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The Development of Contemporary Political Theory

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Abstract

Bold claims are made about contemporary political theory's accomplishments since that theory was inagurated by the writings of Arrow, Downs, Black, Riker, Buchanan and Tullock, and Olson. Others argue, however, that this theory suffers from too great a concern with mathematical notation and too little concern with substantive relevance. This essay argues that both views are essentially correct — that although our understanding of politics (and of economics) remains in such a primitive state that we cannot ignore the practical necessity for considering other, less formal modes of inquiry, something has been accomplished. In particular, we argue that the current paths of development of political theory — the discovery of First Principles – are identical to those set forth by arguably the most successful set of "political engineers" in history — the founders of the American republic. However, we also argue that many of the shortcomings of contemporary theory derive from a failure to appreciate something that was apparent to those Founders; namely, the distinction between science and engineering and the proper role of empirical and experimental analysis.

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The Development of Contemporary Political Theory

In his notes on the Federal Convention, James Madison (1898:108-9) reports the following comments by Benjamin Franklin with respect to the manner in which judges ought to be chosen:

> *Doctor Franklin observed, that the two modes of choosing Judges had been mentioned, to wit, by the Legislature and by the Executive. He wished such other modes to be suggested as might occur to other gentlemen; it being a point of great moment. He would mention one which he had understood was practised in Scotland. He then, in a brief and entertaining manner, related a Scotch mode, in which the nomination proceeded from the lawyers, who always selected the ablest of the profession, in order to get rid of him, and share his practice among themselves. It was here, he said, the interest of the electors to make the best choice, which would always be made the case if possible.*

Thus, with a particular problem in hand, Franklin identifies the components of rational self-interest and suggests an incentive-compatible mechanism for making the desired social choice. So the question that confronts us in this essay is whether, in the 200+ years since Franklin spoke, we have advanced the science of politics in an appreciable way.

To render a judgment about contemporary political theory, we should return to the post-World War II era, because it was then that the research regarded by political scientists as path-breaking was not an outgrowth of economics or the rationalism of the 18th century, but rather of the "behavioral revolution." In response to the purely institutional descriptive mode of inquiry that was previously dominant, this revolution sensitized us to the importance of socio-psychological variables and statistical methodologies, and to the relevance of public opinion and its measurement. But much of that research followed a path in which scholarship was judged by the size of ones's data set and by the novelty of the statistical tools employed, rather than by its contribution to the development of a coherent, parsimonious, and deductive theory of political action. Correlation piled upon correlation, and political scientists felt compelled to become familiar with a plethora of ill-defined ideas such as anomie, alienation, cognitive dissonance, and attitude, as well as innumerable concepts defined by *ad hoc* measurements rather than theoretical constructs.

Of course, behavioralism was not the only contender for the political scientist's sole. But paralleling events of an earlier time in which the radical empiricism of the sixteenth and seventeenth centuries gave way to the rationalism of Hobbes, Locke, and Rousseau in the eighteenth century, a series of books inaugurated a second revolution in the study of politics at approximately the same time

as behavioralism's ascendancy -- a revolution that would draw our attention back to the institutional details of political activity, that would sharpen the distinction between normative and descriptive analysis, and that would reinvigorate the development of a deductive theory of political processes. The centerpieces of that revolution are Arrow's (1951) *Social Choice and Individual Values*, Black's (1958) *Theory of Committees and Elections*, Riker's (1962) *Theory of Political Coalitions*, Downs' *An Economic Theory of Democracy*, Buchanan and Tullock's (1962) *Calculus of Consent*, and Olson's (1965) *Logic of Collective Action.* These volumes brought order to political analysis and set the direction for future research so that it could begin to uncover first principles of political action in such areas as social choice theory, voting in committees, coalition formation, majority rule elections, and interest group action.

Three other books also contributed importantly to this second revolution: Luce and Raiffa's (1957) *Games and Decisions*, Farguharson's (1969) *Theory of Voting*, and Key's (1966) *The Responsible Electorate*. Although von Neumann and Morgenstern's (1945) *The Theory of Games and Economic Behavior* is the seminal volume of game theory, Luce and Raiffa's interpretative survey trained a generation of scholars to think in game-theoretic (strategic) terms. Through their effort, ideas such as Nash equilibria, strategy, extensive, normal, and characteristic function form, the core and V-sets became familiar to those who saw themselves as replacing pure empiricism with formal, deductive thinking. And today, game theory is the primary theoretical structure that anchors and integrates the most recent developments in political theory.

Farquharson makes no direct mention of game theory, but he offered the "radical" idea that voting procedures are strategic environments and that the outcomes they produce can be ascertained only by evaluating how voters respond to the anticipated actions of other voters, where all responses are mediated by the prevailing electoral institution. Farquharson, then, anticipated not only the development of ideas such as subgame perfection, and the necessity for understanding how procedural details can be used by those who design and implement those procedures to manipulate outcomes, he also anticipated the hallmark of contemporary political theory -- that all political-economic action takes place in contexts in which people's fates are interdependent and in which they must make decisions with the understanding that others about them are oftentimes acting so as to anticipate their actions.

Unlike these other volumes, Key's does not offer any formal structure nor does he consider any game-theoretic ideas. But this last contribution by the leading scholar of American politics of his generation legitimized the application of rational choice models in a context, voting, that most researchers assumed was reserved for psycho-sociological analysis. Although few persons would dispute the assertion that election candidates, interest group leaders, and bureaucrats acted in their

individual self-interest, such an hypothesis seemed more difficult to sustain for mass political action. Individual perceptions were blurred, motivations unclear, and habit and socialization seemed the principle explanatory constructs. Key challenged such views and the subsidiary assertion that the rational choice paradigm was too crude a tool with which to approach the study of mass political behavior, and he argued that this paradigm must, of necessity, become the centerpiece of all political analysis.

