DIVISION OF THE HUMANITIES AND SOCIAL SCIENCES
CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA, CALIFORNIA 91125

THE SPATIAL ANALYSIS OF ELECTIONS AND COMMITTEES: FOUR DECADES OF RESEARCH

Peter C. Ordeshook

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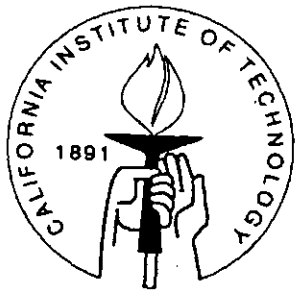

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The Spatial Analysis of Elections and Committees:
Four Decades of Research

Peter C. Ordeshook
California Institute of Technology

It has been more than thirty five years since the publication of Downs's (1957) seminal volume on elections and spatial theory and more than forty since Black and Newing (1951) offered their analysis of majority rule and committees. Thus, in response to the question "What have we accomplished since then?" it is not unreasonable to suppose that the appropriate answer would be "a great deal." Unfortunately, reality admits of only a more ambiguous response.

It is true that developments in the spatial analysis of committees and elections has covered considerable ground since 1957. Beginning with Davis and Hinich's (1966) introduction of the mathematics of Euclidean preferences, Plott's (1967) treatment of contract curves and symmetry, and Kramer's (1972) adaptation of Farquharson's (1969) analysis of strategic voting in committees with spatial preferences, many of Downs's and Black and Newing's ideas have been made rigorous and general. The idea of spatial preferences -- of representing the set of feasible alternative as a subset of an m-dimensional Euclidean space, of labeling the dimensions "issues," of assuming that people (legislators or voters) have an ideal preference on each issue, and of supposing that each person's preference (utility) decreases as we move away from his or her wi-dimensional ideal policy — is now commonplace and broadly accepted as a legitimate basis for modeling electorates and parliaments. Moreover, since Weisbergh and Rusk's (1970) initial application of multi-dimensional scaling, considerable advances have been made in developing statistical methodologies for measuring those preferences within electorates (see, for example, Enelow and Hinich 1982, Aldrich and McKelvey 1977, Poole and Rosenthal 1984, Chu, Hinich and Lin 1993) and legislatures (Hoadley 1987, Poole and Rosenthal 1985 1991).

At the same time, spatial analysis has moved only modestly beyond modeling the simplest possibilities -- two candidate plurality rule elections, exogenously imposed amendment voting agendas, and the formation of majority parliamentary coalitions. Although there are any number of specialized models and empirical case studies, there remains preciously little generalized theory about alternative institutional structures or experience with applying elements of spatial theory to more complex political systems to which we can refer when contemplating the design of political institutions for, say, newly emerging democracies. What, for example, can we say about party-list proportional representation systems employing two or more multi-member districts or systems with multiple candidates and runoff provisions that matches the theoretical generality and simplicity of

the Median Voter Theorem? What do we know about, say, those political systems, including our own, in which policy cannot be imposed by a single victorious candidate but must instead be approved by multiple branches of government, each of which answers to voters under different rules? Where are the definitive theorems about committee processes that allow for the endogenous determination of such things as voting agendas and sub-committee jurisdictions? Indeed, do we even know whether such results are possible or desirable?

Although the literature is beginning to address such questions, the spatial analysis of both committees and elections suffers from a number of deficiencies that are sufficiently serious so as to make it uncomfortable defending that analysis against the charge of having failed to produce the volume of ideas that we could reasonably anticipate in thirty or forty years (for a sense of the rate of progress, the reader can consult my 1976 survey of the literature as it existed in the early 70's and Mueller's 1989 more general survey of Public Choice). Some of these deficiencies, especially those that pertain to the adequacy of a spatial representation of preferences and the limited institutional structures considered, apply to research on both committees and elections. Other deficiencies are unique to one area or the other. On the other hand, I also want to argue that spatial analysis has altered fundamentally the way we think about voting, elections, and parliaments, and that both implicitly and explicitly it contributes much to those who study democratic politics.

1. Fundamental Theoretical Achievements

SOCIAL CHOICE: It is tempting to focus on formal definitions and fundamental theorems when surveying a mathematically deductive field with as extensive a literature as spatial analysis (for a comprehensive bibliography see Coughlin 1992). Surprisingly, though, there are few results that warrant the label "fundamental." There is, of course.

> Black's (1958) theorem about single-peakedness and the existence of a Condorcet winner (roughly, if voters have well-defined preferences that can be scaled along a single issue dimension, then the electorate's median preference is a Condorcet winner -- an outcome that defeats every other one in a majority vote),
>
> the Davis-Hinich-Plott-Sloss (1966, 1967, 1973) generalizations to multi-dimensional issue spaces (roughly, a Condorcet winner exists if and only if a multidimensional median exists, which requires a very special class of preference distributions; for subsequent refinements see Davis, De Groot and Hinich 1972, McKelvey and Schofield 1986).
>
> the McKelvey (1976, 1979) and Schofield (1978) "chaos" results about intransitive social preferences (roughly, if there is no Condorcet winner, then the social preference order under majority rule is wholly intransitive when preferences are

spatial -- every outcome can be reached from every other outcome by some paired sequence of votes over feasible outcomes).

Caplan and Nalebuff's (1991) proof that something less that 2/3rds rule (64% to be precise) can ensure a two-candidate spatial equilibrium for a broad class of electoral preference distributions (see also Greenberg 1979 and Schofield 1985, as well as Enelow's discussion of the literature on majority cycles in this volume).

These theorems, though, have less to do with modeling elections or legislatures than with describing the social preference order as it is defined by majority rule and its variants. Although the labels attached to things convey the impression that these results concern elections or legislatures — candidates choosing platforms, voters choosing between platforms, legislators voting over agendas, and parliamentary bodies negotiating coalitions or governments — those results tell us more about the abstract properties of majority rule applied to Euclidean preferences than anything else. Converting these analyses into models of actual political processes requires greater attention to institutional structure and to the strategic imperatives that alternative structures allow. Indeed, as McKelvey (1979:1106) himself argues, "any attempt to construct positive descriptive theory of political processes based on majority rule ... must take account of particular institutional features of these systems, as the social ordering by itself [and by implication, the analyses that focus exclusively on the properties of that ordering] does not give much theoretical leverage."

