensional analysis of these data. Rivers and Vuong (1988) developed a new two-step maximum likelihood estimator for simultaneous probit models. And Brady and Ansolabehere (1989) developed a threshold model of paired comparison choices and estimated a trinomial probit model with the threshold parameterized by political knowledge and preferences parameterized by a vector of explanatory variables.

Thus, political methodologists, drawing in part upon the distinctive problems, data, and models of political science, have made significant contributions to the analysis of polychotomous data and polytomous choices. The methods they have developed, along with the various methods developed by econometricians and, especially, psychometricians, await fruitful application in a variety of areas of political research.

Event Count and Event History Models

Event counts are ubiquitous and important in political science. In order to understand the various statistical issues arising in analyses of these data, it is helpful to begin with a general statistical framework. Let y_i be the number of events recorded for each observation i, say the number of vacancies on the United States Supreme Court in each year since 1787 (King 1987), and let $\mathbf{x}_i$ be a vector of explanatory variables related to the count, such as the number of seats on the Supreme Court, the degree of electoral turnover, or the time since the last vacancy. The $\mathbf{x}_i$ might also be just a constant or a set of dummy variables for years. Then we can write:

$$y_i = f(\mathbf{x}_i \mathbf{b}) + e_i,$$

where f is a function, $\mathbf{b}$ is a set of parameters, and e_i is an error term. As written, almost any data can be fit to this non-linear model. To be of interest, we must make additional assumptions about the functional form f, the vector of independent variables, and the error term. The classic Poisson model is based upon three assumptions. First, the dependent variable y_i is assumed to follow the Poisson distribution with mean $f(\mathbf{x}_i \mathbf{b})$. Second, the function f is assumed to be the exponential, so that $f(\mathbf{x}_i \mathbf{b}) = \exp(\mathbf{x}_i \mathbf{b})$, ensuring that the expected number of events in each time period is positive. Third, there is assumed to be only one explanatory "variable" x_i with a constant value of one, so that every observation is generated by a Poisson process with the same mean, exp(b).

There is a substantial tradition, dating back at least to Wallis (1936) and Richardson (1944), of using the distribution of event counts produced by a Poisson process as a baseline model for studying events data.[13] The Poisson process is a good baseline model because it only requires a very simple set of assumptions about how events occur: that the probability of an event is constant over time (temporal homogeneity) and across units (actor homogeneity) and that events occur independently of each other (without diffusion or contagion) and with low probability in each time period. Under these assumptions, the number of events per time period can be shown to follow a Poisson distribution (see King 1988 for an accessible proof).

Of course, each of these assumptions will be implausible for many applications.[14] There are ample reasons to believe that the probabilities of events vary by time and unit of observation; this suggests that the homogeneity assumptions are too strong. Independence of events also seems implausible in many situations where diffusion is a strong possibility. Analyses of policy innovations in the American states (Walker 1969; Gray 1973), oil nationalization (Kobrin 1985), social security

(Collier and Messick 1975), and coups (Putnam 1967; Midlarsky 1970; Li and Thompson 1975) provide abundant evidence of diffusion. Thus, going beyond assumptions of homogeneity and independence has been the major challenge facing analysts of events data during the past fifteen years.

Fortunately, there are several ways to generalize the classic Poisson model, depending upon whether one focuses upon the error term e_i, the functional form $f(\mathbf{x}_i \mathbf{b})$, or the vector of attributes $\mathbf{x}_i$.

Until the last decade, the major focus has been on generalizing the error term.[15] As early as Richardson's (1944, 243-44) work, there was a realization that plausible violations of the Poisson process model, such as contagion or heterogeneity, would lead to other distributions, such as the negative binomial. By now there are many sophisticated and clever analyses of event counts in which alternative distributions are fit to data on war (Davis, Duncan, and Siverson 1978; Most and Starr 1980; Levy 1982; Siverson and Starr 1991), alliances (McGowan and Rood 1975; Siverson and Duncan 1976; Job 1976; Midlarsky 1983), terrorist acts (Hamilton and Hamilton 1983), urban riots (Midlarsky 1978), and elite turnover (Casstevens 1989).

However, this research strategy seems to us to face three insurmountable problems. First, since real data never fit any distribution exactly, it is hard to determine how good a fit is good enough to be useful. Second, some hypotheses (such as actor heterogeneity and addictive contagion) lead to identical distributions, so that it is impossible to distinguish them. And, third, fitting distributions does not provide any indication of what might be causing heterogeneity or contagion. (For a related discussion of diffusion, see Ross and Homer 1976.)

These difficulties lead naturally to a shift away from generalizing the error term e_i in favor of generalizing the functional form $f(\mathbf{x}_i \mathbf{b})$. King (1988, 1989a, 1989c, 1989d) employed this strategy to develop an increasingly general set of "Poisson regression" models. In his first paper, King (1988) described the Poisson regression model, argued for the utility of the exponential functional form, demonstrated the biases that can arise from using ordinary least squares, and presented an empirical example. In this initial paper he argued (1988, 858-59) that "the underlying mathematical process generating event count data is driven by the Poisson distribution," but a year later (King 1989d, 763) he took the position that "the Poisson regression model...makes two key assumptions about the way unobserved processes generate event counts [homogeneity and independence] that are implausible in many applications....If these assumptions do not hold, but the Poisson model is applied anyway, parameter estimates will be inefficient and standard errors inconsistent, a situation analogous to heteroskedasticity in least squares models." King developed models allowing for temporal heterogeneity and contagion based upon the negative binomial for overdispersion and the continuous parameter binomial for underdispersion. The result is a generalized event count (GEC) distribution which produces estimates without any a priori assumption about under- or over-dispersion. Finally, in a wide-ranging paper on event count models for international relations, King (1989a) reviewed his earlier contributions and described several new models including a "hurdle" model in which the onset of war is modeled separately from its escalation, a truncated model in which only episodes of international sanctions are observed, an extension of this model in which the variance of the distribution is parameterized in terms of a vector of independent variables, and a model for Poisson regressions with correlated errors.

King generalized the basic Poisson model in all three directions noted above -- the error term, the functional form, and the vector of independent variables -- and also made some progress toward modeling multiple equation systems. His models provide a very useful package of techniques for dealing with event counts, especially since he has programmed his methods into a set of computer routines for the GAUSS statistical package.

At the same time, there is more work to be done in at least two directions. First, under- and over-dispersion might be considered specification problems in the same way that heteroskedasticity and autocorrelation are often considered failures of specification in regression models. From this perspective, the GEC model should be only a way-station to a better specification. Second, more work has to be done to derive event count distributions from first principles -- from the underlying stochastic processes generating the data. This is not an easy task; Tuma and Hannan (1984, 305) noted that "the probability mass function for the count of events...has apparently not yet been derived for a general...Markov model." Indeed, the difficulty of deriving event count distributions from more basic models led Tuma and Hannan to recommend using event histories in preference to event counts whenever the former are available.

Event history analyses have become more common in political science in the past decade. The study of cabinet durations illustrates both some of the pitfalls and some of the possibilities of these analyses. Initial efforts (Browne, Frendreis, and Gleiber 1984, 1986; Cioffi-Revilla 1984) failed to capitalize fully on the strengths of stochastic modeling, being based upon simple Poisson models with constant rates of dissolution. The simplicity of these models probably accounts for Strom's characterization of them as "disappointing and

inadequate" (Strom et al. 1988, 929). King et al. (1990) went a long way towards meeting such objections by developing a stochastic model in which the probability of cabinet dissolution varies with a series of observable explanatory variables.

Event history methods have been applied in other areas as well. Bienen and van de Walle's (1989) proportional hazard model of leadership duration in Africa and Strang's (1991) analysis of decolonization from 1500 to 1987 both allowed for changes in the probability of events over time and as a function of measured covariates. In their study of state lottery adoptions, Berry and Berry (1990) used a pooled time-series cross-sectional probit method to estimate a discrete time hazard rate model described by Allison (1984). These efforts and others like them bode well for the future of stochastic modeling in political science. Useful book-length introductions to the relevant models and methods include Tuma and Hannan (1984), Allison (1984), and King (1989c).

5. Measurement Error and Missing Data

Problems of measurement were central to the intellectual agenda for political methodology set out by Achen a decade ago. "One thing political methodologists *have* done," he wrote (1983a, 70), "is to show that measurement does matter." Political methodologists have made significant additional progress on problems of measurement in the intervening decade. Explicit attention to measurement in the formulation and estimation of statistical models, though by no means standard, is an increasingly common feature of our best empirical work. Structural equation models with latent (unobserved) variables (Bollen 1989) have come into widespread use.[16] Jöreskog and Sörbom's LISREL software, now available within SPSS, is only one of several available computer programs (EQS, LISCOMP) for estimating such models. On the theoretical side, our understanding of proxy variables, though far from complete, is considerably more advanced than it used to be. And problems of measurement outside the classic errors-in-variables and scaling frameworks inherited from econometrics (Fuller 1987) and psychometrics (Jöreskog and Sörbom 1979) are beginning to be recognized and tackled.

The Consequences of Random and Non-Random Measurement Error

Achen (1983a, 71-74) provided a summary and extension of what was known at the time about the effects of random measurement error in explanatory variables. He cautioned against the false but widespread belief that measurement error simply attenuates regression coefficients, so that "if one can establish the existence of an effect in spite of measurement error, the true effect must be even larger." Explicit attention to the consequences of measurement error has recast our substantive understanding of many political phenomena, including the stability (Achen 1975; Erikson 1979) and generational transmission (Dalton 1980) of political attitudes, the role of party identification (Franklin and Jackson 1983; Green and Palmquist 1990), and the impact of media exposure (Bartels n.d.).