A series of seminal papers followed the publication of these volumes and inaugurated more mathematically rigorous developments. Gibbard (1973) and Satterthwaite (1975) demonstrated that no democratic institution is immune from manipulation, and they thereby placed the study of strategic maneuver and institutions at the heart of formal political analysis; Kramer's (1972) analysis of the influence of parliamentary procedures established the ways in which such procedures may or may not induce stability in political outcomes, while simultaneously providing a set of results about voting that today are the foundation of models of legislative process; Plott (1966) and Davis and Hinich's (1966) extensions of Black and Downs' analyses of committees and elections offered a representation of political preferences that paralleled in importance the indifference curve representation of consumer preferences in economics, and demonstrated the fragility of stability in even the simplest of political processes -- 2-candidate elections and majority rule committees; building on Plott and Davis and Hinich's construction, McKelvey (1976, 1979) and Schofield (1978) demonstrated the extent of instability under majority rule and the opportunities to influence outcomes via manipulation of procedural details and they thus established the extent of the inescapable instability of voting processes; Schwartz (1977) clarified the intimate connection between opportunities for vote trading in legislatures and the Condorcet paradox and thereby answered the question as to the efficiency of vote trading; and, building on Farquharson's insights, innumerable papers on incentive compatibility and manipulability made us sensitive to the possibilities (or impossibility) of designing political-economic institutions that realize normative ends (cf. Clarke 1971; Groves and Ledyard 1975; Maskin 1977).

To this list we should also append those contributions that did not offer any startling theoretical conclusion, but showed instead how the paradigm and its associated analytic perspectives can be applied to politics. We have in mind here Niskanen's (1971) analysis of bureaucracy that paralleled Downs' treatment of political parties and candidates, Mayhew (1974) and Fiorina's (1977) analyses of the members of the U.S. Congress in the same terms -- as goal directed utility maximizers -- Popkin's (1979) analysis of peasants in Viet Nam, and Schelling's (1960) seminal discussion of the application of game theory to the study of deception and deterrence.

From this view, then, we have the impression of impressive advances. Our journals are replete with game theoretic models of elections, committees, and war and deterrence; and press editors

eagerly pursue half-completed manuscripts by practitioners in the field. But a more sanguine view argues that our understanding of politics has improved only slightly, that much of this research, like a giant in-grown toenail, is wholly literature-driven and directed at substantively inconsequential matters, and that success at finding publication outlets owes more to an intoxication with notation than to substantive significance. With considerable justification, people have wearied of a field that seems preoccupied with technical results about majority rule instability and sufficient conditions for the existence of Condorcet winners. They have become intolerant of research monographs that toil away through a seemingly endless array of notation in order to establish that a particular conjunction of strategies constitutes an esoteric type of equilibrium to a game that models a 3-person legislature or a 2-country international system. They have learned to circumvent papers whose abstracts begin with "we explore a model ..." or "we show that an equilibrium exists under which ..." They despair because, although much is known about simple forms of majority rule, very little is known about those rules and procedures that actually characterize political institutions. And they find insufferable the hubris of colleagues whose reputations are based solely on a familiarity with the latest game-theoretic technology, and for whom empirical research consists of reading the latest issues of *JET*, the *APSR*, *Social Choice and Welfare*, the *AJPS*, and *Econometrica*.

We should not dismiss such criticism out of hand. Contemporary political theory identifies a wide range of circumstances under which majority rule yields equilibrium and disequilibrium, but can we argue that simple majority rule characterizes anything more than a tiny proportion of political institutions? Countless articles have been written about the infinitely repeated Prisoners' Dilemma, but can we identify any circumstance in which identically the same game between the same people repeats itself even twice? We see the "new institutionalism" heralded as though it were equivalent to the discovery of the atom, but are these ideas new only because we have accepted a straw-man characterization of political science. We have witnessed the skill with which our colleagues manipulate equations pertaining to Bayesian beliefs and sequential equilibria, but what is the technology for measuring such beliefs and how do we contend with the experimental evidence that suggests that the Bayesian model cannot characterize subjective probability? And we have seen the impressive growth of models of legislative processes, but has the *as if* principle been stretched too far by special characterizations of issues and preferences and by highly restrictive assumptions about information and institutional structure?

Moving into the inner recesses of the most advanced papers on political theory is oftentimes equally discouraging. Paper abstracts lead us to believe that advances are being made at a rapid pace, but the essays themselves often reveal a different story. To render matters well-defined, *ad hoc* extensive or strategic forms, which everyone is presumed to see with perfect clarity, constitute the

core formulation. We see legislatures consisting of one or three members, models of intergenerational transfers in which all of society dies after two periods, models of elections in which the victorious candidate is dictator over all social policy, and analyses of international affairs in which country A but not B can make the first threat. To render the analysis tractable and consistent with the dictates of contemporary game theory, common knowledge applies to things we cannot ourselves determine, and utility functions are separable, strictly concave, and twice differentiable.

So what is it, then, that has been accomplished this past forty years? Is our understanding of politics deeper, or is it merely our understanding of our colleagues' papers, along with our ability to manipulate notation and string together abstract definitions, that has grown? This essay will argue that both views are essentially correct -- that although our understanding of politics (and of economics) remains in such a primitive state that we cannot ignore the practical necessity for considering other, less formal modes of inquiry, something has been accomplished. In particular, we argue that the current paths of development of political theory -- the discovery of First Principles -- are identical to those set forth by arguably the most successful set of "political engineers" in history -- the Founders of the American republic. However, we also argue that many of the shortcomings of contemporary theory derive from a failure to appreciate something that was apparent to those Founders; namely, the distinction between science and engineering and the proper role of empirical and experimental analysis.

1. The Spatial Election Model

In the space of a single essay it is, of course, impossible to review critically the vast literature that constitutes "contemporary political theory." However, regardless of our starting point, and regardless of our demarkation of "important contributions," it is evident that of all the asserted accomplishments of that theory, none is more important than that it has led to a reintegration of politics and economics under a common paradigm and deductive structure.

Both the evidence and source of this integration is generally taken to be the increasingly widespread acceptance of Key's argument that the rationality hypothesis has broad if not universal application in politics. Indeed, those scholars who critique this enterprise as either too limited in its application or too narrow in its normative implications focus typically on the meaning and generality of the assumption of self-interested action (see, for example, the essays in Monroe 1991). However, the mere acceptance of this postulate does not account for theoretical developments. Indeed, we can easily imagine a scenario in which its acceptance leads to little more than a reformulation of the behavioralist agenda, with political scientists focusing their attention on the sociological determinants of tastes and perceptions.

The second ingredient of this integration -- supplied by each of the seminal volumes in the field -- is a concern with political institutions. If economics and politics are to be understood in the same terms, it is imperative that we incorporate into our research agendas the fact that these disciplines, if they differ at all, differ only to the extent that the study of politics and economics is little more than a preoccupation with the performance of different institutions.

The focus of economics, of course, is the market and the ways in which governmental decisions impact on it. For political scientists, the focus is elections, legislative bodies, and, in the case of international politics, coordination in anarchic systems. This distinction is not sharp, as when taxation and trade policy are the subject of inquiry, but to be certain that we are studying matters so as to allow for interdisciplinary research, the institutional context of that research must be explicit. That is, studying the relative performance of alternative institutions of social choice and determining how they interact is the primary task of those who seek to develop an wholly integrated, scientifically correct field of political economy.