It is at this point -- supplying institutional detail and giving substantive meaning to an abstract characterizations of social preference orders — that the analysis of elections and the analysis of committees diverge. Different institutional contexts present us with different restrictions on choice, different relationships between choice and outcomes, different strategic circumstances and opportunities, and, therefore, different analytic challenges. The more theoretical implication of McKelvey's argument, though, is that the spatial analysis of committees and elections is but a specific application of game theory -- of ideas about strategies, extensive forms, strategic forms, and equilibria -- where the precise form of that application is determined by the institution under investigation. Spatial analysis offers a specific characterization of the preferences of some decision makers (voters and committee members) and of the strategies of others (candidates). However, we can say that we possess a model of an election, a parliament, or a legislative committee only after we take into account those features of the situation that define the relationship between choice and outcomes and that structure the inter-relationships among different decision makers.

The limitations of spatial analysis we describe subsequently, then, fall into two categories. First, there are those things that pertain to the adequacy of the spatial assumption itself — the adequacy of a Euclidean conceptualization of issues and individual preferences and the extent to which relevant decision makers can be said to operate within the same spatial conceptual framework. Second, there

is the adequacy of the models we use to describe institutional structure --the adequacy of the game-theoretic description of the things that structure choices and dictate the correspondence between choices and outcomes over which spatial preferences are defined.

COMMITTEES: The issue for committees is understanding the nature of cooperative (coalitional) agreements, how those agreements are sustained when they must be self-enforcing, how alternative procedural rules influence final outcomes, and how the messages of voters as expressed in elections are transformed into public policy within a legislature or parliament. We cannot review that literature in detail here except to highlight a few general things.

The usual and simplest approach to the spatial analysis of committees is to suppose that members of the committee are finite in number, have spatial preferences, and must choose a policy in the policy space using some exogenously determined rule such as unconstrained majority rule bargaining, a binary agenda (e.g., an amendment agenda), or issue-by-issue voting. In the case of the first alternative -- if the committee is assumed to be unconstrained by any explicit procedural rule aside from the provision that a majority coalition can dictate any outcome — there is an immediate correspondence between that committee's structure and the usual context of social choice theory's analysis of majority rule. The usual cooperative solution hypothesis for a simple majority rule game is the Core -- the set of outcomes that are undominated in a majority vote by any other. Thus, although a Condorcet winner does not correspond identically to the Core (a Condorcet winner is the Core because it dominates all other alternatives whereas an element of the Core need not be a Condorcet winner), the correspondence is close enough to tell us that with spatial preferences, a non-empty Core exists, for the most part, only if a Condorcet winner exists (for precise relationships see Sloss 1973) -- only if the distribution of ideal points within the committee satisfies some strong regularity conditions (Davis and Hinich 1966, 1967, Plott 1967, Davis, DeGroot and Hinich 1972, Kramer 1973, McKelvey and Schofield 1986; again, see Enelow's survey in this volume).

Prior to the focus on Euclidean preferences, judgements about the severity of the likelihood of a non-empty Core or a Condorcet winner -- of being able to make an unambiguous prediction about final outcomes -- were based on two things: (1) since the Core is empty for constant-sum cooperative games, an assessment of whether the committee's environment is constant sum; and (2) tabulations of the relative frequency of cyclic social preferences (i.e., for a committee of a given size and for a given number of alternatives, what is the probability, if all preference orders are equally likely, that the social order is transitive?). Neither of these approaches, though, gave much general insight because, first, a non-constant sum environment is merely a necessary but not a sufficient condition for a non-empty core, and, second, the assumption of equiprobability is merely a null-model assumption without an empirical referent. But once McKelvey (1976) addressed this problem in a spatial context, we understood more fully the potential for intransitivity and empty cores.

The theoretical pervasiveness of social intransitivities caused considerable consternation and led at least one eminent student of the field to conclude that a science of politics may be a practical impossibility (Riker 1980). However, the fact that a wide class of games possessed empty cores came as little surprise to game theorists, who were armed with a variety of hypotheses -- V-sets, bargaining sets, kernels, value theory, and so on — to treat those circumstances in which every outcome can be dominated (defeated in a majority vote) by some other. Unfortunately, all such hypotheses suffer from two inadequacies.

First, these ideas are based on ***ad hoc*** assumptions about bargaining and the properties of outcomes, which derives from the practical difficulties of modeling most interesting cooperative processes using extensive- or strategic-form representations. Although it is important not only to make predictions about coalitions and outcomes but also about how agreements are enforced, not all cooperative processes can be reduced to some analytically tractable extensive- or strategic-form game, which is what we require for the study of this subject (for political models that examine enforcement see Baron and Ferejohn 1987, McKelvey and Reisman 1991, Niou and Ordeshook 1990, 1993, Bianco and Bates 1990). Indeed, the study of parliamentary and legislative coalitions seems resistant to such models owing to the complexity of the processes within these institutions (on the other hand, see Baron 1991 for one such extensive form model of parliamentary government formation). Thus, as a substitute for complexity (and, in all likelihood, as a substitute for equally ***ad hoc*** assumptions that would need to be imposed to form a tractable model to study enforcement) the game theorist imposes the ***ad hoc*** assumptions upon which cooperative coalition theory rests such as equally probable coalitions, and internal and external stability (for an attempt to bring some axiomatic coherence to this literature see Schwartz 1990).

The second problem with cooperative solution theory derives from the first. Because cooperative theory does not model the institutional context of bargaining, we cannot judge it in the same way we judge an extensive or strategic-form model of some political-institutional situation -- by how well assumptions model institutional structure. Instead, aside from results about existence or uniqueness, we can only judge a particular solution hypothesis by seeing whether its predictions are "reasonable." Unfortunately, the traditional framework of cooperative game theory — games in characteristic function form -- provide too abstract a basis for judging reasonableness; fortunately, the spatial analysis of majority voting games gives us what we need. Not only do we learn that the Core is almost always empty with spatial preferences, but we also learn that the V-set and the several bargaining sets offer predictions that are unsupported by experimental and empirical evidence (see McKelvey, Ordeshook, and Winer 1978, Fiorina and Plott 1978 and, for a general survey of the experimental literature employing spatial preferences, McKelvey and Ordeshook 1990b). And

although we do not yet have a wholly general and universally accepted hypothesis about coalition formation for even simple majority games, spatial preferences provide an important arena for such explorations (Schofield 1985, and Bennet and Zame 1988, Sharkey 1990) since it is there that we can also learn whether a solution hypothesis matches the experimental literature and whether its predictions give us insight into actual parliamentary coalition processes (see, for example, Ordeshook and Winer 1980, Laver and Schofield 1990).