Achen also pushed political scientists to go beyond the standard assumption of random measurement error by showing (1983a, 75) that when measurement errors are not random, literally anything can happen: "for any arbitrary coefficient vector β^*, there exists a systematic error structure...that will generate it." This sweeping negative result demonstrated -- if any formal demonstration was necessary -- that it is futile to hope for a solution to the problems of inference inherent in analyzing data with non-random error, except insofar as we can bring to bear some specific knowledge of the relevant error structure.

Later, Achen (1985) identified a particularly perverse and previously unrecognized consequence of non-random measurement error. He showed that, if two proxies for a single latent variable are included in a regression model, the regression coefficient for the less reliable of the two proxies will tend to have the wrong sign, even in large samples, if the measurement errors in the proxies are sufficiently correlated. Only the combined effect of both proxies is interpretable in this case. What is worse, Achen showed that sign reversals can occur even when the measured variables are proxies for distinct latent variables (so that their individual effects are of direct substantive interest), if the measurement errors are sufficiently correlated and the latent variables themselves are also sufficiently correlated. Thus, "the common practice of using a long series of regression 'control variables' which are inexact proxies can be a misleading data-analytic technique. Such variables not only bias the estimated effect of non-proxy variables, as is well known, but in the presence of correlated errors, they may severely distort or even reverse their own estimated effects as well" (Achen 1985, 310).

Other analysts have derived the consequences of random and non-random measurement error in regression

models of varying generality. Green (1991) analyzed the effects of measurement error in the context of a simultaneous equation model of party identification and political attitudes. Cowden and Hartley (n.d.) analyzed the effects of using complex measures that proxy for more than one latent variable in an ordinary regression analysis. And Palmer (1992) derived similar results from a somewhat more general model in which a source of correlated measurement error also appears (or belongs) as an explanatory variable in its own right in the structural equation of interest.

Guessing and Other Sources of Error in Survey Responses

These results of the consequences of random and non-random measurement error reinforce Achen's message (1985, 311) that in order to make any real progress surmounting the inferential problems posed by measurement error, we "must know something about the error structure a priori." One especially promising source of a priori information about the error structures of political data is careful consideration of our measuring instruments themselves. Rather than simply tacking an additive random error term onto an explanatory variable, as in the standard econometric approach, a few analysts have begun to develop models recognizing and exploiting the fact that some specific kinds of measurement error are more likely than others.

For example, Brady (1985a) and Green (1988) analyzed non-random measurement error resulting from response sets -- different people using response scales in idiosyncratic but consistent ways across items. The general setup used by Brady and Green is similar to the assumption of a linear transformation of true underlying opinion made by Aldrich and McKelvey (1977). In models of this sort, even if true responses are independent of one another, survey responses across two items may be correlated if some people always use the upper part of a scale and other people always use the lower part of the scale. Green (1988) corrected for these effects and found that liberalism-conservatism may be unidimensional and not multidimensional as argued by Conover and Feldman (1981). Brady (1990) showed that because rankings of candidates are neither interpersonally comparable nor interval measures, standard factor analysis methods can be very misleading. He proposed a method that just uses ranking information and applied it to candidate rankings collected from Democrats who attended the 1984 Iowa caucuses.

In a similar spirit, Brady and Sniderman (1985, Appendix 2) proposed a "guessing" model in which those without opinions on seven-point issue scales were assumed to choose responses centered on the midpoint of the scale. Their scaling method produced an estimate of 8.5% guessers. Jackson (1992b) took a somewhat different approach, modeling the probability of various observed survey responses -- or non-responses -- as a function of two unobserved variables representing, respectively, latent preferences and latent non-opinion. For example, Jackson's analysis of responses to an item about school integration in the 1956 American National Election Study survey found that a substantial proportion of respondents without real opinions nevertheless agreed or disagreed with the statement that "The government in Washington should stay out of the question whether white and colored children go the same school," despite the presence of both a filter question asking whether respondents had thought much about this issue and a "not sure" response option. What is especially interesting is that these "pseudo-responses" were not equally distributed between the "agree" and the "disagree" categories but were much more likely to be "agree" responses, producing a significant misrepresentation of the division of "genuine" public opinion on the issue.

On the surface, these guessing models might seem similar to standard measurement error models, but they can have substantially different implications. For example, it is not hard to show (Brady 1993) that in a population in which some fraction of the population follows a Wiley and Wiley (1970) autoregressive model with any arbitrary amount of attitude stability, and the remainder simply guess by choosing randomly from some distribution with a mean at the center of the scale, the standard Wiley-Wiley estimation procedure will produce parameter estimates that are indistinguishable from those produced when the entire population follows the Wiley-Wiley model. This is true as long as the fraction of non-guessers is not zero, and it is true no matter how many waves of data are available. In short, the Wiley-Wiley model is fundamentally unidentified, and stability measures of 1.0 are consistent with a population in which nearly everyone guesses.[17]

The implication of these results is that high estimated stability coefficients alone are not very informative about the substance of political opinion. Higher and higher piles of computer output demonstrating that attitudes are extremely stable in the sense measured by Wiley-Wiley and other measurement models -- as in Krosnick's (1991) industrious analysis of hundreds of correlations among issue items in three different data sets -- are completely consistent with the guessing model described above with an arbitrarily high fraction of guessers. At this point, we do not need more such data analyses, but better models of how measurement error arises and what it represents.

Non-Random Samples and Sample Selection Bias

Political scientists commonly work with sets of observations that are non-random samples from some population of interest. In opinion surveys, for example, an inappropriate sampling frame may make some people more likely than others to be selected for interviewing; differences in lifestyles may make some of those selected more likely than others to actually be contacted by an interviewer; differences in political interest may make some of these contacted more likely than others to cooperate. Comparable problems arise in many other research settings.

This non-random selection will generally bias the results of cross-tabulations, regressions, and other statistical analyses. The pitfalls of "selecting on the dependent variable" have been widely, if vaguely, understood for some time. But the broader significance of selection bias was unrecognized, and statistical corrections for its effects were unknown, until Heckman (1976, 1979) showed how to treat censoring as a specification error in the familiar econometric framework.

Heckman's technique proceeds from the observation that the mean of a dependent variable conditional upon selection into the sample may depend in a systematic way upon the sample selection process. If we can model this dependence, then we can distinguish between structural effects and sample selection effects. In much the same way that instrumental variables are required to distinguish reciprocal effects in simultaneous equation models, the key to successful application of Heckman's technique is that there must be available a variable (or set of variables) significantly related to selection into the sample, but unrelated to the dependent variable of substantive interest. Achen (1986b, chaps. 4 and 5) provided a thorough intuitive and technical treatment of the bias engendered by censoring and of Heckman's correction, as well as an extension from the probit framework to the linear probability framework.

Although much empirical work still proceeds regardless of the pitfalls of analyzing data from censored samples, sensitivity to these pitfalls has increased. In the field of survey research, designers and analysts of the 1984 American National Election Study went to some lengths to estimate the impact of non-response; Brehm (1990) provided a comprehensive report of the results in the context of a more general discussion of survey non-response. Gelman and King (1990) applied the logic of selection bias to the problem of estimating the electoral advantage of congressional incumbency from observed election outcomes. Jackman and Vella (1992) looked for selection bias in estimates of partisan bias and responsiveness in redistricting plans. Geddes (1991) described the pitfalls of making inferences from censored samples in cross-national comparative studies. And Achen and Snidal (1989) criticized the use of non-randomly selected case studies in the study of deterrence in international relations.

Missing Data

Sample selection bias of the sort addressed by Heckman and Achen is only one of a variety of missing data problems plaguing political research. It is the one most frequently addressed, not because it is necessarily the most important but because a coherent and reasonably straightforward solution is available within the familiar econometric framework. The more common case of partially missing data -- in which some but not all of the data for a given observation are available -- is both less well understood theoretically and less well handled empirically.

The simplest technique for dealing with missing data is probably also the most common in political science: throw out the observations for which any data are missing and analyze only complete observations. Of course, the resulting loss of information can be considerable. In a typical analysis of issue positions in an election survey, for example, half or more of the respondents may have data missing on one or more issues. What may be worse, the non-randomness of the subset of respondents for whom complete data are available may induce serious sample selection bias of the sort described above.

An alternative approach is to impute values for missing data. For example, analysts sometimes substitute the sample mean for any values that are missing -- mean imputation. Usually it is preferable to substitute some other value that makes use of the non-missing data for the observation, such as the conditional mean from a regression analysis -- regression imputation -- or the observed value from a matched observation in the sample -- hot deck imputation. When the dispersion of a distribution is of interest, it may make sense to impute missing values by random sampling from some appropriate distribution -- stochastic regression imputation.

As a discipline, we are remarkably ignorant of the benefits and pitfalls of these various approaches. The result is that imputation of missing data is relatively rare in political research and is seldom documented in sufficient detail to be replicable or justified on any good methodological grounds. Although the statistical theory of missing data has many unsolved problems, results of

the sort provided by Little and Rubin (1987, chaps. 3 and 4) offer a considerable statistical basis for dramatic improvements in the current state of empirical work.

The most sophisticated treatment of missing data in the political science literature to date appears in Mebane's (n.d.) analysis of the effects of U.S. local government fiscal activity. The Census Bureau's Census of Governments data sets provide detailed information on intergovernmental transfers for 80,000 local governments at five-year intervals, while Surveys of Governments in other years provide similar information for samples of a fifth to a third of these local governments. In order to estimate the effects of local fiscal activity, Mebane had to pool these two sources of information to estimate total intergovernmental transfers for all local governments in a given area in each year.