The Representation of Preferences: It is, however, too self-congratulatory to argue that the previously cited research holds a monopoly on appreciating the roles of self-interest and institutions. Morgenthau's (1948) text on international politics, Hoag and Hallett's (1926) study of voting procedures, or Bentley's (1935) analysis of interest groups should convince us that the perspectives of Franklin, Madison, and their contemporaries had not somehow been banished from the landscape, only to be resurrected in 1957 or 1962. And with respect to formalism, we should recall that sociology was perhaps even more mathematical than political science, and that a number of sub-fields in psychology were at least as sensitive to the canons scientific research. And although acceptance of the rationality postulate necessarily leads to game theory, merely studying alternative institutions with an plethora of game-theoretic formulations can only yield a dizzying selection of models in which synthesis is not assured. Even though game theory provides a unifying theoretical base, it alone cannot produce substantively meaningful integrative theory, because of necessity it remains little more than a set of mathematically abstract ideas (outcome sets, actions, topological trees, strategies) held together by *ad hoc* notions of equilibrium.

In addition to abiding by the rationality paradigm, exhibiting a concern with institutions, and employing game theoretic tools, what was required for the development of true theory was a unifying representation of preferences that allows for comparisons of the relative performance of those institutions and the determination of how they evolve and interact. And in this respect the key contribution is the spatial preference structure first described by Black and Newing (1951) and formalized by Plott and Davis and Hinich in terms of convex preference sets and Euclidean loss functions.

To illustrate this idea's importance, consider the early research on Condorcet's paradox. Without applying a particular topology to outcomes and preferences, assessing the importance of that paradox relied on a simple counting of possibilities under empirically vacuous null assumptions of equi-probability. On the other hand, once provided with a spatial topology, Plott, Davis and Hinich, and Sloss (1971) could convince us of the fragility of Condorcet winners and McKelvey and Schofield could establish the potential severity of cycles in democratic systems. Specifically, only by accepting the spatial representation can we see that Condorcet winners are rare and that top cycle sets can readily encompass the entire set of feasible outcomes.

More generally, the spatial idea unifies the treatment of the primary actors in markets and elections -- consumers and voters -- in such as way that they can be viewed as the same individuals operating in different institutional contexts. At the heart of economic theory is the idea that if people's preferences over alternative states of the world satisfy only two assumptions (transitivity and completeness), then we can begin to describe abstractly the basic activities of barter and production. Moreover, if we add a few technical assumptions about preference and outcomes (e.g., continuity), then we can represent preferences by indifference curves and we can apply the tools of calculus and algebra to characterize barter and production in compact and wholly general ways that allow for the formulation of that most important idea in social science - the characterization of efficient markets.

So powerful are these ideas about preference that their initial acceptance in economics largely accounts for the unfortunate separation of political economy into two disciplines. However, the spatial model allows us to formally reconnect economic and political theory. The mechanism whereby this "reconnection" occurs derives from the fact that spatial preferences can be derived from "economic preferences." Briefly, if we imagine a two-good model in which both goods are to be publicly provided, then Black's notion of a single peaked preference corresponds to the preference of an individual over a budget constraint that models a government's feasible spending policies with respect to those two goods. By extension, Plott and Davis and Hinich's spatial preference representation generalizes Black's idea to preferences over a budget simplex involving n goods or n-1 goods plus a tax rate that determines the government's budget and the consumer-voter's ability to purchase other commodities in the market (c.f, Ordeshook 1986; Coughlin and Hinich 1984). Thus, the idea of a spatial preference allows us to analyze processes in which people can simultaneously be consumers and voters (c.f., Klevorick and Kramer 1973; Meltzer and Richard 1981; Romer and Rosenthal 1979), and it thereby moves us closer to the goal of integrating the analysis of political and economic institutions.

The Median Voter Theorem: Aside from Condorcet's paradox, perhaps no result captured as much attention as Black's *Median Voter Theorem*, especially after Downs breathed substantive life into it. The theorem, which illuminates the strong centralizing tendency of simple majority rule electoral institutions, is perhaps intuitive and obvious -- especially to politicians whose livelihoods depend on understanding the basic forces of the political system with which they must deal. However, people have been designing and overturning political institutions for millennia, and we should not be surprised to find that a great deal of what we learn about them through game theory, axiomatic analysis, or the application of new empirical methodologies merely reconfirms or restates ideas that are otherwise obvious or well understood. The fundamental contribution of theoretical inquiry is not always the production of the counter-intuitive result -- rather, it is the expanded ability to relate seemingly diverse phenomena via a unitary deductive structure.

The median voter theorem was the first small step towards understanding a variety of phenomena using such a structure. First, in exploring this idea's generality, we establish a venue for bringing mathematical argument to politics, for injecting game-theoretic reasoning into our discourse in a serious way, and for developing a standard whereby we can descriptively and normatively evaluate the performance of alternative political institutions. Second, that model plays the same role in political theory as does the neoclassical model of markets. Although few empirical examples of markets match the mathematics that introductory economics texts describe, the neoclassical model of perfectly competitive markets has served as a focus for most of the theoretical advances in economics this past forty or so years. Treatment of externalities and public goods, the initial development of the rational expectations hypothesis, and the incorporation of game-theoretic reasoning, for example, all occurred under the impetus of generalizing and extending that model.

Following a similar path of development, early variants of election models abstracted greatly from reality with assumptions about perfectly informed voters and candidates, 2-candidate competition, and perfect spatial mobility. But political scientists have extended that model so that today it accommodates a diverse array of considerations such as multidimensional issue spaces, voters who are imperfectly informed about candidates, candidates who manipulate the uncertainty of their platforms, and voters who choose retrospectively rather than prospectively. Although most spatial analyses focus on a 2-candidate scenario, the spatial model has been used to show how political parties can protect themselves from third-party entry either by nominating candidates that deviate from the median preference (Brams and Straffin 1982; Palfrey 1984, Shepsle and Cohen 1990) or, appealing to some recent research that has formalized Duverger's (1954) hypothesis that simple majority rule fosters the development of a 2-party system (Cox 1987b, Palfrey 1989; Fedderson, Sened, and Wright 1990), by manipulating election procedures so that they more closely match simple majority rule (Bartholdi *et*

al 1990). And paralleling the economist's research into the sources of market failure and the development of the idea of rational expectations, we know that the median voter theorem is robust to some weakening of the assumption of complete information. Thus, we can assert with greater confidence than our intellectual predecessors that democratic institutions do not necessarily operate imperfectly in less than ideal circumstances (McKelvey and Ordeshook 1985, Austen-Smith and Banks 1988, Banks 1990, Ledyard 1989).