Insofar as its substantive accomplishments are concerned, the spatial analysis of committees, as revealed by Poole and Rosenthal's (1985, 1991) extensive historical study of voting in the U.S. Congress, is that its conceptualization of preferences allows an especially convenient summary of issues, ideology, and legislative party alignments (see also Hoadley 1987). Using all roll calls from all congresses beginning with the first, Poole and Rosenthal portray the emergence and disappearance of issues, the formation and dissolution of parties, and the correspondence between ideology and public policy choices, so as to provide a geometric representation of American political history as that history is reflected in the issues that arise in the Congress and legislators' votes on them.

The contributions of a spatial perspective would be slight, though, were those contributions limited to developing an ad hoc solution theory and facilitating the development of methodologies to measure itself. In fact, its primary contribution to our understanding of legislative and parliamentary processes is the framework it provides for studying the various procedures committees can use to reach a decision. In particular, spatial preferences serve as an especially convenient structure for analyzing such things as the manipulation of outcomes by the selection of a voting agenda, presidential vetoes, the influence of bicameralism, and government formation in parliaments. With proofs about an all-encompassing social intransitivity as common starting points, we can better understand, for example,

> the extent to which bicameralism limits outcomes over what might prevail in a unicameral legislature (briefly, the greater the spatial separation of preferences in the two legislative chambers, the more likely is a stable outcome to exist and the more likely is the status quo to be preserved; see Hammond and Miller 1987, 1989, Tsebelis 1993, and, extending some of these perspectives to the study of federalism, Weingast 1993),
>
> how various parliamentary procedures such as voting one issue at a time might induce stability (a stable point — the median on each issue -- exists if preferences on the issues are separable; Kramer 1972, Shepsle 1979, Shepsle and Weingast 1981) but such a point need not exist if preferences are not separable and committee members are strategic (see Kramer 1972 again and Denzau and Mackay 1981; for relevant experimental evidence see McKelvey and Ordeshook 1984, Wilson 1986).

- how strategic voting might limit the power of an agenda setter (briefly, although McKelvey's 1976 study of global intransitivities implies that agendas can be designed to reach any outcome if all voters vote sincerely, final outcomes are limited to the uncovered set of the alternatives on the agenda if voters are strategic; see Miller 1980, McKelvey 1986, Shepsle and Weingast 1984), and
- how such a setter can increase his power by expanding the class of agendas at his disposal (normal congressional agendas do not necessarily pit the winner of the previous vote against a new alternative. Thus, they are not of the amendment agenda type and this increased flexibility enables a dictatorial agenda setter to reach outcomes that lie outside of the uncovered set; Ordeshook and Schwartz 1987).
- the circumstances under which strategic voters vote sincerely and, therefore, the circumstances under which it is impossible to ascertain the sophistication of committee members (Austen-Smith 1987)

Although much of what we know about agendas does not presuppose spatial preferences, stripped of the Euclidean topology on outcomes and preferences, our theoretical results would lack geometric intuition and would confront the reader with an even greater array of uninterpreted notation and definitions. More substantively, this literature, taken as a whole, reveals not only the importance of institutional structure but alerts us to the likelihood that many if not most of the interesting battles in a legislature or parliament occur over the selection of rules and procedures. In fact, although an institutional structure, once established, can either induce stability or limit the range of possible outcomes, the power of institutions to manipulate final outcomes implies that group preferences over institutions are subject to the same social intransitivities that we can ascribe to simple majority rule with spatial preferences over policy (Coleman and Ferejohn 1986).

It is here, then, that we begin to see the role of constitutions and social norms. If the alternative rules for selecting a final outcome inherit the intransitivity we ascribe to outcomes, if the rules for selecting those rules inherit the same thing, and so on, then we confront an infinite regress in which it becomes impossible to argue that institutions can so structure political process as to induce stability. Indeed, not only is stability and prediction impossible, but, in more practical terms, it becomes impossible for society to choose (although, to confuse matters further, failing to choose can be conceptualized as a choice). One resolution of such a regress -- one way to avert the presumed irrationality or inefficiency of never choosing -- is to terminate it through convention (tradition or norms). In the event that such conventions do not exist, society must try to invent them, where among these invented conventions are the things we call political constitutions. Thus, although our route seems indirect, the spatial analysis of committees and social choice takes us to a core question

in political theory -- how society chooses conventions, and why certain conventions "stick" whereas others do not.

Of course, it is unreasonable to require that spatial analysis alone answer such questions. As we noted earlier, spatial analysis is but a specific application of a more general theoretical structure, game theory, and we must appeal to other elements of that theory (e.g., the properties of alternative equilibria, individual beliefs and conjectures) to learn why some rules prevail and others do not (Ordeshook 1992, Niou and Ordeshook 1993b). Nevertheless, we can offer criticisms of the literature on committees that parallel those we offer later of the spatial analysis of voting and elections. First, with but a few exceptions (see, for instance, the exceptions discussed in Section 3 of this essay), only the simplest institutional forms are examined -- simple coalitional processes, simple amendment agendas (despite the fact that legislatures rarely use this procedure except for the simplest circumstances, although see Ordeshook and Schwartz 1987, Banks 1989), rules that are imposed exogenously (but see Banks and Gasmi 1987 for preliminary analysis of endogenous agenda formation), and legislative sub-committees that abide by rigidly adhered to restrictions on the issues in their domain.

Second, the assumption that legislators or deputies have spatial preferences over policy appears to preclude a full treatment of the relationship between their actions and the actions of those who elect them. The preferences of representatives derive presumably from the preferences of their constituents. Assuming otherwise is to admit the possibility of being inconsistent with the usual assumption that the primary goal of election candidates is to win. That is, it seems inconsistent to suppose that politicians, when acting as legislators or as parliamentary deputies, act in accordance with personally held well-defined policy preferences, but when acting as candidates, wholly ignore those preferences and simply adhere to equilibrium strategies defined by the electorate's preferences and the relevant election rule. Specifically, if the electorate's preferences yield an intransitive social order -- the essence of McKelvey and Schofield's analysis of majority rule -- then what is the basis for supposing that the preferences of a representative are otherwise?