Mebane implemented a three-step estimation procedure combining features of the sample selection and measurement error corrections proposed by Heckman (1976, 1979) and Fuller (1987), respectively. In the first stage, Mebane estimated the probability that a given local government would be included in a given annual Survey of Governments sample; given the Census Bureau's sampling procedures, these probabilities are influenced (but not entirely determined) by the size of the government and its level of fiscal activity. In the second stage, Mebane used the estimated probabilities of inclusion from the first stage to impute fiscal totals for each area from the incomplete observed data by reweighting the observed totals in each area by the ratio of expected to actual observations; in addition to providing an estimate of the total fiscal activity in each area, this stage provided estimates of the measurement error in these estimated totals (which varied with the proportion of governments actually observed in a given area). In the third stage, Mebane outlined a procedure for correcting parameter estimates from a simple regression model in which local fiscal totals appeared as explanatory variables for biases resulting from sample selection (for areas in which no governments happened to be selected for observation) and measurement error (in areas where some but not all local governments were selected for observation), using variants on the approaches of Heckman and Fuller, respectively.

Although the details of Mebane's analysis reflect many specific features of the substantive problem he set out to address, the general outlines of his approach are clearly relevant to a broader class of problems in which missing data play an important role.

6. Dimensional Analysis

Since the pioneering work of Rice (1928), Thurstone (1932), and Gosnell (1937), dimensional analysis has been used to study electoral preferences and legislative roll-call votes. The field continues to be driven by the availability of voting data, candidate evaluations (often "thermometer" scores), and issue placements of candidates from sample surveys and roll-call or interest group ratings from legislatures. In voting and legislative research, the major issues have been the fit of the models, their underlying dimensionality, the interpretation of these dimensions, their stability over time, and the centrality or non-centrality of the candidates with respect to the voters or the members (or committees) relative to the legislature as a whole. In each of these areas, political scientists have developed original methods for estimating these models, and they have used these methods to provide statistical tests of important political questions.[18]

Voting Studies

The point of departure for most voting studies has been the "ideal-point" model of preference inspired by the psychometric literature (especially Coombs 1964) and the spatial modeling literature (Downs 1957; Davis and Hinich 1966).[19] This model presumes that voter i's "utility" U_{ij} for a candidate j is a function of the distance between the voter's positions V_{it} on T different dimensions of concern, and the candidate's locations C_{jt} on these same issues:

$$U_{ij} = - \Sigma_{t=1,T} (V_{it} - C_{jt})^2 + e_{ij}$$

where e_{ij} is an "error" term including any idiosyncratic influences on voter i's evaluation of candidate j. In this equation, as the distance between the voter and the candidate increases, the voter's evaluation of the candidate gets lower. This simple model has been generalized to other functional forms, and variables for "valence" issues -- about which everyone agrees that more is better -- have been added in some versions. Thus, a more general formulation is

$$U_{ij} = - [\Sigma_{t=1,T} (V_{it} - C_{jt})^r]^s + a X_j + e_{ij}$$

where r and s are exponents providing a flexible functional form and X_j is a vector representing valence issues such as competence or character. The values of r and s define the way that distance is measured: if $r=2$ and $s=1/2$, then the distance is Euclidian; the value $s=1/r$ defines a more general type of distance called the "Minkowski" metric. Still other values are possible, such

as the squared Euclidian distance (r=2, s=1). Research in the past twenty years varies in the kinds of assumptions that are made about the functional form, whether or not valence issues are included, the nature of the error term, and the properties of the measure used for utility. There is also variation in the degree to which these assumptions are made explicit.

Rabinowitz (1976, 1978) developed and applied a novel "line of sight" procedure to obtain unbiased similarity measures for candidate pairs, which were then used as the basis for non-metric multidimensional scaling (NMDS). Whereas previous authors had argued over whether Pearson, Spearman, or some other correlations should be used for scaling procedures (e.g., Weisberg 1974b), Rabinowitz started from specific ideal point models (Euclidian and more generally Minkowski metrics) and developed a new and more appropriate measure of similarity. This measure was the basis for a NMDS procedure (Rabinowitz 1976) using Kruskal's (1964) "stress" as a measure of fit. Rabinowitz (1978) reproduced Weisberg and Rusk's (1970) two-dimensional representation of candidate positions; using these candidate positions, he estimated voter ideal points and showed that, contrary to the expectations of the median voter theorem (Black 1958), there was no candidate located in the center of the distribution of voters. Kruskal's stress is an ad hoc measure of fit, so it is hard to evaluate these results, but Brady's (1985b) statistical framework for NMDS provides one way to obtain standard errors of estimates and tests for goodness of fit.

Hinich and his collaborators (Cahoon 1975; Cahoon, Hinich, and Ordeshook 1978; Enelow and Hinich 1984, Appendix 9.1) took a much different approach to estimating spatial models of elections. Unlike Rabinowitz, they made strong metric assumptions about the measurement properties of thermometer scores, and they then cleverly modified factor analysis methods to obtain statistically consistent estimates of candidate positions for a squared Euclidian model (r=2, s=1). Their empirical work confirmed the social and economic dimensions found by previous scholars, and in later work they provided evidence for a "valence" dimension such as "integrity, executive ability, compassion, and intelligence" (Enelow and Hinich 1984, 174) about which all voters feel the same way. After obtaining candidate positions in the space, Enelow and Hinich used a procedure analogous to the estimation of factor scores to estimate voter ideal points. Using these estimates, they argued (Enelow and Hinich 1984, 222) that "major American political figures are not far away from the center of the electorate."

Poole (1981, 1984) developed a metric unfolding method for the Euclidian model (r=2, s=1/2), and Poole and Rosenthal (1984) applied it to thermometer evaluations. Poole's technique locates voters and candidates simultaneously, and it provides a very efficient algorithm for obtaining estimates. Rivers (1987) and Brady (1991b), however, have argued that this method can yield statistically inconsistent estimates for both candidate and voter positions. Intuitively, the difficulty is that voter positions are essentially factor scores and there is a well-known indeterminacy in their estimation (Macdonald 1985) that corrupts the estimates of candidate positions as well.[20]

These papers have made substantial advances in our understanding of the spatial structure of candidate evaluations. Yet, except for the use of stress and other similar R-squared kinds of measures for the adequacy of the scaling method, no statistical measures of fit and no standard errors are reported for these methods. Brady (1991a) provided a "computationally simple, statistically consistent, reasonably efficient, and statistically informative generalized least squares (GLS) estimator for a general class of nonlinear multidimensional scaling (MDS) models including the 'ideal-point' models of voters' and legislators' behavior proposed by Melvin Hinich, Keith Poole and others" (1991, 97). The approach described by Brady assumes that thermometers are metric ratings and provides a statistical framework for testing a wide range of hypotheses about these models, including their functional form, their dimensionality, and the values of specific parameters. Moreover, Brady showed that a T-dimensional squared Euclidian model can be estimated using LISREL as a constrained T+1 dimensional factor model. This provides a very simple way to estimate this model and to obtain all the statistical information provided by LISREL, including standard errors of parameter estimates and a χ^2 goodness-of-fit statistic.

Applying his method to 1980 National Election Studies data, Brady showed that the ideal point model outperforms a factor model and that candidates are far away from the median voter. Using LISREL, it would be a simple matter to use this model to perform a variety of statistical tests, including tests for the constancy of candidate positions and of the distribution of voter ideal points over several waves of a panel. In two other papers, Brady (1989, 1990) considered rankings of candidates for which metric techniques are clearly inappropriate. These papers also provide a statistical basis for analyzing these data.

Aggregate voting data have also been scaled using principal components analysis (Rabinowitz, Gurian, and Macdonald 1984; Rabinowitz and Macdonald 1986; Macdonald and Rabinowitz 1987). This work yields two basic dimensions representing ideology and party. Rabinowitz and Macdonald (1986) showed that this approach yields reasonable estimates if every voter across all the states and all the years makes ideal point calculations using the same dimensions, if there are no

idiosyncratic factors like the e_{ij} above, and "if the distribution of the voters in each of the 50 states is multivariate normal, with states having different mean locations but identical covariance structures." These assumptions are a reasonable starting point, but there are good reasons to believe that idiosyncratic factors do matter and that covariance structures might differ substantially across states. For one thing, it is easy to show that a large number of precincts with different means and otherwise identical multivariate Normal distributions of voters will produce an aggregate distribution that is not multivariate Normal, but a mixture of Normals. This suggests that the multivariate Normal assumption is a very special assumption.

Perceptual Studies

Since the 1970s, the National Election Studies have asked citizens to place themselves and candidates on seven-point issue scales. These data provide a way of mapping how citizens perceive themselves and the candidates. They also provide a more direct way of determining whether vote choices can be explained in terms of voter distances from candidates, instead of generating candidate and voter positions indirectly from voter choices or candidate evaluations, as with the scaling methods described above. One disconcerting feature of these placements, however, is that for the same issue and candidate, perceptions vary substantially from one person to another. One person will see Ronald Reagan as tough on crime, while another will view him as easy on crime. This has led researchers to develop models of these responses. Aldrich and McKelvey (1977) developed the first sophisticated model of response in which a person i's placement Y_{ijm} of candidate j on issue m is a linear transformation of some underlying true position Y_{jm} of the candidate:

$$Y_{ijm} = a_{im} + b_{im} Y_{jm} + d_{ijm}$$

where a_{im} adjusts for the fact that different people may anchor the scale in different ways, b_{im} adjusts for differences in the range of the scale used by otherwise identical people, and d_{ijm} represents random error in perceptions of the candidate's position. Aldrich and McKelvey suggested a method that simultaneously estimated both the locations of candidates and the voter characteristics a_{im} and b_{im}, but they did not provide any proofs of its statistical properties. They found that, except for Wallace in 1968 and McGovern in 1972 on Vietnam, candidates were close to the median voter. Palfrey and Poole (1987) used the Aldrich-McKelvey method to consider the relationship between ideological extremism and voting behavior.