We learned early, of course, that the median voter theorem is not robust to the dimensionality of the policy space, and this fact generated nearly as much turmoil as constructive research. Political theorists became mesmerized by the fact that even the slightest perturbation in preferences away from perfect symmetry destroyed the existence of a Condorcet winner, and McKelvey's (1976, 1979) and Schofield's (1978) results convinced some researchers of the inherent unpredictability of political outcomes (but see Coleman and Ferejohn 1976). Indeed, at least one prominent contributor to contemporary political theory came perilously close to asserting that the instability of majority rule precluded a science of politics (Riker 1980).

At approximately the same time, however, we saw the emergence of alternative or more general hypotheses about equilibria and candidate platforms, such as the uncovered set (Miller 1980). Thus, we now know that if people vote deterministically, the domain of undominated spatial strategies is limited to a subset of the policy space with an identifiable structure that can be viewed as a generalization of a multi-dimensional median, and that even without a multidimensional median, candidates will not wander far from the "center" of the policy space (McKelvey and Ordeshook 1976; McKelvey 1986). Moreover, although voting cycles may be unavoidable, specific institutional structures and election processes can greatly limit the set of outcomes that can be realized (c.f., Kramer 1977).

2. Committees

Mass elections are not the only venue for applying the spatial preference structure. Indeed, Black and Newing's early monograph, as well as Black's subsequent presentation of the median voter theorem assume that the same model of preferences applies to committees as to electorates, and thus both volumes are seminal with respect to the contemporary study of legislatures, committees, and the integration of models of political institutions.

Issue-by-Issue Voting: Of the several extensions of Black and Newing, perhaps none is more important that Kramer's (1972) analysis of issue-by-issue voting. Begun initially as a search for variants of majority rule that would guarantees stability in an otherwise unstable environment, Kramer incorporated Farquharson's perspectives and formalized Black and Newing's spatial analysis

of committees so as to offer a set of theorems about sophisticated voting showing that the existence of a stable point under issue-by-issue voting depends critically on the assumption that preferences on the issues are separable. Although oftentimes ignored or misrepresented (but see Denzau and Mackey 1981, Enelow and Hinich 1983, and Epple and Kadane 1990 for a correct interpretation), this result is profoundly important. Specifically, owing to the evident restrictiveness of the separability assumption, we can infer that procedural details such as dividing the question or assigning substantive jurisdictions to subcommittee need not resolve the instabilities inherent in simple majority rule processes unless voters are naive or stupid.

This is not to say, however, that Kramer's analysis establishes the irrelevance of institutions and committee procedures, because it is also true that issue-by-issue voting occasions a stable point with strategically sophisticated voters in the special case of separable preferences. Thus, Kramer enriches our understanding of institutions and procedures by showing that their performance depends not only on their character, but also on the structure of preferences and the sophistication of decision makers. That is, we cannot appeal, as early 20th century institutionalist were want to do, to mere descriptions of institutions, however carefully crafted, to infer final outcomes; nor can we look, as naive behavioralists, only at the structure of preferences. Instead, we must look at all factors simultaneously before definitive conclusions can be uttered, and in this way, we have moved closer to achieving a truly *causal* explanation of political outcomes.

More generally, by refocusing attention on procedural details, we have not only gained a deeper appreciation of the role of those details in facilitating stability (Shepsle 1979; Shepsle and Weingast 1981; Riker 1983, Hammond and Miller 1987, 1989), but we have also begun to understand how institutions mediate the perceptions and beliefs that the behavioralist seeks to measure. At the same time, we have also learned how to identify circumstances under which institutional structures make little difference in the final determination of outcomes or least to appreciate the limits of their influence on outcomes. We now know, for example, that final outcomes in 2-candidate elections are constrained to the same subset of the policy space as are the outcomes that are predicted (via the V-solution, bargaining set, or competitive solution) to arise in simple majority rule committees (McKelvey 1986).

This fact is profoundly important for democratic theory. Specifically, we are no longer required to rationalize the use of elections solely on the basis of simple majoritarian principles or vague appeals to legitimacy. In addition, we now have a well founded theoretical basis for viewing 2-party, mass elections as a reasonable substitute for the ideal of democracy, the "New England Town Meeting," whenever the number of potential participants in such a meeting is too great.

Agendas: Aside from establishing some results about a particular procedural detail, Black and Newing showed how the spatial model can be applied usefully to a wide range of political institutions, and the research that followed contributed to a clearer understanding of the influences of various rules and institutional structures in legislatures (Cain 1978; Denzau and Mackay 1983; Shepsle and Weingast 1984), of the historical trends in the ideological basis of American politics (Poole and Rosenthal 1985, 1991), and of coalition formation in parliaments (Rosenthal 1970; Ordeshook and Winer 1980).

But in addition to learning that "institutions matter," we also gained a deeper appreciation for how specific procedural details might be manipulated to influence outcomes. Research on voting agendas provides the best example. Farguharson's familiar discussion of the example of Pliney the Younger reveals that students of politics have long been acquainted with the opportunity to manipulate outcomes by manipulating the ways in which voting and majority rule are implemented in committees. And Levine and Plott's (1977, 1978) study of a Southern California flying club also convinced us in contemporary terms of the importance of agendas.

The particular difficulty with such examples, however, matches that of determining the consequences of electoral disequilibrium Although we know that people can manipulate outcomes by manipulating agendas, this fact alone does not allow us to measure an agenda setter's power. But as with election models, the spatial model provides the structure we require to evaluate that power. The seminal essay here is McKelvey's (1976, 1979) proof that if there is no Condorcet winner, cycles encompass the entire issue space. Although this result has mistakenly been interpreted (not by McKelvey) to mean that an agenda setter's power is absolute and that majority rule processes in general are chaotic, McKelvey's analysis inaugurated a research agenda that sought to describe the influence of agendas and the potential power of a setter. The critical contribution in this instance is Miller's (1980) formulation of the uncovered set and his proof that, with strategic voters, amendment agendas cannot produce outcomes that are covered by any outcome on the agenda. Shepsle and Weingast's (1984) subsequent extension of Miller's analysis to spatial preferences, then, moves us closer to completing a research agenda that parallels the one set for 2-candidate elections -- determining the range of outcomes that can arise in committees that use some type of agenda to give order and coherence to their deliberations (see also Ferejohn, McKelvey, and Packel 1984 for a different institutional analysis that nevertheless yields an equivalent conclusion -- namely that even if cycles are ubiquitous, final outcomes have a limited domain).