There is, then, a serious theoretical disjuncture between the spatial analysis of legislative and parliamentary committees and that of elections. This disjuncture awaits closure, but even if it can be achieved it is anything but certain that it would sustain a spatial conceptualization of legislative or parliamentary preferences (special assumptions about probabilistic voting are sufficient to ensure a transitive order for the electorate see, for instance, Ledyard 1981, 1984 and Coughlin 1992 -- but rarely does analysis suppose anything but deterministic preferences for legislators). Thus, although the assumption of spatial preferences allows us to structure various questions about, say, the adequacy of cooperative game-theoretic solution concepts and to recast our understanding of specific parliamentary procedures, and although Poole and Rosenthal's (1985, 1991) empirical research gives

us some confidence that the spatial representation of preferences can be used to summarize important aspects of the issues that the U.S. Congress has confronted in its history, we must remain uncertain as to the full generality of these analyses as models of real legislative and parliamentary institutions and processes.

ELECTIONS: Insofar as the spatial analysis of elections is concerned, that analysis has contributed to our understanding of some fundamental things with respect to two-candidate plurality rule systems. Most fundamentally, we know that there are good reasons for supposing that, for a wide range of circumstances, if two candidates compete in a plurality-rule system, they espouse policies near the "center" of the electorate's preference distribution. That is, simple majoritarian processes, even if they do not yield Condorcet winners or some other simple equilibrium of strategies, generate powerful incentives for the approximate convergence of policy by the two candidates or parties that are assumed to be competing.

This centrist tendency of two-candidate winner-take-all-elections is established in several ways. AH of them begin by formulating an election as a two-player non-cooperative game: Typically, citizens are robots who merely choose between voting for a preferred candidate or abstaining; the active players in the election game are candidates whose strategies consist of alternative positions in the policy space. Because Condorcet winners cannot, by definition, be defeated when paired against any other alternative, such a winner is the Nash equilibrium to this game. That is, if both candidates choose the Condorcet winning alternative as their platform, neither candidate has a unilateral incentive to move to some other platform or, equivalently, no party has an incentive to nominate a candidate who advocates a policy other than the Condorcet winner. We can then appeal to those essays, beginning with Black, that assume spatial preferences and that establish sufficient conditions (e.g., unidimensional single-peaked preferences, radially symmetric distributions of preferences) for the existence of such a winner. It is in this literature, then, that we find the Median Voter Theorem, which identifies the electorate's median ideal preference as a Condorcet winner when preferences are unidimensional.

Second, there is the research that substitutes probabilistic voting for the Median Voter Theorem's assumption that voters vote deterministicly -- for the assumption that small changes in a voter's evaluation of a candidate can effect critically a voter's choice of candidate. Instead, probabilistic voting supposes that the relationship, between policy, preference, and choice is continuous. By thus "smoothing" functional relationships and desensitizing outcomes to incremental changes in candidate strategies, the existence of an equilibrium is more readily established (see, for example, Hinich, Ledyard, and Ordeshook 1972, Ledyard 1981, 1984 and, for a general survey of this line of research, Coughlin 1992).

Finally, McKelvey and Ordeshook (1976) and McKelvey (1986), appealing to ideas like Miller's (1980) uncovered set and its derivative concepts, show that even if a Condorcet winner does not exist, but if candidates eliminate dominated strategies, then the candidates continue to adopt centrist policies (see also Feld, Grofman, and Miller 1988, Miller, Grofman and Feld 1989). In fact, we also know that the uncovered set is contained in another set called the Yolk, which generally shrinks as preferences become more dense (Ferejohn McKelvey and Packel 1984, Koehler 1990, Tovey 1993a). Thus, although the social preference order might be intransitive, there exist spatial positions that can be judged "better" than others (positions that, as strategies, dominate other strategies), where the adoption of these better positions keeps candidates from wandering "too far" from some center of gravity of preferences, and where "too far" can itself be given precise meaning.

Much of the research that followed Downs, Davis, Hinich, and Plott can be interpreted as ascertaining the robustness of this conclusion about the centrifugal force of winner-take-all-elections. After all, there are a great many assumptions that must be satisfied before we can assert that the preceding analyses have much to say about politics. These assumptions include:

> only two candidates compete and neither candidate fears the entry of a third competitor;
>
> candidates are concerned solely with winning rather than with the policy positions they must espouse to win;
>
> voters are fully informed about the candidate's platforms;
>
> voters are fully informed about their own preferences;
>
> candidates are fully informed about the issues that concern voters, voter preferences on these issues, and the relative salience of issues;
>
> candidates do not deliberately render their platforms ambiguous even if clarity alienates potential support;
>
> candidates possess full spatial mobility and are unconstrained by such things as the need to be nominated by a party before running in the general election or by the fact that voter perceptions of candidates change only slowly to the extent that they are based on retrospective evaluations;
>
> victorious candidates, like dictators, implement their campaign platforms unencumbered by other political actors -- candidates keep their promises;
>
> campaign dynamics are irrelevant insofar as we can sustain the assumption that both candidates reveal their platforms simultaneously;
>
> all eligible voters vote.

This is a list that is likely to dissuade most people from believing that spatial theory can contribute much to their enterprise. On the other hand,

Calvert (1987) establishes that even if candidates hold policy preferences, the competitive forces of two-candidate elections compels them nevertheless to converge to policies near the center.

Shepsle (1972) and McKelvey and Richelson (1974) consider the possibility that candidates might deliberately choose to offer ambiguous platforms; however, if all voters are risk averse, the median preference retains its attraction to candidates.

Aranson, Hinich, and Ordeshook (1974) establish that if plurality is a random variable whose mean is determined by the candidate's platforms, and if the election is otherwise symmetric, then the maximization of expected plurality and probability of winning are equivalent in the sense that they imply the same election equilibria.

Harrington (1991a,b) and Austen Smith and Banks (1989) explore the circumstances under which candidates have an incentive to keep campaign promises, and thereby they begin a formalization of the idea of retrospective voting.

McKelvey and Ordeshook (1985a, 1986) and Bowden (1989), in research that is closely related to Harrington's, employ the idea of rational expectations to show that even if candidates do not know the precise nature of voter preferences, even if most voters are only imperfectly informed about the candidate's policy positions, and even if all fully informed voters prefer extremist policies, indirect sources of information such as public opinion polls and campaign endorsements are sufficient, in equilibrium, to allow uninformed voters to vote "correctly" and to induce the candidates to centrist policies (see also Bernhardt and Ingberman 1985, Ferejohn 1986).

Lupia (1992) shows how the perspectives developed be McKelvey and Ordeshook (1985a) to treat imperfect information can be extended to analyze voting on referenda in a spatial context.