Enelow and Hinich (1989) modified the Aldrich and McKelvey model by assuming that a small number T of candidate positions C_{jt} underlie the true candidate positions Y_{jm} on each issue:

$$Y_{jm} = \Sigma_{t=1,T} C_{jt} f_{tm},$$

where f_{tm} is the degree to which dimension t underlies issue m. This is then substituted into the equation for Y_{ijm} above:

$$\begin{aligned} Y_{ijm} &= a_{im} + \Sigma_{t=1,T} b_{im} C_{jt} f_{tm} + d_{ijm} \\ &= a_{im} + \Sigma_{t=1,T} g_{imt} C_{jt} + d_{ijm}, \end{aligned}$$

where $g_{imt} = b_{im} f_{tm}$. Enelow and Hinich assumed that the g_{imt} are produced by random variables that are independent across dimensions t for each issue m. Once a_{im} is subtracted off each observed candidate issue position, a factor analysis of the covariances across candidates for each issue should yield the same candidate positions C_{jt}. Enelow and Hinich (1989, 464) claimed that "very little change could be detected across these factor analyses," so they pooled the data across all issues to obtain estimates of the covariances for their final factor analysis. They found two dimensions and that "the candidates lie relatively close to the average voter and exhibit little movement over the campaign" (1989, 461).

Enelow and Hinich's model has the virtue of utilizing methods for which we have well-developed statistical theory; but that virtue has not yet been fully exploited. For example, Enelow and Hinich did not test for the stability of candidate positions across issues, as they could have done using LISREL for simultaneous factor analysis in several populations (Jöreskog and Sörbom 1979, chaps. 7 and 8). A more complete integration of available methods, with more attention to standard errors and statistical tests, will greatly facilitate substantive interpretation of the results of these analyses.

More generally, direct scaling of votes, candidate evaluations, or perceptual items almost invariably lead to two or three basic dimensions, but the interpretation of these dimensions varies markedly from study to study, with substantially different conclusions about whether or not candidates are near the median voter. Hinich and his collaborators have consistently found that candidates are near the median, whereas Rabinowitz, Poole, Brady, and others have found that candidates tend to be far away from the center of the voter distribution. To reconcile these different results and interpretations, we must pay more attention to statistical hypothesis testing, and we must extend our scaling models, possibly along the lines suggested by Brady and Sniderman (1985), to include more ancillary information -- voter issue positions, candidate

perceptions, and socioeconomic variables -- in order to refine and better interpret our results.

Legislative Studies

The major strides in legislative roll-call analysis during the 1980s have been undertaken by Poole, Rosenthal, and their collaborators, who assembled some extraordinary datasets, overcame significant computational problems, and generally improved our understanding of the American Congress. Poole's metric unfolding technique (1981, 1984) has been used to analyze interest group scores of legislators (Poole and Daniels 1985).[21] Poole and Rosenthal (1985) have also utilized another scaling method for the direct analysis of roll-call votes. Their initial NOMINATE program assumes a one-dimensional space and that "each roll call is a choice between two points on the dimension -- one point represents the outcome corresponding to a yea vote and the other point represents the outcome corresponding to a nay vote" (1985, 360). Legislators calculate their utility for each point based upon an ideal point model, their own position on the one dimension, and idiosyncratic factors represented by an error term with a Weibull distribution. The probability that the legislator votes yes or no is given by an analytical expression involving exponentials, which is used to form a likelihood function across all legislators and all roll calls.

In later versions of this program, called D-NOMINATE, Poole and Rosenthal (1991) extended the method to consider multiple dimensions and change over time. As with the metric procedure, Poole and Rosenthal have only provided Monte Carlo simulations and not analytical proofs of the statistical properties of their method.[22]

In addition to concerns about the statistical properties of their methods, two other criticisms have been leveled at Poole and Rosenthal's work. First, Koford (1989, 1991) argued that their measures of fit overemphasize the role of the first dimension relative to later dimensions, although Poole and Rosenthal have disagreed (1991). Second, a formal model of legislatures proposed by Snyder (1992, with a reply by Rosenthal) implies that "If some committees are 'preference outliers' relative to the legislature as a whole...then roll call data are likely to be 'artificially unidimensional.'" The lessons of both these critiques are simple: we need better models undergirding our statistical techniques, and we need to work harder to develop the statistical properties of those techniques. At the same time, the critiques themselves signal the importance of Poole and Rosenthal's work, which offers one of the most prominent examples of methodological advance in political science in the past twenty years.

7. Model Specification[23]

In textbooks, "model specification" is sometimes taken quite narrowly to refer to the problem of choosing an appropriate list of explanatory variables in a regression model. Sometimes the scope of the topic is broadened to include questions of functional form and assumptions about the distribution of the disturbance term. But even this broader definition makes it easy to gloss over some important practical problems that are fundamentally similar in their causes and consequences to those encompassed in the usual formulation. The problem of choosing an appropriate population, for example, is essentially identical to the problem of choosing an appropriate set of explanatory variables (since two or more distinct populations can be represented in a pooled model by appropriate use of indicator variables and interaction terms). Problems arising in the use of aggregated data can be addressed using much the same framework used to address seemingly unrelated problems of model specification (Theil 1971, chap. 11; Hanushek, Jackson, and Kain 1974; Erbring 1990). Time series (Beck 1985) and simultaneous equation models (Bartels 1985, 1991b) generate fundamentally similar problems.

Specification Uncertainty and the Perils of Data Dredging

Statistical inferences always rest upon the assumptions embodied in a specific statistical model. In an ideal world, the specification of a model would be determined by strong theoretical expectations, together with accumulated knowledge of the measurement processes generating our data. But real political research is more often marked by very considerable uncertainty about which explanatory variables may be relevant, their functional forms, and the sources and nature of stochastic variation and measurement error.[24]

Ironically, as political methodologists have become more sophisticated, fundamental problems of specification uncertainty have become increasingly pressing. As we have already noted, complex simultaneous equation, factor analysis, and covariance structure models have become increasingly commonplace in various areas of political science. In most respects, this increasing complexity is well and good; but one costly side effect of complexity is to multiply the number of difficult, and often arbitrary, specification decisions upon which any empirical result depends. Too often we lack the strong theory necessary to specify clearly how observable indicators are related to latent variables, or which variables in a structural equation are exogenous and which are endogenous. "We are left with a plethora of loosely related, partially contradictory models in a

single substantive area; one analyst's key endogenous variable is assumed by a second to be exogenous and totally ignored by a third, with substantive consequences that are completely unclear to all concerned" (Bartels 1985, 182). The result is that, even after we have estimated our complex models, we remain -- or at least, we should remain -- much less confident in our estimates of causal effects than classic statistical theory would lead us to expect.

Specification uncertainty is especially troubling to the extent that it invites "data dredging," an extensive exploration of alternative specifications in search of one that produces "significant" parameter estimates, a high R-squared value, or any other desired result. Even for relatively simple regression models, the range of possible alternative specifications is sufficiently large that really efficient data dredging requires the help of a computer equipped with a "stepwise regression" or other variable selection procedure; but humans can, and too often do, also engage in data dredging in their own slightly less efficient way. By "capitalizing on chance" -- overfitting the data to produce impressive results that reflect the peculiarities of the sample at hand more than the underlying causal structure of interest -- they knowingly or unknowingly mislead themselves about the strength and certainty of their results.

The perils of data dredging can be elegantly illustrated by the behavior of data analysts in simple controlled experiments. For example, Green (1990) presented the results of an experiment in which first-year graduate students estimated regression parameters from simulated data without knowing the exact form of the model used to generate the data. Three potential regressors were included in the students' data sets, but only two of these actually had non-zero parameters in the "true" model. Even this rather modest level of specification uncertainty was sufficient to produce errors in the estimated parameters far in excess of what would be expected on the basis of sampling variability: the observed root mean squared error of the parameter estimate Green focused upon was more than twice as large as the nominal standard error derived from classic regression theory.

In an interesting pedagogical wrinkle, Green compared the effects of data dredging among two distinct groups of students who were randomly assigned to somewhat different courses of statistical training. He found that "in comparison to students who were warned against atheoretical model specification, those whose training in regression emphasized classical F-tests and goodness-of-fit were more likely to misspecify their models by including endogenous regressors and excluding causally relevant, but statistically insignificant variables" (Green 1990, 7). Because the latter group "were quick to abandon common sense when the opportunity arose" (Green 1990, 9), their observed root mean squared error exceeded the nominal standard error by 155%, while the observed root mean squared error in the former group exceeded the nominal standard error by a somewhat less harrowing 85% (computed from data presented by Green 1990, 8-9).

Sensitivity Analysis, Out-of-Sample Validation, and Cross-Validation

Green's experiment provides some limited support for his hopeful contention (1990, 9) that "data-dredgers are made, not born." Nevertheless, his results and others like them suggest that, even in the best homes, a latent predisposition toward data dredging leads empirical research seriously astray. The best safeguards against the inferential ill effects of data dredging are sensitivity analysis and out-of-sample validation. Unfortunately, neither has been common in political science.

Sensitivity analysis explores the robustness of key parameter estimates under plausible alternative specifications of a statistical model. When several plausible specifications produce similar results, we can have considerably more confidence in the validity of those results than when apparently minor changes in model specification produce essentially different conclusions. No competent researcher would be so oblivious to this fact as to run a single regression and trust the results it produced. Nevertheless -- whether due to the insecurity or lack of imagination of researchers or the space constraints of journals -- a single regression is often reported as if it were the only one run. Sometimes a footnote is included to the effect that "I tried something different and the results came out the same."[25] But it is seldom made really clear how far a model can be pushed before the results do change significantly. For the most part, we have preferred the false security of a single, implicitly unproblematic model specification to the messier but more enlightening truth that would emerge if extensive sensitivity analysis was a routine part of our published empirical research.