A number of subsidiary results followed quickly from this research. In particular, Ordeshook and Schwartz (1987) noted that, except for those instances in which only three motions (including the status quo) are considered, Congressional agendas do not correspond necessarily to amendment agendas and that an agenda setter's power is increased considerably if we allow him the full range of

agendas that might be formed. On the other hand, Austen-Smith (1987) modeled a particular endogenous agenda formation process and gave us reasons for supposing that members of Congress can construct amendment agendas so as to secure the outcomes they want while simultaneously voting sincerely, and Banks (1989) shows how earlier results can be extended to one type of agenda, the 2-stage agenda, that can arise in Congress.

Admittedly, though, this research has two weaknesses. First, it assumes complete information on the part of all participants, whereas Ordeshook and Palfrey (1988) give examples of how incomplete information can upset even the most basic conclusions -- namely, that sophistication and binary agendas are sufficient to produce Condorcet winners as final outcomes -- and how events such as non-binding straw polls that precede the implementation of an agenda can influence outcomes. As with previous research, however, Ordeshook and Palfrey present only a possibility result by way of example, whereas Jung (1989) shows how these conclusions about the impact of informational asymmetries -- especially those concerning the ability of agendas to produce Condorcet winners -- must be modified if we once again supply preferences and outcomes with a simple spatial structure. Specifically, if it is common knowledge that all voters have single-peaked preferences, then the Condorcet winner once again reappears as the final outcome of any amendment agenda.

More problematical is the issue of endogenous agendas. Although Banks and Gasmi (1987) provide a partial handle on this matter in a spatial context, we can also infer from their analysis that studying the processes whereby agendas arise in "large" committees (i.e., $n > 3$) will pose severe analytic difficulties. The general difficulty, of course, is that the processes whereby agendas are formed in committees are not well-defined in the sense that those processes do not correspond to any self-evident game form. Thus, models like Austen-Smith's (1987) are driven more by the demands of tractable analysis than they are by the substantive context of choice. At present, then, there is no successful model of endogenous agenda formation that allows for any meaningful interpretation of events.

The particular problem is that to apply game theory usefully so that we can derive more than the most general conclusions (e.g., possibility or impossibility results) requires that we provide a game-form that specifies precisely the identity of decision makers, the sequence with which they make decisions, and the information at their disposal when they act. And although agenda voting, like simple descriptions of elections, lends itself readily to the construction of such a form, the processes whereby agendas are formed is far less structured and, thereby, less amenable to unambiguous game-theoretic analysis. Thus, before we can achieve meaningful results about endogenous agenda processes, greater attention must be paid to the real-world processes that characterize their construction.

Despite these limitations, we have learned a great deal about agendas, including that they cannot lead anywhere if voters are sophisticated, that final outcomes are sensitive to information structure, and, perhaps most importantly, that even with sophisticated voters, agenda voting confers considerable power on those who design them or who otherwise control their construction. Thus, although many things must be measured and understood before we can predict outcomes, we have learned the factors that mediate the influence of procedures as well as how those factors combine. And, in addition, we have learned and can now formally document something that was self-evident to the Founders of the American republic -- namely, that no only does the broad character of institutions matter, but so do procedural details, and that gaining control of those details is often the critical political event.

3. Game Theory

The literature on 2-candidate elections and agendas reveals not only the extent of game theory's impact in political science, but it reveals also the increasing sophistication with which political scientists make use of that theory. With the exceptions of spatial election models, which relied on zero-sum games, Riker's development of the *size principle*, and Kramer's analysis of issue-by-issue voting, the application of game theory throughout the 1960's and most of the 70's consisted for the most part of simple stories about and adaptations of 2X2 games -- if a political process did not correspond to a Prisoners' Dilemma, then it must have been a Game of Chicken or a Battle of the Sexes, and if a coalitional process could not be analyzed using some descriptively meaningless "power index," then a simple majority game with transferable utility was assumed to apply. These analyses illustrated and illuminated the forces that are common to a wide range of political phenomena, but they are little more than examples to be appended to more serious empirical research.

The more sophisticated applications of game theory began in two areas. And unsurprisingly, one of these areas pertained to spatial games while the other concerned the analysis of voting procedures. In the first area, McKelvey, Ordeshook, and Winer (1978) observed that, although much of the profession was transfixed by the generic instability of majority rule, it had been evident to von Neumann and Morgenstern (1945) that such instability characterized nearly all cooperative processes. But rather than conclude that generic instability implied "chaos," they instead provided a solution hypothesis -- the V-set -- to deal with such matters. Unfortunately, the application of these ideas to spatial majority rule games revealed their general inadequacy -- they either made no prediction at all or they made predictions that were patently silly. Consequently, McKelvey, Ordeshook, and Winer developed the Competitive Solution, and applied it to the study of parliamentary coalitions and to Congressional vote trading (McKelvey and Ordeshook 1980, Ordeshook and Winer 1980). At about

the same time, Schofield (1980, 1982) developed various extensions of solution theory that also treated parliamentary coalitions. Neither the competitive solution nor Schofield's extensions have proven to be wholly satisfactory, and research in this area languished as people became concerned with the endogenous sources of cooperation and with the "Nash agenda" for studying cooperation. Nevertheless, research has continued, even though a wholly satisfactory cooperative solution hypothesis has not yet appeared (cf. Bennett and Zamir 1988; Sharkey 1990, Greenberg 1991).

The second venue for the more sophisticated application of game theory concerned agendas, which began with McKelvey and Niemi's (1978) and Moulin's (1979) demonstration of the correspondence between the analysis of sophisticated voting and subgame perfection and their subsidiary results about agenda outcomes. These essays, of course, are the immediate predecessors of the research on agendas reviewed in the previous section.