As Ordeshook (1970), Hinich, Ledyard and Ordeshook (1972), Ledyard (1981, 1984), and Coughlin (1992) establish, non-voting, when formulated as a probability of voting, does not necessarily negate the centrist tendency of two-candidate winner-take-all elections, although Hinich (1977) does show that probabilistic voting can lead candidates to converge to the electorate's mean rather than median preference.

As a partial response to the finding that Condorcet winners and uncovered sets may be difficult to compute and find (Bartholdi, Narasimhan, and Tovey 1990, Tovey 1992b), Kollman, Miller and Page (1992) show that even if candidates are boundedly rational adaptive decision makers operating in an environment of incomplete information, the median preference nevertheless exerts a powerful influence on their strategies if those strategies are dictated by some simple search algorithms.

A large experimental literature also demonstrates the robustness of spatial theory's primary results. For example, Collier, McKelvey, Ordeshook, and Williams (1987) demonstrate under a variety of experimental conditions that the median preference exerts a powerful influence on candidate platforms even when voters can vote only retrospectively. Indeed, the median retains its attractiveness even if candidates are rewarded when deviating from it (provided that they realize that reward only if elected) and even if random events perturb their true spatial positions (McKelvey and Ordeshook 1990). Moreover, sophisticated voters, learning that candidates converge, learn also to minimize the cost of voting by choosing to act retrospectively and on the basis of candidate reputations or party labels (Williams 1991, McKelvey and Ordeshook 1985b, Collier, Ordeshook and Williams 1989, Plott 1992).

Admittedly, the attractiveness of centrist policies is weakened by the threat of entry (Palfrey 1984, Greenberg and Shepsle 1987), by the existence of more than two candidates and voters who can cast preferential ballots (Cox 1990b, Denzau, Katz, and Slutsky 1985), by non-voting that derives from alienation within the electorate (Hinich and Ordeshook 1969), by institutional variations that distort the competitive forces generating the alternatives voters confront (Romer and Rosenthal 1978, 1979, Rosenthal 1990), and by the demands of campaign contributors (Aldrich 1983). Nevertheless, despite the caveats and footnotes that must accompany any summary assertion, majoritarian electoral institutions exert a powerful centrifugal force on candidates and parties. If we combine this conclusion with the formal proofs of Duverger's (1956) hypothesis about winner-take-all systems (Palfrey 1989, Fedderson, Sened and Wright 1990, Fedderson 1992), which assume that voters vote strategically for viable candidates or outcomes, we find at least one institutional arrangement — winner-take-all elections in which the victorious candidate has a relatively free hand at implementing his or her campaign platform -- that yields but a few political parties (two in the abstract equilibrium of the models), all of whom compete with centrist policy platforms.

Of the caveats to this conclusion, though, perhaps none is more important than that its validity depends critically on the assumption that centrist policies in fact exist. The validity of this assumption, in turn, depends on the type of issues that voters use to evaluate candidates and policies. The usual spatial preference structure assumes, first, that there is a consensus on the criteria (spatial issues) used to evaluate candidates as well as a consensus on how public policy and candidate election platforms map into this set of criteria. Second, that structure assumes also that although people's preferences may differ, there is sufficient commonality of interests to allow similar policy preferences as well as agreement about policies that ought to be avoided. Generally, voters are assumed to evaluate candidates and public policy on the basis of some small number (usually one, two or three) of generalized issues (ideological or otherwise) and ideal points are assumed to cluster sufficiently so that their distribution can be described by standard probability density functions.

There is, though, an alternative to this structure. If voters conceptualize policy in redistributional terms so one person's gain can come only at the expense of someone else, then the usual spatial representation may be inappropriate. Indeed, when the things a government supplies to its citizens are perfectly divisible, transferable, and in constant supply, we can require one dimension for every person or household in society to represent preferences and ideal points will be widely scattered and located at the vertices of the constraint that defines feasible policy. In this event, there is no reason to suppose that candidates or parties converge (approximately or otherwise) to anything (barring some very specialized assumptions about probabilistic voting, see Coughlin 1992). No proposed coalition is invulnerable to disruption by an appropriately: constructed counter-proposal, and the only prediction we can offer about final outcomes is that each candidate tries to form some majority coalition and proposes to expropriate all things from those excluded from the coalition.

Thus, the applicability of spatial theory's fundamental theoretical results depends not only on the relevance of a rather special and simple institutional arrangement, but also on the types of issues that arise the structure of preferences over them. Thus, it is essential to understand that the spatial perspective is not intended to be a universal one, but rather a perspective that is only more or less relevant to politics, depending on the nature of the issues that concern society.

2. Fundamental Conceptual Achievements

If we want to understand better spatial theory's limitations as well as what it contributes to our understanding of politics, we need to consider why it focuses on 2-candidate winner-take-all-elections. First, there is the fact that the existence of equilibria -- at least of pure strategy Nash equilibria — is more difficult to establish when we allow more candidates or more complex institutional arrangements (see, for example, Greenberg and Weber 1985, Greenberg and Shepsle 1987, Cox 1987, 1990a, 1990b). Suppose we try to model party-list proportional representation systems in which final policy is dictated by a governing parliamentary coalition. If we assume that voters pay some attention to the governing coalitions that form after votes are counted and parliamentary seats allocated, then we are stymied by the fact that we possess only partially satisfactory treatments of coalition formation in committees that allow for the calculation of possibilities in a way that allows unambiguous inferences about the policy consequences of different electoral outcomes, and, thus, that allows an unambiguous definition of best response strategies for voters (for some attempt to grapple empirically with this problem see Rosenthal and Sen 1973 and 1977 whereas for an initial theoretical excursion see Baron 1993).

Second, as political scientists became more adept at game theory, they came to appreciate the necessity for pursuing Farquharson's (1969) agenda of allowing voters as well as candidates to act strategically. But whereas strategic and sincere voting are equivalent in two-candidate plurality

elections, the analytic difficulties of allowing strategic voting under nearly any other institutional arrangement at times appear insurmountable and tractable only with heroic assumptions (see, for example, Myerson and Weber 1993). Compounding matters further, of course, is the fact that whenever large numbers of decision makers are allowed to act strategically and when few or none of them possess dominant strategies, then typically there are a multiplicity of equilibria. In this event, it can be a supremely challenging task to characterize all of them, which is what we must oftentimes do before we can begin eliminating certain equilibria as reasonable predictions.