Another remarkably underutilized test of the extent to which the fit of a statistical model reflects data dredging or capitalizing on chance is its success in accounting for data outside the sample used to generate the original results. Kramer's (1971) analysis of economic voting in U.S. congressional elections is one of the very few statistical analyses in the political science literature to have been tested against new data. Kramer's model fit aggregate vote results for congressional elections from 1896 to 1964 with a standard error of about three percentage points (Kramer 1971, 140).

Atesoglu and Congleton (1982) applied Kramer's model to eight subsequent elections (1966-1980), producing an out-of-sample root mean squared forecasting error of about four percentage points (omitting the anomalous Watergate election of 1974 -- about six percentage points for the whole period). Atesoglu and Congleton (1982, 873, 875) concluded that "Kramer's equations have relatively good post-sample predictive ability" and discounted the allegation of "data mining" made by at least one of Kramer's critics. Political scientists unused to validating their models with independent data may be surprised that a "relatively good" result would produce out-of-sample forecast errors on the order of 30 to 100% larger than those produced in the original sample. What should be more surprising -- and embarrassing -- is how few important statistical relationships in political science have ever been validated, even to this degree of accuracy, on samples other than those used to generate them in the first place.

Even when independent data are not available to estimate out-of-sample forecast errors, it is still possible to get a more realistic estimate of the goodness of fit of a model by cross-validation (Geisser 1975). The cross-validation criterion is the square root of the sum of squared prediction errors for the available sample, where each prediction error is the difference between an observed value of y and the corresponding predicted value based on a regression omitting that observation. Thus, the cross-validation criterion differs from the usual standard error of the regression, $\hat{\sigma}$, in that the estimated error for each observation is derived from a regression omitting that observation, rather than from the overall regression with parameter estimates chosen, in part, to fit that observation as well as possible.[26]

Of course, cross-validation is not a panacea for all our problems of model specification. In particular, using cross-validation as a criterion for selecting a regression model (Beck and Katz 1992) reintroduces the familiar perils of data dredging in a new, slightly more sophisticated form. Nevertheless, it seems clear that model assessments based upon cross-validation are less prone to unrealistic optimism than those based upon more conventional techniques, especially in small samples. In addition to providing a useful exposition, Beck and Katz (1992) presented a series of political science examples applying the cross-validation criterion to regression, probit, and duration models estimated on small (15 to 314 observations) data sets.

Specification Tests

Restrictive assumptions are necessary elements of any model specification. But how restrictive should they be? Specification tests are intended to check the restrictive assumptions in a given model specification by treating them as statistical hypotheses to be tested. For example, Wald, likelihood ratio, or Lagrange multiplier tests may be used to decide whether a specific restriction "significantly" decreases the goodness of fit of a given model (Judge et al. 1985, chap. 21).

The explanatory variables in a regression model may be correlated with the disturbance term for a variety of reasons: for example, because other relevant explanatory variables are omitted from the regression, because included explanatory variables are measured with error or have the wrong functional form, or because the dependent variable has a simultaneous reciprocal impact on one or more of the explanatory variables. If misspecification of any of these sorts is suspected, it may be possible to specify an alternative model that is sufficiently general to avoid the suspected misspecification, but at some cost in terms of precision. For example, additional explanatory variables might be added to the regression, or an instrumental variables estimator might be used instead of ordinary least squares (if appropriate instrumental variables are available).

Hausman (1978) proposed a general family of specification tests applicable to situations of this kind, where an efficient but potentially inconsistent estimator can be compared with a consistent but less efficient alternative. In some simple cases, Hausman tests are equivalent to more familiar hypothesis tests. For example, a Hausman test comparing a more parsimonious (and thus more efficient) regression model with a consistent but less parsimonious alternative specification is equivalent to an F-test of the joint significance of the additional variables in the alternative model. The real power of Hausman's formulation lies in its generality, which has prompted applications in a variety of areas. For example, White's (1980, 1982) approach to testing for heteroskedasticity produced an estimator for the covariance matrix of regression parameter estimates that is consistent even when the regression disturbances are heteroskedastic. White's heteroskedasticity-consistent standard errors not only provide a benchmark for testing stronger assumptions, previously adopted uncritically, concerning the homoskedasticity of regression disturbances; they are also beginning to see widespread use in their own right in situations where the traditional assumptions are implausible.

Another useful byproduct of the specification testing approach has been to focus attention on the statistical properties of conditional estimation strategies, or "pretest estimators" (Judge and Bock 1978; Judge et al. 1985, chap. 3). Suppose we decide in advance to specify a restrictive model, test its adequacy against a more general alternative using some specification test, and then adopt the parameter estimates from either the

restricted model (if the specification test fails to reject the null hypothesis at some prespecified confidence level) or the more general alternative (if the specification test results in rejection of the null hypothesis). For simple strategies of this sort, it is feasible to examine the statistical properties of the resulting parameter estimates much as we would those of the parameter estimates produced by either of the two alternative specifications themselves. Formally specified conditional estimation strategies are only a rough approximation to the complexity of actual data analysis. But in the absence of any fully developed theory of "ad hoc statistical inference" (Leamer 1978), rigorous analysis of pretest estimators is a useful first step toward accommodating statistical theory to statistical practice.

Finally, some analysts have advocated using specific batteries of specification tests on a routine basis as an integral part of the model-building process. For example, Granato (1992), following Hendry, Qin, and Favero (1989), proposed a specific 12-step "agenda for model building" based upon a battery of diagnostic tests for serial correlation, heteroskedasticity, non-Normality, omitted variables, inappropriate functional forms, and parameter variation. Granato advocated the use of these tests as criteria for successfully "reducing" a general model to a simpler, more parsimonious specification consistent with the data. Although systematizing the diagnostic process in some such way may help to avoid egregious misspecification, it is unclear what criteria should be used to balance the competing demands of parsimony and accuracy in the process of model reduction. "Reduction *does* involve a change in the original parameters," Granato wrote (1992, 134-35, emphasis in original), "but for valid reduction, the parameters *must be* constant and maintain the same sign as in the original model. Or, to put it another way, model reduction must not result in a loss of information."

The apparent contradiction between change and constancy is partially resolved by an explanatory footnote: "In other words, the parameter of the 'reduced' model is roughly the same as the 'general' model" (Granato 1992, 135). Nevertheless, the myriad complications bound up in the phrase "roughly the same" seem sufficiently daunting to hamstring this or any other mechanical formula for model specification. How rough is "rough"? Which restrictions should we test first, how stringently, and why? In the absence of some foolproof formula for managing the inevitable, various, and complex tradeoffs between unbiasedness and precision in real data analysis, model specification must continue to be guided more by statistical experience and substantive insight than by any all-purpose recipe.

Analyzing the Consequences of Misspecification

Specification tests can be very useful for establishing that a model is misspecified, but they are much less useful for understanding or managing the practical consequences of misspecification. A rather different approach to the uncertainties of model specification is reflected in the work of Bartels (1985, 1991b). "Often," as Bartels (1985, 181) put it:

> our existing stock of theorems allows us to specify with impressive rigor what the world would have to look like in order for our standard procedures to produce the right results. But theory of the usual sort too often provides little in the way of useful guidance to researchers who want to proceed, even though their data and models fail to satisfy the assumptions on which the impressive theorems are based. We need a different sort of theory capable of illuminating the practical consequences of the simplifications and approximations involved in everyday empirical research.

Bartels (1985) compared the consequences of three distinct sorts of misspecification in simultaneous equation models: omitting endogenous regressors, treating them as exogenous, and treating them as endogenous by introducing false restrictions on the effects of other (exogenous) variables. In addition to characterizing the inconsistencies in the parameter estimates resulting from each of these three sorts of misspecification, Bartels (1985, 190) derived a "rule of thumb" for choosing between the first and second strategies (in order to minimize the resulting inconsistency due to misspecification) based upon prior beliefs about the likelihood that a specific true parameter exceeds a specified critical value.

In much the same spirit, Bartels (1991b) analyzed the practical consequences of using "quasi-instrumental" variables that only approximately satisfy the textbook assumption of exogeneity. Bartels showed (1991b, 781, 786) that the true asymptotic mean squared error of an instrumental variables parameter estimate will be understated in this case by a factor of $(1 + N \rho_{zu|X}^2)$, where N is the sample size and $\rho_{zu|X}^2$ is the squared population partial correlation between the instrumental variable **z** and the disturbance **u**, holding constant any other (exogenous) explanatory variables in the regression. This result suggests that the inferential consequences of even fairly minor misspecification may be quite serious. For example, the true (asymptotic root mean squared) error of the instrumental variables parameter estimate will be more than twice as large as the nominal standard error

if the sample size is 50 and the partial correlation between **z** and **u** is greater than .25, or if the sample size is 300 and the partial correlation between **z** and **u** is greater than .10.

In addition, Bartels derived a decision rule for preferring an instrumental or "quasi-instrumental" variables estimator to ordinary least squares by an asymptotic mean squared error criterion. He showed that a "quasi-instrumental" variables estimator will have a smaller asymptotic mean squared error than the ordinary least squares estimator if and only if

$$\rho_{xz|X}^2 > (\rho_{zu}^2 + 1/N)/(\rho_{xu}^2 + 1/N),$$

where ρ_{xu}^2 and ρ_{zu}^2 measure the endogeneity of the original regressor **x** and the instrumental variable **z**, respectively, and $\rho_{xz|X}^2$ is the squared population partial correlation between **x** and **z**, holding constant any other (exogenous) explanatory variables **X** in the regression. The importance of the squared population partial correlation $\rho_{xz|X}^2$ in this expression led Bartels (1991b, 798-800) to suggest that the corresponding squared sample partial correlation $R_{xz|X}^2$ should be reported as a matter of course in instrumental variables applications, since the more conventional practice of reporting the R^2 statistic from a purging regression of **x** on both **z** and **X** may greatly exaggerate the apparent efficiency of the instrumental variables estimator (since only that portion of **z** uncorrelated with **X** actually contributes to the efficiency of the instrumental variables parameter estimates).