Today the applications of game theory to politics encompass nearly all areas and all forms of analysis. In addition to the applications to elections cited previously, the technology of incomplete information games has allowed us to extend and generalize Schelling's (1960) treatment of threats and deception and has thereby revolutionized the analysis of strategic deterrence in international affairs (c.f., O'Neill 1989a,b, Powell 1990, Bueno de Mesquita and Lalman 1991, Kilgore and Zagare 1991). Similarly, recursive games have been applied to formulate hypotheses about the sources of seniority rule in Congress (McKelvey and Riesmann 1991) and the nature of Congressional redistributive politics (Baron and Ferejohn 1989). The concept of Evolutionary Stable Strategies (Maynard Smith 1982) is slowly creeping into our research, and promises to offer a better understanding of equilibrium selection and the evolution of institutions. And applications of repeated games have advanced from Taylor's (1976, 1987) and Axelrod's (1984) treatments of the repeated Prisoners' Dilemma to more general analyses of cooperation in politics (Bendor and Mookherjee 1987; Calvert 1989; Bianco and Bates 1990). Indeed, political scientists have been nearly as quick as their colleagues in economics to realize that contemporary developments in game theory allow them to theorize about the fundamental question of institutions -- namely, how those institutions emerge and are maintained in otherwise anarchic systems.

Game theoretic reasoning has also escaped the boundaries set by the topics of war, elections, and committees. The application of the repeated Prisoners' Dilemma is no longer relegated to simple stories about the sources of cooperation, but are used instead to model, for example, the endogenous development of institutions to accommodate the provision of public goods and the management of common property resources (Ostrom 1990, Ostrom and Walker 1991), and in at least one instance games of incomplete information have been applied to study the consequences of alternative forms and levels of punishment on criminal behavior (Cox 1991). Thus, in addition to facilitating the analysis of existing institutions, we are now beginning to see game theory applied to their design.

Attention to the "Nash agenda" with respect to cooperation has also led to some initial success at addressing a classic problem of political theory -- the sources of cooperation and stability in anarchic international affairs. We have previously noted Morgenthau's (1948) general acceptance of the rationality postulate. But without the tools of contemporary theory -- game theory in particular -- students of international politics had to contend with a confusing array of concepts and definitions such as those of "alliance," "collective security," "power," "stability," and "balance of power," and they seemed unable to achieve the fundamental objective of international political theory -- determining the sources of stability in anarchic systems and the viability of alternative prescriptions for foreign policy.

Admittedly, with the exception of recent research in deterrence and the applications of various 2X2 games, the development of international relations theory has lagged behind the study of elections and committees. This lag is due, doubtlessly, to the fact that structural character of various committees and election processes is well defined -- or at least better defined than that of international politics. (Indeed, as we have already noted, when dealing with ill-defined committee processes such as endogenous agenda formation, our advances are no more striking than those we attribute to international relations theorists.) Wagner (1986), however, pointed the way towards the application of recursive games, and Niou and Ordeshook (1990, 1991a,b), building on Wagner's framework, have established that a balance of power (corresponding to an equilibrium of stationary strategies) and collective security (corresponding to an equilibrium of simple punishment strategies) can simultaneously exist in an otherwise anarchic system, and that alliances the correspond to limited collective security arrangements can survive as well.

Game theory, then, is at the core of contemporary political theory in nearly every subfield, and those who wish to remain on the frontiers of research must grasp new concepts and results as they appear in specialized journals. It is also interesting to note that as the sophistication of applications increases, reliance on the spatial preference structure diminishes. None of the research on deterrence or on the sources of stability in international systems is impaired by the failure to adapt to such a structure. The explanation, we suspect, is that although that structure was necessary so that political scientists could escape from simplistic 2X2 game-theoretic stories, the present level of sophistication allows for more varied assumptions about preference and for models that better treat less formal processes.

But it is also important to ask: Are the components of this research anything more than a forgettable series of essays analyzing models that the demands of tractability render irrelevant to the real world? This question has two answers. First, in addition to facilitating the integration of political and economic models and the study of institutions, contemporary theory offers a fresh perspective on the study of political actors. Outside of the traditions of formal analysis, different

types of political actors, as well as the institutions in which they operate, are frequently treated in isolation and without full regard for the strategic complexity of their environments. If other decision makers are relevant to the actions of those under investigation, then their motives are largely treated as exogenous to the analysis and their actions are treated as contingent in some simple way on the actions of the class of decision makers under investigation. Thus, voters are studied in contexts in which the strategic responses of candidates and parties are ignored in favor of understanding the sociological and psychological determinants of attitudes and the influence of attitudes on party or candidate choice. Legislators, although assumed to be primarily interested in maximizing their chances of reelection, are assumed to act in an environment in which voters and interest groups either approve or disapprove of their actions or in which bureaucrats either shirk or respond to congressional initiatives. Similarly, in evaluating, say, a particular election reform, it is not uncommon to see past voting data "plugged into" an alternative procedure for tabulating votes in order to "see what would have happened had this procedure been in effect," despite the fact that that behavior as well as the behavior of other relevant decision makers (e.g., candidates) might have been influenced by a change in procedures.

In contrast, owing to its reliance on game theory, contemporary theory requires a full strategic view in which, given the institutional context of choice, candidates and voters choose best responses to each others' strategies, and in which the actions of constituents, legislators, and government bureaucrats are simultaneously viewed as maximizing their objectives under the assumption that all other actors are seeking to do the same thing.

4. Science versus Engineering

However, critics of this theory can point to the fact that few if any persons pursue this general equilibrium research agenda -- presumably because constructing models in which all relevant decision makers act strategically yields an unwieldy analysis and does not engender generalizable analytic results. The literature does not offer a spatial elections model, for example, that simultaneously accommodates the strategic decisions of voters, candidates, party leaders, and campaign contributors, operating under complex but realistic elections rules, while simultaneously taking into account the fact that the election will be followed by a period of legislative-executive interaction, which will be followed by another election, and so on.

However, continued calls for greater scope, generality, and relaxation of assumptions ignores the important difference between the **scientific** enterprise of uncovering fundamental forces that operate universally and the **engineering** enterprise of using the lessons of theory to model actual or proposed political-economic institutions and processes.

To illustrate what we mean by this distinction, consider the ostensible contributions of spatial models of elections. At a minimum, those contributions include: (1) demonstrating the profound difference between uni- and multi-dimensional election contests (Downs versus Plott and Davis and Hinich); (2) demonstrating the possibility of placing bounds on the strategies that candidates adopt in multidimensional 2-candidate contests (McKelvey and Ordeshook 1976; McKelvey 1986); (3) establishing the possibility of complete information revelation in incomplete information elections (McKelvey and Ordeshook 1985, Ledyard 1989); (4) developing statistical procedures for measuring spatial preferences and candidate locations, and for determining how voters map their concerns into a general policy space (Enelow and Hinich 1984); (5) ascertaining the theoretical basis of Duverger's hypothesis (Cox 1987b; Palfrey 1989; Fedderson, Sened, and Wright 1990); (6) showing how the spatial analysis of 2-candidate elections can be extended to multi-candidate contests and to the analysis of alternative voting institutions (Cox 1987a, 1990; Greenberg and Weber 1985); and (7) showing how probabilistic voting can "smooth" candidate responses and generate stability (Hinich, Ledyard, and Ordeshook 19xx; Coughlin 1984); and (8) showing that even if all of the other assumptions of the median voter theorem are satisfied, probabilistic voting can yield outcomes other than the median (Hinich 1977).