A final explanation for spatial analysis's focus on two-candidate winner-take-all elections is that there does not appear to be any other institutional structure that.serves as a convenient focus for research. Moving from the simple winner-take-all format confronts the researcher with a long list of possibilities — the single non-transferable vote (SNTV), approval voting, party-list proportional representation, the single transferable vote (STV), unicameral versus bicameral legislatures, presidential versus parliamentary systems, line-item vetoes -- so that every research paper threatens to assume the character of being but a highly specialized (read: narrow) creature.

This is not to say that we cannot find valuable contributions to our understanding of alternative or more realistic institutional arrangements.

> Austen-Smith and Banks (1988), for example, establish the strategic complexity inherent in proportional and parliamentary systems as well as the fact that, owing to strategic voting, proportional representation systems need not produce an allocation of seats across parties that matches the distribution of preferences.
>
> Hinich and Ordeshook (1974) show how the Median Voter Theorem can be adapted to predict the policy biases of an electoral college as compared to a direct vote, and show, in particular, that although the electoral college can be credited with biasing policy in the first half of this century, the increasing homogeneity of the country has virtually eliminated the main sources of bias today.
>
> Cox, in a sequence of essays (1984a, 1984b, 1987, 1990a, 1993), has made a concerted effort at modeling alternative voting systems with the idea of contrasting equilibria in them with those found in winner-take-all systems. Of special note is his classification of election systems that yield convergent equilibria and those that yield divergent ones (Cox 1990a, 1990b: briefly, convergence is less likely in systems that allow voters to express second, third, etc. preferences or that give voters few votes relative to the number of seats and candidates), as well as his analyses of strategic voting under SNTV that provides a theoretical explanation for Reed's (1991) finding in Japan of an apparent equilibrium of $\mathbf{5}+1$ competitive candidates, where $\boldsymbol{s}$ is the

number of seats in an election district (Cox 1993; see also Osborne and Slivinski 1993 and for a survey of earlier research into multi-candidate elections, Shepsle 1991). Baron (1993) explores the positions of parties in a parliamentary system with proportional representation and offers a sequential bargaining model in which those positions diverge in accordance with our empirical understanding of the consequences of party-list proportional representation, and where the extent of that divergence depends on the process whereby government coalitions are negotiated.

It is true that these analyses rely on specialized assumptions. The models of Austen-Smith and Banks and Hinich and Ordeshook are one-dimensional and Baron's is two-dimensional; Austen-Smith and Banks assume a transferable good among voters, while they, like Baron, allow only three parties; and Cox's analysis of SNTV assumes that candidate positions are fixed. Each essay, then, is but an incremental advance and none provides a general theoretical result. Nevertheless, we can see in them spatial theory's potential for contributing to our understanding of even these more complex political processes. First, and as I have already noted, it offers a formal conceptualization of individual preferences that links political theory to economic theory (Kramer 1973, Klevorick and Kramer 1973, Boylan, Ledyard, and McKelvey 1993) and thereby promises a rigorous synthesis of these two disciplines.

Second, because spatial analysis formulates election competition as a game, it brings to the analysis of elections all the tools of game theory as well as an appreciation of the ambiguities inherent in decision contexts in which people's fates are interdependent (e.g., the implications of multiple equilibria, the role of coordination in equilibrium selection, the ambiguities in extensive form - representations of social processes). Game-theoretic reasoning in the study of elections, moreover, compels us to consider issues that might otherwise be ignored, such as strategic voting in mass electorates. Indeed, it is not unreasonable to argue the profound importance of this contribution as we begin to try to understand not only how political elites might respond to alternative institutional structures, but also how "ordinary" citizens respond to them as well.

Third, and somewhat paradoxically, spatial analysis deepens our understanding of politics by it failure to resolve some issues. One of the most pervasive findings of the literature that seeks to identify issues and measure preferences using multidimensional scaling is that, regardless of the electorate under consideration, only one or two issues is required generally to represent preferences (see, for instance, Enelow and Hinich's 1984 and Poole and Rosenthal's 1984 analysis of American voting, Poole and Rosenthal's 1985, 1991 study of issues in the U.S. Congress, and Chu, Hinich, and Lin's 1993 analysis of spatial preferences in Taiwan). On the other hand, more direct evaluations of voter perceptions and of candidate and party platforms suggest that the number of issues greatly exceeds this small number (see, for example, Aldrich and McKelvey 1977 and Niou and Hsieh 1993).

Thus, spatial analysis raises important questions about the very meaning of the concept of an issue, about the meaning of public opinion, and about the relationship between public policy and people's evaluations of policy. Enelow and Hinich (1984) and Hinich and Pollard (1981) offer a distinction between "actionable issues" and issues in some basic policy space in which voters somehow imbed actionable issues. The analysis and conceptualization of this idea of multiple layers of issue spaces, though, remains abstract and fails to address such basic matters as how voters connect these layers or even how political elites might try to manipulate the connections. Of course, it may be unreasonable to require that spatial theory alone address such matters. At the very least, then, it is important to appreciate that spatial theory makes evident a research matter that, although fundamental to our understanding of politics, has gone largely unnoticed by researchers who employ different conceptual tools.

Fourth, a spatial conceptualization provides a convenient basis for comparing the performance of alternative institutions. As suggested throughout this essay, the questions we can hope to ask and answer include: Which voting systems satisfy various welfare criteria, such as ensuring the selection of a Condorcet winner when such a winner exists? What are the implications of direct democracy devices such as the town meeting and popular referenda as compared to the indirect mechanisms of representative democracy? What is the influence of such things as bicameralism, presidential vetoes, and legislative subcommittees on final outcomes? Without a spatial topology on preferences, the comparison of outcomes is difficult and an evaluation of the welfare consequences of any difference -- a qualitative evaluation of the meaning of "significant difference" — nearly impossible to form (see, for example, Tsebelis's 1993 use of spatial analysis to address the debate over the virtues of presidential versus parliamentary government).

Finally, we should appreciate what even the basic elements of spatial theory tells us about the very foundations of democratic theory. Although the Median Voter Theorem is the best-known result, it is in fact not the most important. That theorem merely establishes a sufficient condition for a rather specialized type of equilibrium. Instead, the most important result appears when we compare the policies likely, to prevail in simple plurality rule elections with those likely to prevail in cooperative committees with those likely to prevail in committees using restrictive binary agendas. Specifically, if preferences are Euclidean, then the theoretical prediction is that all three abstract institutional mechanisms yield outcomes that fall within the same subset of the policy space — the uncovered set or some nearly geometrically equivalent subset (although it makes use of a number or results -- see, for instance, McKelvey and Ordeshook 1976 and Shepsle and Weingast 1984 -- the formal structure of this argument is best summarized in McKelvey 1986).