8. Estimation

The increasing methodological sophistication of political research owes much to the explosion of computing technology. Researchers who once spent late nights at their university computer centers running batch jobs using SAS or SPSS can now perform much more complex analyses much more conveniently on increasingly powerful and inexpensive personal computers using increasingly powerful and specialized statistical software. Since software is being developed and disseminated at a rapid rate, any detailed discussion of specific packages would quickly be obsolete, so none will be attempted here. *The Political Methodologist*, the newsletter of the Methodology Section of the American Political Science Association, is a good source of current information.

One implication of these technological developments is that political methodologists have been able to adopt and develop increasingly complex models without sacrificing the ability to estimate the parameters of those models using real data. Two developments have been especially important. First, the increasing power of iterative algorithms to search large parameter spaces has facilitated the application of iterative estimation techniques, and thus of maximum likelihood estimation. Second, the ability of modern computers to engage in intensive resampling has facilitated the development of a variety of estimators based upon resampling, most notably bootstrap estimators.

Maximum Likelihood Estimation

A decade ago the vast majority of estimators used in political science research were based upon the least squares criterion, and maximum likelihood estimation was an exotic solution to special problems in which the least squares principle was inapplicable, including probit and logit models with individual-level data and factor analytic and covariance structure models (as, for example, in the very influential textbook treatment by Hanushek and Jackson 1977).[27]

Today the situation is quite different. Political methodology has its own survey of maximum likelihood estimation (King 1989c) comparable to those available in biometrics (Edwards 1972) and econometrics (Cramer 1986). And, thanks in part to King's advocacy, political scientists have applied maximum likelihood estimators to a growing variety of models, including measurement models, event count models, and generalizations of logit and probit models (Nagler 1992).

The extent to which applied research in political science has begun to utilize maximum likelihood estimation is heartening. Nevertheless, some persistent conceptual confusions have not been entirely dispelled. In particular, researchers and readers alike sometimes seem to forget that maximum likelihood is, properly speaking, an estimation criterion rather than either a model or an algorithm.

The familiar phrase "maximum likelihood model" is a misnomer, since model specification and estimation are logically distinct enterprises. It is certainly true, as King (1989c) has argued, that the maximum likelihood framework may facilitate the specification and estimation of more interesting and flexible models; but the models themselves must stand or fall on their substantive plausibility and utility. Whenever the "maximum likelihood" brand name serves to forestall rather than promote attention to real issues of model specification, the result is detrimental.

On the other hand, the term "maximum likelihood estimation" is sometimes misleadingly applied to any iterative estimation scheme. Although maximum likelihood estimation is almost invariably implemented using some iterative search algorithm, it certainly does

not follow that any parameter estimates produced by any iterative search algorithm are, ipso facto, maximum likelihood estimates. Maximum likelihood estimates, with their attractive statistical properties, only result if the function being maximized is, in fact, a correctly specified likelihood function satisfying some non-trivial technical conditions.

Among other things, the full specification of a likelihood function requires specific distributional assumptions. However, it is worth noting (McCullagh and Nelder 1989) that "quasi-likelihood" estimators are also available for a broad class of "generalized linear models" (including linear regression, logit and probit, log-linear, count, and survival models). These estimators require less strong distributional assumptions, but they can still be shown to be optimal within the rather broad class of linear estimating functions.

Bootstrapping

Although resampling techniques of various sorts have a considerable history in statistics, developments in the field accelerated rapidly when Efron (1979) proposed the bootstrap technique. Bootstrapping "has been the subject of furious theoretical research and heated debate in the statistical community" (Mooney and Duval 1991, 1). Mooney and Duval's own exposition includes references to the relevant statistical literature, as well as a clear introduction and examples aimed at political scientists. The "leisurely" exposition by Efron and Gong (1983) is also especially useful in that it places bootstrapping within a broader array of resampling techniques.

The basic idea of the bootstrap is to estimate parameters repeatedly from independent samples drawn from the original sample of interest. For example, in a regression analysis based upon 100 observations, many samples of 100 observations each would be drawn (with replacement) from the original 100 observations, and the regression parameters would be estimated in each of those samples. Depending upon the complexity of the problem and the availability of computing power, for example, each regression parameter might be estimated independently in 1,000 different samples, each drawn randomly from the original sample of 100 observations. The empirical distribution of the 1,000 different estimates of each parameter would then serve as an estimate of the sampling distribution of that parameter.

Bootstrapping has two primary applications. First, since estimates of parameter variability often rely on different and more stringent assumptions than estimates of the corresponding parameters themselves, the possibility of dispensing with those more stringent assumptions and estimating the sampling distribution directly is an attractive one. For example, in a regression model where the usual assumption of Normally distributed errors seemed implausible, bootstrapping could be used to estimate the sampling distribution of parameter estimates without recourse to that assumption.

Bootstrapping techniques are also useful for estimating parameters for which no simple analytic formulae exist. An especially important example for political scientists, as Mooney and Duval (1991) noted, is the difference between two sample medians. Formal theories of collective choice often predict an outcome reflecting the preference of the median voter in the relevant collectivity. Thus, for example, a congressional committee and its parent body would be expected to produce different policy outcomes if their respective median voters had different preferences, but not otherwise. The existence of "significant" differences between the positions of median legislators in committees and on the House floor has been a matter of some controversy in the literature on congressional politics, but it has usually been investigated without formal statistical testing or with difference-of-means tests instead of difference-of-medians tests as a matter of statistical convenience (Krehbiel 1990; Hall and Grofman 1990). Mooney and Duval (1991) pointed out that bootstrapping provides a straightforward technology for estimating directly the sampling distribution of the difference between the relevant medians in this problem and others.

9. Political Methodology and Political Science

Much of the scientific progress in any field occurs well behind the intellectual frontier, where yesterday's breakthroughs are consolidated and disseminated to become tomorrow's standard practice. Thus, the improvement in the state of political methodology during the past decade must be gauged not only from the best theoretical work in *Political Analysis*, but also from the best and less-than-best empirical work in the discipline's mainstream journals and books. By this standard, too, the state of quantitative political methodology looks better now than it did a decade ago.

The limitations of some once-popular techniques, such as stepwise regression and standardized regression coefficients, are better understood by workaday researchers, thanks to the pedagogical efforts of Achen (1982), King (1986), and others. Bivariate analyses have to a considerable extent been supplanted by more realistic multivariate specifications. And the careless practice of reporting regression parameter estimates without standard errors or t-statistics is fading, though it is by no means eliminated.

All these changes reflect a significant increase in the average level of methodological sophistication of empirical research in the field. But with what payoff? New and better methods and standards are of no intrinsic interest to political scientists who are not themselves methodologists; the discipline as a whole cares only -- and rightly so -- about the light political methodology can help to shed upon substantive questions about political phenomena. Thus, it is incumbent upon political methodologists to demonstrate that their techniques have made real contributions to the study of politics.

We believe that there are real contributions to point to in a variety of areas. Statistical analysis appears with increasing prominence and sophistication in the discipline's prize-winning books (Bartels 1988; Lewis-Beck 1988; Carmines and Stimson 1989; Freeman 1989), if not in its leading journal.[28] Empirical analyses explicitly derived from or shedding light upon rational choice or other formal theories of political behavior, though still rare, appear with increasing frequency (Poole and Rosenthal 1985; Enelow and Hinich 1985; Brady and Sniderman 1985; Bartels 1986; Krehbiel and Rivers 1988; Brady and Ansolabehere 1989; Krehbiel 1991). In the field of international relations, new methods have shed new light on the dynamics of great power rivalry (McGinnis and Williams 1989; Goldstein and Freeman 1991). In American politics, the conventional wisdom concerning presidential influence in Congress has been recast through more careful attention to the endogeneity of presidential requests (Rivers and Rose 1985). And in comparative politics, the literatures on cabinet durability in parliamentary democracies (King et al. 1990) and on leadership succession and regime stability (Londregan and Poole 1990, 1992) have been put on firmer statistical footing.

These advances and others like them are -- or should be -- familiar to scholars in the various subfields of political science. In this section, rather than simply multiplying examples, we describe in some detail two cases in which methodological advances have had clear and important substantive payoffs. For purposes of illustration we choose precisely the two problems Achen chose a decade ago to represent the untapped potential of political methodology: the nature of the survey response and the phenomenon of economic voting. The fact that political methodologists have made significant progress on both these problems in the intervening decade seems to us to suggest that further investment in basic methodological research will continue to pay handsome dividends in terms of our substantive understanding of politics.

The Nature of the Survey Response

Ever since the classic work of Converse (1964, 1970) demonstrated the remarkable instability of individual political opinions as measured over relatively short periods of time, analysts of public opinion have struggled to figure out "what survey responses are measuring" (Achen 1983a, 80). Achen's own earlier (1975) contribution and many others (Erikson 1979; Converse and Markus 1979; Jackson 1983; Judd, Krosnick and Milburn 1981; Kinder 1983; Norpoth and Lodge 1985) addressed, but did not resolve, the issue of whether response instability reflected the absence of stable attitudes or simply the vagaries of opinion measurement.