By themselves, however, these conclusions do not constitute a complete model of any election, nor do they appear to provide much useful practical advice about, say, democratic reform in Eastern Europe or elsewhere. To see this, consider the following problem: Under the authoritarian control of the KMT since 1949, Taiwan (the Republic of China) seeks to democratize, which includes the forced retirement of the permanent mainland members of the Yuan elected in 1948. This commitment to reform occasions a series of design issues such as determining whether representatives should be elected in single or multiple member districts, and, if multiple member, whether districts should be large or small and whether a single non-transferable vote scheme should be implemented. A related issue concerns the selection of chief executive and choosing between a presidential direct election system and a parliamentary system. The specific concern here is ascertaining the consequences of alternative reforms for political stability, party factionalism, and relative KMT influence, as well as determining the impact of these alternatives on the relative political power of native Taiwanese versus mainlanders.

This is not an unimportant design issue, at least for the residents of Taiwan. However, merely being told about the centralizing tendency of two-candidate plurality rule elections or the fact that multi-dimensionality can occasion disequilibrium is unlikely to win many converts in Taiwan to a more theoretical perspective. We can say that multi-member districts induce party factionalism (Cox 1990) -- a fact already widely understood on the basis of Japan's experience -- or that bicameralism facilitates stability (Hammond and Miller 1990), but there are only a few attempts at understanding

the relationship between electoral and parliamentary processes (Rosenthal 1970, Austen Smith and Banks 1990), and we have made almost no progress at all in determining the implications for minority rights of alternative electoral systems except in the most abstract way. Indeed, despite thirty years of modelling efforts, we know relatively little about multi-party electoral competition (cf. Shepsle and Cohen 1990; Cox 1990; and Shepsle 1990 for a review) and competition in those electoral systems that more generally characterize reality.

The realization of the shortcomings of contemporary theory can produce at least two responses. On the one hand, we can continue on the path of incrementally exploring alternative models that are driven largely by the availability of new analytic technologies and the demands of tractability -- arguing forcefully with our more substantively oriented colleagues that the construction of theory is an incremental process that requires patience. Doubtlessly, in a hundred years or so we might have something more specific to report to the residents of Taiwan -- although there is a reasonably good chance that we will also only confront at that time a set of abstract theorems detailing the impossibility of stable political systems and the intractability of the concept of minority rights. Moreover, our current inability to "derive" effective economic reform policies for the faltering economies of Eastern Europe and the USSR using any part of the hundreds of essays appearing in *Econometrica* or *JET*, despite their advanced mathematical development, should diminish our confidence in the argument for patience.

On the other hand, we might try to respond to critics by constructing wholly integrated models that add as many features of reality as possible, including simultaneous assessments of elections and legislative actions along with a consideration of the macro-economic forces that impinge on the performance of elections and legislatures. The difficulty here, however, is that such models are unlikely to yield the sort of punchy analytic results that guarantee success in the ultimate objective of lengthening our vitae. And should such results be sought, the necessity for maintaining rigor and tractability would require compromises of Herculean proportions.

There is, however, a third response which derives from the fact that the construction of models informed by theory is not impossible. Specifically, existing theory tells us to be concerned about issue dimensionality, but it also tells us that it is unnecessary to presuppose that a simple equilibrium prevails before useful predictions can be uttered. We should also be aware of the possibility that voters and candidates will gather information about relevant parameters in indirect ways, and we should consider the fact that voters, politicians and interest groups will respond strategically to electoral procedures in such a way that parties may splinter or recombine in different ways. Naturally, in constructing any model of an existing or a proposed electoral system, we must take careful measurements of preferences and perceptions, with the understanding that the general

character of competition will be molded by the underlying structure of preferences and the ways in which voters combine substantive issues in order to produce a coherent spatial map for themselves.

Naturally, in fitting these pieces together, we cannot anticipate a model that will generate readily interpretable analytic results, and we may be able to determine outcomes only through such techniques as laboratory experimentation and computer simulation. However, just as the engineer does not attempt to derive theorems about bridge design or to establish equations in closed analytic form about the point at which fluid flowing through a pipe changes from laminar to turbulent flow, we should not anticipate theorems here.

Our argument is that political analysis ought to consist of two enterprises. First, it ought to be concerned with uncovering basic laws and forces. For the Founders of the American republic, those laws took the form of ideas about the permanence of faction, the necessity for a tripartite balance of forces, the inhibiting influence of a bicameral legislature, and the fact that institutions can regulate but cannot eliminate the forces of self-interest. To these ideas contemporary political theory adds the median voter theorem, results about stability and instability in complex issue arenas, Duverger's hypothesis about simple plurality rule, the possibility of rational expectations, the strategic character of vote trading in legislatures, theorems about the influence of agendas and other procedures on final outcomes, and a fuller understanding of the sources of cooperation.

In uncovering those basic laws, the political theorist should, of course, seek as much generality as possible. Gibbard (1973) and Satterthwaite's (1975) manipulability result, McKelvey and Schofield's (1988) research into the necessary and sufficient conditions for the existence of cores, and Miller's (1980) development of the uncovered set illustrate this enterprise. Alternatively, the theorist can attempt to formulate models that, although not general, reveal the relevance of factors hitherto ignored. Plott (1967) and Davis and Hinich's (1967) proof of the relevance of issue dimensionality, Kramer's (1972) analysis of issue-by-issue voting, McKelvey and Ordeshook's (1985) demonstration of the possibility of rational expectations in elections, Brams's (1976) cataloging of paradoxes in politics, and Wagner's (1986) demonstration of the possibility of stability in an otherwise anarchic international system are examples of this avenue of investigation.

The second enterprise -- political engineering -- is unfortunately only weakly, if at all, represented in contemporary political theory. That such an approach is required if political science is to be an applied science is made evident by the fact that even after four decades of research into social choice and voting institutions, practitioners in the field do not agree even among themselves about the ultimate implications of their research. Riker (1983) and Coleman and Ferejohn (1986), for example, disagree over whether contemporary social choice theory and, in particular, the "chaos" theorems of McKelvey and Schofield imply anything about the relative value and stability of populist versus liberal democracy. Similarly, McKelvey and Ordeshook (1984) and Shepsle and Weingast

(1984) disagree over whether the "new institutionalism" in fact offers anything new other than the proposition that institutions deflect majority preferences only to the extent that transaction costs are high.