Among the many things this result accomplishes is that it answers a fundamental question about representation. Specifically, it tells us that, in principle at least, we should be indifferent between

having policy determined by a two-candidate election and having the entire electorate meet to debate matters using simple majority rule. Because this latter possibility is impractical, we can view two-candidate elections as a practical solution. Moreover, to the extent that preferences within a legislature mirror those of the electorate, we should also be indifferent between letting the legislature determine outcomes, using whatever agendas it might choose to give coherence to its deliberations, and letting the electorate choose using agenda procedures of its own design. Once again, then, legislative representation can be viewed as a practical response to the fact that electorates of even a few million persons cannot duplicate the New England town meeting.

Spatial theory, then, provides us with a basic null model of democracy, against which we can compare realistic alternatives and answer such questions as: how great a distortion in the equivalence of these three forms is occasioned by the fact that few voters pay much attention to the policies that legislators enact? What distortions arise owing to alternative schemes of districting and alternative methods of conducting elections? Are there representation schemes that allow a political system to approximate, in terms of final outcomes, a wholly collegial electorate?

.3. Science and Engineering

Despite spatial theory's accomplishment, we cannot escape the fact that the formal theoretical results it offers impose assumptions that depreciate their immediate practical application. Anyone trying to use them to convince a constitutional reformer, for example, that he or she ought to prefer one type of political institution rather than another will meet with failure if not a sense of frustration about the seeming irrelevance of established theorems. Of what interest is it to know that plurality rule induces centrist policies provided that some list of ten or so assumptions are satisfied? Where are the results that assist reformers who must try to establish stable political institutions in the deteriorating economies of the successor states of the former Soviet Union? Can we say whether large or small election districts under SNTV best facilitates the development of political parties? Does spatial theory tell us anything about how to construct a viable federal government with corresponding election rules in an ethnicly heterogeneous society? What advice can we offer about alternative methods of electing a president, or even about whether to adopt a presidential or parliamentary system, that students using less analytical methods and perspectives cannot also provide?

In fact, the problem here has less to do with any inherent limitation of spatial theory than it does with the character of political science itself and with an imperfect understanding on the part of formal theorists of their ultimate objective. Once we appreciate this objective and the way it is achieved, then we can better appreciate spatial theory's contribution -- actual and potential.

Anyone familiar with electoral systems in even a few randomly selected countries appreciates that such systems come in great variety. Even if we restrict ourselves to the most basic elements of their description, we have at least the following:

Single-member districts using simple plurality rule;

Single-member district using majority rule with a runoff if no one receives a majority on the first ballot

Multi-member districts using party-list proportional representation;

Multi-member districts using a single non-transferable vote;

Multi-member districts using a single transferable vote; and

Single or multi-member district using preferential voting such as the alternative vote or approval voting.

Our descriptions can be made more complicated, moreover, by considering thresholds that parties must surpass before assuring themselves of representation, alternative algebraic formulas for allocating parliamentary seats, the possibility of pre-election party coalitions and pooling of votes, and the nature of the offices being filled. Not only must we consider whether the voter is voting for a unitary president, a collegial presidency, members of a city council, or deputies to a parliamentary body, but we must also consider the ultimate relationship of these offices to policy, since, presumably, it is this relationship that provides the voter's ultimate motivation. Finally, to make matters more complicated still, we must consider that we can easily think of situations in which voters, when entering the voting booth, are asked to participate in two or more of these variations simultaneously.

Clearly, the character of the strategic environments in which candidates and parties compete are nearly endless, and it is unreasonable to suppose that spatial theorists can model every one of them or that a handful of "fundamental" theorems can summarize the differences among them with respect to strategic imperatives of candidates, voters, and political elites. Even a cursory reading of Cox's (1990b) survey of research on multicandidate spatial competition should convince the reader that not only does the variety of election laws allow for a near infinitiy of assumptions about candidate objectives and voter decision rules, but also that simple theoretical generalizations about the structure of competition are unlikely to be forthcoming.

Nevertheless, to see what can be done, consider Romer and Rosenthal's (1978, 1979) study of school board referenda in Oregon. Briefly, the setting for their study is a referendum in which a school board can offer the electorate a take-it-or-leave-it proposal, which if rejected by voters results in the imposition of a generally undesirable reversion outcome -- frequently, a school budget of zero (for a general summary of this line of research see Rosenthal 1990).

Theoretically, their study demonstrates little more than that, by presenting voters with such a those who control the initial proposal have a powerful influence on final outcomes. Specifically, a

school board that seeks to increase expenditures can secure passage of a proposal that exceeds the spending increase most preferred by the median voter, which is only logical since voters must choose between a "bad" alternative and a wholly unacceptable one. But if Romer and Rosenthal fail to establish a theorem that establishes a wholly unanticipated consequence, what do they accomplish? We might say that they test their model and find support for its associated hypotheses. But so what, and what hypothesis would we reject if the data failed to support their model? Certainly we would not be prepared to reject the rational choice paradigm itself. Instead, we (and they) would probably argue that "other factors" mitigated against their model's strategic imperatives, at which point their results might have served primarily as input for PHD dissertations searching for these factors.

In fact, Romer and Rosenthal's contribution is more fundamental than the proof of some new theorem or falsification of some previously accepted hypothesis. Instead, they give us confidence that spatial theory's perspectives have general relevance and that it provides a way to conceptualize preferences and to model the strategic environment engendered by specific institutional structures. That is, they give us confidence that we can usefully combine a spatial conceptualization of preferences and policy with a game-theoretic model of alternative institutional structures when trying to assess the implications of those structures. In addition, they expand our experience with confronting reality with purely abstract tools and with an imaginative recombination of those tools. As such, then, they contributed greatly to what ought to be a fundamental goal of political science as a profession, political engineering — the design and assessment of political institutions.

Romer and Rosenthal's approach does not stand alone. Another innovative demonstration of the applicability of spatial analysis that directly applies the Median Voter Theorem is Klevorick and Kramer's (1973) study of the German regional assemblies used to control pollutants in the Rhine river basin, the Gennosenschaften. Within each Gennosenschaften, the voting weights of each industrial and village representative depends on the taxes paid by the relevant entity in the last period, which depends on the tax rate and the amount of pollution each firm and village chooses to produce. The tax rate, in turn, is determined by a majority vote in each Gennosenschaften and is thereby dependent on the median preference there. But, completing the cycle, this median preference depends on voting weights. So the theoretical question is whether there exists an equilibrium tax — a tax that is a fixed point in the sense that, once all persons adjust their propensity to pollute in accordance with it, the resulting voting weights imply that tax. A characterization of the conditions under which such an equilibrium exists then serves as an important component of any effort to replicate the German experience elsewhere with a similar institution structure. In the process, moreover, Kramer and Klevorick demonstrate formally how spatial preferences can be derived from preferences of the traditional economic sort -- from preferences over consumable goods and profits, combined with constraints on product inputs and production functions (see also Kramer 1973).