As Achen pointed out (1983a, 76-81), the persistence of disagreement in this debate may largely reflect the limitations of the mathematical models used to study response stability.[29] In particular, Achen pointed out that very different mathematical models -- including one allowing for random measurement error and implying almost complete stability in underlying political attitudes (Wiley and Wiley 1970) and another allowing for autocorrelated measurement error and implying almost complete *in*stability (Wiley and Wiley 1974) -- could account equally well for the available data. This result was discouraging because each model was just identified, and a model incorporating both would require more information than was available in a three-wave panel.[30]

On the basis of that observation, Achen (1983a, 80-81) suggested that "a great deal could be learned by extending our current techniques to lengthier panel studies....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 course of subsequent research in the field has followed precisely Achen's prescription. Feldman (1990) and Palmquist and Green (1992) both used data from Thomas Patterson's five-wave panel study to estimate a more general model of response instability in which random and correlated error could be distinguished. Both found that the random error model "fit the observed data...very well" (Feldman 1990, 51), especially when the measurement error variances were allowed to vary across panel waves. Feldman's stability estimates for issue positions and party identification ranged from .85 to 1.00, and his reliability estimates ranged from .49 (for a tax item) to .90 (for party identification); candidate evaluations had similar reliabilities (from .63 to .94) but lower stabilities (as low as .37 and no higher than .89). Feldman (1990, 40) concluded that "there is nothing inherent in the Wiley and Wiley measurement model that automatically produces perfectly stable estimated true scores."

Feldman went on to examine whether individual level response instability is related to political knowledge or other characteristics. He found that "knowledgeable people do report more stable attitudes as the nonattitudes model would predict," but that "this relationship is *not* large enough to account for all or most of observed response instability" (1990, 51-52). He concluded that "neither of the major interpretations of response instability is sufficient to account for these findings." Feldman went on to propose a more complicated model in which observed instability results from a combination of pure measurement error and the variability produced by respondents reconstructing their specific opinions in each interview from essentially stable sets of information, beliefs, and values "rather than carrying around preformed and rehearsed positions on a host of issues, people can draw on their information, beliefs, and values to respond to policy questions as they arise" (1990, 52-53).

Drawing upon Feldman's results, a considerable accumulation of research on specific question wording and question placement effects, and psychological theory, subsequent analysis by Tourangeau and Rasinski (1988), Zaller and Feldman (1992), and Zaller (1992) has gone a long way toward developing and testing more realistic mathematical models of the survey response. In these models, respondents are assumed to construct opinions by sampling considerations from a store of relevant possibilities. Question wording and question placement are interpreted as influencing the probability of retrieving some considerations rather than others. And response instability represents neither an indictment of ordinary people's political sophistication nor a mere nuisance caused by imperfections of measurement, but a window upon the diversity and complexity of political considerations that are the basic building blocks of public opinion. The result is a methodological advance, but it is also a theoretical advance of profound importance to anyone who studies the nature or impact of public opinion.

Economic Voting

The literature on economic voting provides one of the clearest instances in which political methodology has contributed to significant empirical and theoretical advances in a research area of widespread substantive interest. A vigorous controversy regarding the relative importance of "sociotropic" and individual "pocketbook" effects of economic conditions on the vote (Kramer 1971; Kinder and Kiewiet 1979; and many others) has been largely resolved through a combination of sophisticated statistical modeling (Kramer 1983) and careful empirical work (Markus 1988).

The essential contribution of Kramer (1983) was to specify clearly the limitations of both time-series and cross-sectional data in the context of economic voting models. Individual-level cross-sectional analyses had typically found modest and inconsistent effects, Kramer argued, because "politically relevant" changes in individuals' personal income were swamped in the available survey data by politically irrelevant changes deriving from idiosyncratic individual circumstances (promotions, retirements, and the like). On the other hand, aggregate time-series analyses had found consistent and significant effects because they were much less subject to this sort of measurement error (at least given Kramer's assumption that politically irrelevant changes in economic circumstances cancel out across the population). But Kramer also pointed out that estimates based on aggregate time-series data necessarily confounded the effects of individuals' own economic circumstances and the effects of national economic conditions, making it impossible to disentangle "pocketbook" and "sociotropic" voting with the data at hand.

Markus overcame this logjam by pooling survey data from a series of presidential elections (1956-1984) in which aggregate economic conditions varied significantly.[31] The resulting variation in both aggregate economic conditions (across election years) and personal economic circumstances (both across and within election years) made it possible to distinguish the effects of national conditions and personal economic circumstances on vote choices. Markus's results demonstrated that both national and personal economic conditions significantly influenced presidential votes. According to Markus's (1988, 146) estimates, each 1% increase in aggregate real disposable personal income per capita increased the vote share for the presidential candidate of the incumbent party by 2.3%, of which 1.9% was a direct effect associated with national income changes and the remainder was an indirect effect produced by changes in personal circumstances. As for the relative weight of national and personal circumstances, Markus (1988, 148) estimated that an improvement in personal economic circumstances (being "better off than a year ago" rather than "about the same") increased a voter's probability of supporting the incumbent party's presidential candidate by about the same amount as a 3.6% increase in aggregate real disposable personal income per capita -- roughly, the difference in aggregate economic conditions between a boom year (1956, 1964, or 1972) and a year with no aggregate income growth (1960).

As with the work on the nature of the survey response, this brief history of research on economic voting illustrates clearly how careful methodological work can have significant substantive payoffs. The scholarly literature on economic voting has taken a permanent step

forward, overcoming in the process conceptual and practical problems that seemed daunting only a decade ago. In view of these examples, and others like them, it hardly seems unduly optimistic to hope for similar gains in other substantive areas of political science in the decade to come.

Conclusion

Quantitative political methodology in the past decade has been a strong growth stock. We see no signs of a downturn in the intellectual vitality of the field or in the quantity or quality of research. On the contrary, there is every indication that further investment will pay even greater intellectual dividends. But what sort of investment is required at this stage? We see the same three glaring needs that Achen identified a decade ago. In each case there has been notable progress since he wrote, but in each case the underlying problem remains far from solved.

First, as Achen (1983a, 87) argued a decade ago, political methodologists and political theorists alike need to develop "formal theories with measurement models built into them." Where such theories have been developed, as in the fields of dimensional analysis, survey response, event counts, polytomous choice, and some areas of time-series analysis, significant methodological progress has followed. In the field as a whole, however, there is still far too much data analysis without formal theory -- and far too much formal theory without data analysis.

Second, "to provide for the future, much better mathematical training will have to be provided at both the graduate and undergraduate level" (Achen 1983a, 89). Our traditional patterns of undergraduate training demand reform; as the research literature throughout the discipline becomes more sophisticated methodologically (and theoretically), it will be increasingly difficult to pretend that an undergraduate curriculum with little or no formal analytical training can provide more than a superficial caricature of the field. The problem is even clearer at the graduate level, with the added difficulty that we must also devote sufficient intellectual resources to train new and more sophisticated generations of methodological specialists. As long as otherwise reputable political science departments rely upon economists, psychologists, and sociologists to supply their graduate coursework in methodology, it will continue to be the case that "the pages of our journals frequently have the look of living rooms decorated at garage sales" (Achen 1983a, 70).

Third, as Achen (1983a, 89) also argued a decade ago, "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." In this respect there has been greater progress in the past decade. Young methodological specialists who were graduate students or assistant professors when Achen wrote now fill senior positions in leading political science departments; their students are entering the field's junior ranks in increasing numbers; and their teachers continue to publish not only respectably but with great distinction.

Progress on these fronts has been, and will undoubtedly continue to be, stimulated by the institutional development of political methodology as a subfield. The Methodology Section of the American Political Science Association, a goal when Achen wrote, is now a functioning organization with hundreds of dues-paying members. Its annual publication, *Political Analysis* (Stimson 1990, 1991b, 1992; Freeman n.d.), publishes a significant fraction of the best research in the field. And its newsletter, *The Political Methodologist*, is an increasingly ambitious forum for news, reviews, and intellectual debate. Summer methodology conferences at Michigan (1984), Berkeley (1985), Harvard (1986), Duke (1987), UCLA (1988), Minnesota (1989), Washington University (1990), Duke (1991), and Harvard (1992) have provided an important new forum for presenting and discussing methodological research. Many of the articles described in this review were first presented at these conferences. With funding from the host universities and the National Science Foundation, recent conferences have included dozens of graduate students in addition to faculty members from across the United States.

Nevertheless, any survey of the field must recognize that the number of political scientists working and thinking primarily as methodologists is still exceedingly small and that, in the long run, the number and energy of the field's part-time adherents cannot adequately compensate for a dire shortage of trained, committed specialists. As long as most political science departments have more Latin Americanists, urbanists, and voting behavior specialists than methodologists it cannot be surprising that we know more about the vicissitudes of politics in Nicaragua, New Haven, and the New Deal coalition than about the central methodological problems of our discipline. And as long as colleagues in other fields count anyone who does applied statistical work as a political methodologist, and vice versa, we cannot be surprised that our principal methodological agenda remains unconquered.

Notes

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1. We chose these journals because they covered all the major subfields of empirical political science and because our colleagues suggested that they would be the best places to find innovative quantitative work.

2. These data sets cover somewhat different time periods (1966-1986 and 1948-1978, respectively) and use different coding procedures for actions (WEIS codes each event into one of 22 different categories and COPDAB uses a 15-point ordinal cooperation-conflict scale), but they are similar in that each identifies an originating nation, a target nation, and a date of the action.

3. The first volume of these data includes over one hundred "Cross-National Attributes and Rates of Change" circa 1975 for the major nations of the world, and repeated values, often back to 1950, at five-year intervals for over half the variables. The second volume includes "a record of political protest and violence, state coercive behavior, and formal governmental change and elections in the major countries of the world for the years 1948-1977" (1983b, xiii). Over 87,000 events are recorded in both daily and annual form, and attributes are coded for over 30,000 of these events.