Of course, it may be that our theory is too primitive to contribute to political engineering, but we should not shirk from such a role if we can augment our theories with the talents and insights of the substantive expert. And even though such activities do not produce theorems of the sort deemed necessary for publication in prestigious journals, a serious attempt to meld theory into being a practical science is far more valuable than deriving with skill that next cute theorem in the context of an otherwise silly model.

5. Empirical Research

Before we can fully evaluate the opportunities and requirements for political engineering, though, we should first evaluate the empirical applications of political theory. It is certainly true that although theoretical advances, both useful and not useful, continue to be made at a fast pace, direct empirical applications of theory proceed with considerably greater difficulty. There are far too few parallels, for example, to Sawyer and MacRae's (1962) analysis of cumulative voting in Illinois, Romer and Rosenthal's (1979) study of Oregon referenda, Rosenthal and Sen's (1973, 1977) empirical testing of alternative hypotheses about non-voting, Poole and Rosenthal's (1985, 1991) historical study of issue cleavages in the U.S. Congress, Enelow and Hinich's (1984) estimation of the correspondence between actionable issues and generalized policy spaces, McCubbins and Kiewiet's (1988) analysis of presidential vetoes; Cox's (1987c) study of political party development in England, Roberts' (1990) validation of the rational expectations hypothesis in politics (see also Forsythe et al 1991), Plott and Levine's (1977, 1978), Enelow's (1981) and Enelow and Koehler's (1980) examples of agenda manipulation, and Riker's (1983, 1984, 1988) applications of social choice theory to macro-historical data.

Each of these studies gives us confidence that the preoccupation of the theorist is something more than a pleasant hobby. However, the actual contribution of the majority of empirical research to theory is difficult to evaluate, because it is generally difficult to determine its purpose. We are amused, for example, by those essays that ostensibly test even today the hypothesis that voters are "rational." Is it possible that practitioners in the field are prepared to accept the hypothesis that the rationality paradigm is bunkum? Or consider those analyses that "test" models that are grossly false *a priori*. Although we might interpret such research as uncovering some empirical regularity, even if we learn that such a model "explains" some data up to an acceptable level of statistical accuracy, we would nevertheless be foolhardy to believe that the forces being described are invariant with the

assumptions that render the analysis tractable. We might argue that such findings suggest that our efforts are "headed in the right direction," but how do we contend with the fact that we can establish nearly any reasonable outcome as an equilibrium to a variety of models, provided only that those models are sufficiently complex?

To evaluate contemporary contributions and future directions, then, consider first the proposition that such research ought to have one of five objectives: (1) identifying or otherwise demonstrating the existence of empirical generalities that require explanation; (2) testing basic laws and premises; (3) demonstrating the universality of some law by way of empirical (historical) example; (4) estimating a model's parameter values; and (5) testing the design of political mechanisms.

Nearly all empirical research matches one of these categories, but a considerable portion of that research is only poorly designed to accomplished its objective. Nevertheless, examples of research that correspond to the first two categories can be found in a growing experimental literature. For example, Berl et al's (1976) and Fiorina and Plott's (1978) studies of the core establish this idea as more than a mere mathematical invention -- it is a solution hypothesis that is at least as robust to a variety of social-psychological variables as any other available idea. Augmenting the Congressional studies cited earlier, experimental work by McKelvey and Ordeshook (1984); Wilson (1986); Salant and Goodstein (1987); Herzberg and Wilson (1991); and Rapoport et al (1991) gives us confidence that theoretical research into the effects of institutions and procedures is not without substantive foundation. McKelvey and Ordeshook's (1985) incomplete information election experiments draw attention to broader definitions of equilibrium and to the notion of rational expectations. And Miller and Oppenheimer's (1982) and Eavey and Miller's (1984) research on fairness, which fits nicely in the first category of empirical research, serves as a warning about the existence of motivating factors other than a myopic concern with the maximization of wealth and suggests that people consider fairness in systematic ways.

Of course, political scientists will quibble over whether their empirical research matches one of the first three categories and whether experimental or field data is best suited for the purpose of hypothesis testing. This is due in part to the fact that the notion of a first principle is vague. Such a principle can refer to some "micro" idea about, say, how people process information or the validity of a specific equilibrium notion, or it can refer to a more "macro" idea, such as the median voter theorem or an hypothesis about the sources of governmental growth and inefficiency. However, regardless of how we interpret "first principles," little research match the fourth or fifth categories, which are the ones that correspond most closely to "political engineering." And in this context it is time to consider the approach to empirical "research" taken by those preeminent political engineers, the Founders of the American republic.

First, it is evident that they proceeded in much the same way as theorists operate today in that they "relied mainly on deducing descriptive and prescriptive truths ... from First Principles ... Historical and contemporary experience were considered not the sole source of truth but merely a quarry from which illustrations could be mined to clarify and lend persuasive force to the truths revealed by Right Reason from First Principles" (Ranney 1976:144). But as the debate between Schwartz (1989) and Cain and Jones (1989) suggests, the Founders were also experimentalists in that they understood that neither historical experience nor deductive logic could definitively answer all practical design issues.

Barring the tools of laboratory experimentation or econometrics, they struck upon an imaginative approach -- designing a political system that would not only be an experiment, but also an experiment capable of self-correction as seemed warranted by the data. Even though they understood fully the importance of institutions, they designed a constitution that was itself open-ended and capable of revision in light of experience. And rather than proscribe specific procedures for the branch of government they regarded as most important -- the Legislative -- they allowed that branch to choose its own rules of organization and voting.

Today, of course, far too little use is made of historical data as a source of new ideas and as a test of existing ones. Nevertheless, one of the most encouraging features of contemporary political theory is that it appears to be accommodating itself slowly to an engineering-experimental agenda. Ostrom and Walker's (1991) research on common pool resource issues, Isaac and Walker's (1988) study of public goods provision and group size, Boylan, et al's (1991) analysis of the influence of electoral mechanisms on government investment, and Plott's (1991) analysis of the influences of polls on election results all illustrate research that moves us closer to understanding the behavior of complex systems. Far more studies of this sort, however, are required before we can assert that the practical value of contemporary political theory has equalled or surpassed the level achieved in 1787.

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