Both experimentally and theoretically, this derivation of spatial preferences is developed more fully in Boylan, et al (1991) and Boylan, Ledyard, and McKelvey (1993) in a way that allows spatial theory to be applied to some classic problems of political-economic development. In this model, voters must choose between immediate consumption of some publicly produced good and public investment that can be used to increase future supply of that good. Marrying the classical economic model of investment and growth to a model of two-candidate competition, where the candidates compete by proposing alternative macro-economic platforms and voters must weight future against current consumption, conditions are established for the existence of a two-candidate stationary investment-consumption equilibrium. Moreover, experimental data are offered to suggest that candidates converge to such a policy, at least when voters are fully informed about the relationship between investment and growth.

Like Romer and Rosenthal, this model is important for more than the theorems it offers (which depend critically on fine technical details) or the experimental evidence offered on its behalf (which is subject to the criticism of being little more that a complex IQ test of undergraduate subjects). Because of the model's complexity, theorems describing equilibria impose strong assumptions. It is virtually impossible to derive formally the influence of the model's most interesting parameters (for example, the information conditions of voters, the extent to which the candidates must keep their election promises, or the frequency with which incumbents are allowed to change policy). In this instance, the experimental laboratory seems the only practical device for exploring such matters. What differentiates research here from earlier experimental explorations of election processes (with the possible exception of Plott 1991) is that the stage is now set to use the experimental laboratory as a tool of political institutional design. Since most design problems cannot wait for the development of models and the proof of theorems that apply specifically to them and since such models and theorems are unlikely to be developed in any event, the experimental lab can begin performing the same function in political science as the wind tunnel does in aeronautical engineering.

Each of these studies demonstrates how the spatial analyst's perspective can structure research about political processes and institutions without abandoning scientific rigor. They also demonstrate that we must learn to understand the distinction between and interdependence of engineering and science.

I began this essay by noting that spatial analysis is but a specific application of game theory and that many of its limitations and accomplishments are those of that theory. If the spatial analyst has not fully incorporated features of, say, incomplete information and uncertainty into his models and if he considered only a few of the strategic environments that we think are important, it is largely because the analytic challenges that such circumstances present are severe. Nevertheless, there are

inadequacies that derive from an imperfect understanding of our craft and the incorrect incentives that that misunderstanding establish.

As of this essay's writing journals are inundated with manuscripts employing the latest faddish techniques and concepts of game theory -- repeated games, signaling games, stochastic games, sequential rationality, and so on. Although some of this research promises to deepen our understanding of processes that have heretofore been neglected, too often the display of technical skill is merely a substitute for thinking deeply about a problem and for confronting substantive complexity. Too often research consist merely of an adaptation of a particular idea that yields neither a general result nor anything that relates to some specific empirical phenomena or problem. The rewards from lengthening one's vita are too great to ignore, and they often result in "research" that is little more than the repeated application of some newly learned "hammer" to slightly modified "nails."

Of course, fads die and what frequently remains is a residue of new insights, along with an augmentation of the technical skills of the profession. However, it remains true that with attention focused on mere mathematical manipulations and with promotions arriving most quickly to those who can sustain a stream of publications, the type of research cited in this section is often undervalued. Instead, we find manuscripts with a minimal ratio of meaningful results to notation and in which things loftily proclaimed to be "theorems" are based on such restrictive assumptions that they contribute little to our understanding of anything. It would seem that it is often easier to theorize about a Planet X than our own.

The application of game theory and spatial analysis will achieve maturity when practitioners suppress the instinct to begin essays with silly sentences like "assume an infinite sequence of candidates," or "we find an equilibrium such that..." and to avoid burdening the reader with notation that promises a degree of generality that is lost with the first assumption. We must learn to devalue notation in favor of theoretical insight and to ape not the mathematicians craft but to develop one of our own.

Nevertheless, we are encouraged not only by the essays reviewed in this section but also by the increasing amount of research directed at classes of elections systems other than simple plurality rule and that are motivated by substantive rather than analytic concerns. Of course, there remain any number of issues concerning party formation and fragmentation or of the advantages and disadvantages of presidential versus parliamentary systems that can only be understood with the rigor that spatial analysis and game theory promises. The case studies upon which our current understanding rests are too few in number to permit definitive conclusions. But to pursue these issues, we must learn to value something in addition to a nicely arrayed lemmas and theorems. These are valuable things. But too often, "political scientists" prove theorems about things that are of such

a degree of complexity that the resulting lemmas, propositions, and theorems require assumptions of such specificity as to preclude generality and even relevance. If the list of assumptions required to establish a particular result exceeds what can be summarized in a single breath, then we have a good indication that the word "theorem" ought to be banned from the corresponding manuscript. At a minimum we must learn to differentiate between those things that can be stated as general principles (e.g., the Median Voter Theorem, the McKelvey-Schofield results about cycling, Duverger's Law) and those things that are merely complex combinations of those principles (e.g., manifestations of strategic complexity owing to incomplete information in particular environments).

At the same time, we must learn to value more practical objectives. Natural science does not progress merely because the phenomena natural scientists study are less complex than social processes (which may be true), nor are the engineering efforts that feed off natural science theory successful merely because they rest on a firm theoretical base. Instead, success derives from the interaction of these enterprises. The search for solutions to practical problems uncovers new problems and empirical regularities that are then subject to general theoretical inquiry and explanation, and theoretical results are "tested" when we try to use them to facilitate the discovery of solutions to practical problems.

Unfortunately, this interplay is largely absent from political science. As a consequence, the proponents of formal political analysis too often fail to differentiate between the things that allow pure theoretical investigation and the things that must be studied without resorting to the mass production of lemmas and theorems. Nevertheless, despite the frequent critical tone of this essay, it should by now be evident that spatial analysis -- owing to its general structure, to the well-defined problems it poses, and to its self-evident shortcomings -- promises to be fertile ground for the synergy of science and engineering. What remains is merely a better developed interest in solving specific practical problems of institutional design and the rewards that come from doing so.

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