4. The event history, the most complete description of the data, tells us the timing of all moves into and out of each possible status; the events sequence only records the sequence of states occupied by each sample member without the timings; event counts record only the number of relevant events in an interval.

5. When these methods are not employed, a Durbin-Watson statistic is usually reported to justify the use of ordinary least squares. Using the Durbin-Watson statistic makes sense because it is a relatively good test for both autoregressive and moving average errors (Judge et al. 1985, 322-32), but probably too many authors assume that a significant Durbin-Watson implies a first-order autoregressive process when there are, as Hibbs (1974) noted, many other possibilities.

6. Granger and Newbold (1986) offer a useful technical introduction to time-series methods in econometrics, albeit with a heavier emphasis on forecasting than has been common in the political science literature.

7. Of course, this rule presupposes that we have already decided which variables belong in the system on theoretical grounds. In addition, some discretion remains regarding appropriate lag lengths; here it is customary to rely upon one or another goodness-of-fit test rather than upon any theoretical presuppositions about the relevant dynamics (Freeman, Williams, and Lin 1989, 845).

8. Tests of this sort were originally motivated by the concept of Granger causality (Granger 1969; Freeman 1983). A variable X is said to Granger cause another variable Y if current values of Y can be predicted more accurately from the histories of both X and Y than from the history of Y alone. Thus, rejecting the null hypothesis that all of the parameters associated with lagged values of X in a vector autoregression are zero constitutes evidence that X Granger causes Y.

9. This framework can be further generalized -- for example, to include longer lags of endogenous variables -- by appropriately supplementing the state vector γ_t (Beck 1990, 130-31).

10. Time-series cross-sectional studies have considered defense burden sharing (Palmer 1990; Oneal 1990), determinants of military expenditures (Rosh 1988) and democratization (Gonick and Rosh 1988), diffusion of social policy innovations (Schneider and Ingraham 1984), budgeting in the U.S. (McCubbins and Schwartz 1988; Kiewiet and McCubbins 1988; Kamlet and Mowery 1987), community expenditures (Schneider 1989), electoral results by state (Holbrook 1991; Chubb 1988), union political action (Delaney, Fiorito, and Masters 1988), and enforcement of environmental regulations by counties (Scholz, Twombly, and Headrick 1991).

11. Indeed, there may be a need for I separate variances due to heteroskedasticity across units, an additional I or more parameters for autocorrelation, and one or more parameters to correct for spatial autocorrelation across units.

12. Wilkerson (1991) criticized this approach on two grounds. First, he doubted whether legislators always vote sincerely, as they are assumed to do in Krehbiel and Rivers's model. Second, he doubted whether a point, particularly in one dimension, can completely characterize legislators' assessments of a bill or amendment.

13. The Poisson distribution has been applied to a wide variety of political phenomena, including Supreme Court appointments (Wallis 1936; Ulmer 1982), wars (Richardson 1944; Houweling and Kune 1984), international alliances (McGowan and Rood 1975; Job 1976; Siverson and Duncan 1976), democratic transitions (Starr 1991), the circulation of elites (Casstevens 1989), and the duration of governments (Browne, Frendreis, and Gleiber 1986).

14. In addition, the Poisson has the peculiar property that its mean equals its variance, which seems odd in many applications.

15. The focus on the error term may seem odd to those versed in multivariate statistical procedures, but there were two compelling reasons for it. First, many interesting data series -- the number of wars, alliances, coups, or urban riots -- were collected without much attention to possible explanatory variables. Second, adding covariates to the Poisson model produces likelihood equations that must be solved iteratively, a daunting task before the advent of modern computers and packaged software programs. Although these limitations have handicapped research in this area, they have also meant that the literature on event counts is significantly more self-conscious than other literatures in political science about the underlying stochastic processes generating observed data.

16. Applications include multiple indicator structural or path models of citizen tolerance (Shamir and Sullivan 1983), welfare guarantees in the American states (Plotnick and Winters 1985), individual preferences for tax policy (Hawthorne and Jackson 1987), black belief systems (Allen, Dawson, and Brown 1989), the relationship between political participation and efficacy (Finkel 1985), state policy liberalism (Wright, Erikson, and McIver 1985) and spending by House challengers (McAdams and Johannes 1987), confirmatory factor analysis of emotional responses to candidates (Marcus 1988) and to the economy (Conover and Feldman 1986) and of the structure of core beliefs (Feldman 1988), and many others.

17. This result is reminiscent of the classic Converse "black-white" model in which some people have attitudes and others do not; but it has the virtue of "nesting" the Converse model and the Wiley-Wiley model within a larger model. Moreover, it can be shown that within the larger model, high stabilities and high reliabilities (low error variances) can coexist, and guessers can have an error variance that is no larger than that of non-guessers. These surprising results follow because the respondents who are guessing do have a "stable" point of reference -- the center of the scale -- which the Wiley-Wiley method treats as a stable true opinion.

18. One stimulus for these developments was a clever and very readable paper by Weisberg (1974a) entitled "Dimensionland: An Excursion in Spaces." Weisberg posed fundamental questions about the utility of scaling methods for political science data, explored the meaning of unidimensionality and the relationship between factor analysis and multidimensional scaling (MDS), and suggested that political scientists had to develop a distinctive set of scaling methods suitable to politics.

19. Although they did not use scaling procedures directly, Rabinowitz and Macdonald (1989) and MacDonald, Listhaug, and Rabinowitz (1991) developed a very interesting alternative to spatial modeling called a "directional theory of issue voting." Building upon Rabinowitz's (1978) finding that most candidates did not seem to be near the median voter, these papers argued that voters choose candidates who are on the same side of the issue as they are. This means that a candidate who is close to a voter based upon a distance measure might be shunned if the candidate is on the wrong side of an

issue that is important to the voter. A moderate pro-choice voter would prefer an extreme pro-choice candidate to a moderate pro-life candidate.

20. Brady (1991b) showed that a Poole-like estimator actually does produce statistically consistent estimates of both factor scores and factor loadings in a linear factor model. Rivers's (1987) result, then, is partly the result of the non-linearities inherent in ideal point models. It can also be shown that the Poole estimator would yield correct results if there were no error in the model. These results suggest that the Poole estimator may not produce much bias if the amount of error is small.

21. Jackson and Kingdon (1992) have criticized the use of interest group scores to explain congressional votes, but their argument does not bear on the debate about the number of dimensions in legislative votes.

22. Indeed, Poole and Rosenthal recognized that the standard proof of consistency for maximum likelihood does not apply, but they made the odd statement (1991, 272) that "At a practical level, this caveat is not important. The key point is that data is being added at a far faster rate than parameters." Yet there are many examples in statistics where adding data faster than parameters does not ensure consistency. The Monte Carlo reports in their paper are more convincing; but much more work still needs to be done to assay the statistical properties of their innovative method for scaling roll-call votes.

23. This section draws heavily upon Bartels (1990), where some related points are discussed in more detail.

24. Fascinating questions about the role of theoretical expectations in statistical inference were raised by a Box-Jenkins ARIMA analysis in the *Journal of Conflict Resolution* (Orme-Johnson et al. 1988), demonstrating that "a very small group practicing [the Maharishi technology of the unified field] in East Jerusalem appeared to influence overall quality of life in Jerusalem, Israel, and even in neighboring Lebanon." The editor of the journal (Russett 1988, 773) did not know what to make of the finding and admitted that "The hypothesis has no place within the normal paradigm of conflict and peace research. Yet the hypothesis seems logically derived from the initial premises, and its empirical testing seems competently executed." Schrodt (1990) questioned whether the research was, in fact, competently executed; but whatever the truth in this case, the original article raises significant questions about what scientific hypotheses we should be willing to entertain and what proof we should require for their demonstration. How strongly should theory incline us to believe or disbelieve that there are "long cycles" in the severity of war in the international system (Goldstein 1988, 1991; Beck 1991), or that postwar U.S. savings rates were influenced by changes in public perceptions of the threat of nuclear war (Slemrod 1986)? As we add new and more powerful techniques to our kitbag of tools, it is worth remembering that good inferences require more than powerful techniques and large t-statistics.

25. It is striking how seldom, in footnotes of this sort, trying something different makes the results come out different.

26. The cross-validation criterion is simply the square root of the PRESS (PRediction Error Sum of Squares) criterion of Allen (1971) and is also closely related to the jackknife and bootstrap techniques (Efron and Gong 1983).

27. Of course, the least squares and maximum likelihood criteria sometimes lead to the same estimator, as in the case of ordinary regression with normally distributed errors. In cases like this, researchers did maximum likelihood estimation in the same way they wrote prose, without knowing it.

28. King (1991, 2-3) reported the results of a content analysis in which the proportion of *American Political Science Review* articles using "quantitative data and methods in some way" has fluctuated around 50% since 1969.

29. Another approach to the problem of mass belief systems has been to reconceptualize political sophistication and ideological constraint. Luskin (1987) provided a magisterial overview of various measures of political sophistication and some of his own suggestions. Peffley and Hurwitz (1985) and Hurwitz and Peffley (1987) suggested an interesting approach based upon a hierarchical structure of attitudes with core beliefs informing broad postures which, in turn, are the basis for specific beliefs.

30. Later, Palmquist and Green (1992, 128) showed that the identification problem is even worse than in Achen's telling because "standard errors for the measurement error parameters [of the Wiley and Wiley (1974) model with correlated errors] are typically so large that it is not hard to see how implausible values could result simply from sampling error." With three waves of data it is not only impossible to distinguish between the two models but also virtually impossible to get informative estimates of the parameter estimates of the correlated error model.

31. Rivers (1986) independently proposed a similar solution to the Kramer problem, albeit with a somewhat different model and estimation strategy.

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