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THE REINTEGRATION OF POLITICAL SCIENCE AND ECONOMICS AND THE PRESUMED IMPERIALISM OF ECONOMIC THEORY

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Abstract

No discipline can claim a greater impact on contemporary political theorizing than that of economics, whether that theorizing concerns the study of legislatures, elections, international affairs, or judicial processes. This essay questions, however, whether this impact is a form of "economic imperialism," or the logical development of two disciplines whose artificial separation in the first part of this century merely allowed the development and refinement of the rational choice paradigm, unencumbered by the necessity for considering all of reality. Indeed, applications to specific substantive political matters -- most notably collective and cooperative processes where game theory proves most relevant -- reveal the paradigm's incompleteness. These applications, however, illuminate the necessary theoretical extensions, which is no longer the sole domain of the economist.

The Reintegration of Political Science and Economics and The Presumed Imperialism of Economic Theory

In this century, the study of politics has been the beneficiary as well as the victim of many intellectual currents from other disciplines, most notably sociology, law, and psychology. But no discipline has had a greater impact than economics or can today challenge economics's preeminence as a sister discipline. This influence and parallelism are due, first, to the political scientist's and the economist's desire to understand the same general phenomena -- the allocation of scarce and valued resources by mechanisms of human creation. Second, unlike sociology or psychology, both disciplines are concerned with specific, readily identifiable mechanisms. The focus of economics is markets and the institutions that arise in response to general market forces, whereas elections, representative institutions, and decentralized systems such as those that describe international politics are what concern political science. Contributing importantly to the synergism between the two disciplines is the discovery by political scientists that the rational choice paradigm commonly associated with economics offers a compelling perspective with which to begin a deductive and rigorous study of the phenomena that concern them, and the discovery by economists that the "non-market" mechanisms studied by political scientists decisively effect the existence, structure, and performance of markets. Thus, the economist now recognizes that political institutions and processes must be understood in the same terms as markets, and political scientists have been made aware of the possibility of integrating their research with economic principles.

Despite the failure of scholars from earlier centuries to distinguish sharply between these two disciplines, the first half of our century witnessed a division and intellectual

specialization with both unfortunate and beneficial consequences. The division was unfortunate because of the self-evident fact that political and economic activity, however defined, cannot be separated in the description of any society. The hard-learned lesson of those we might label "political conservatives," for example, is that it is the state that establishes the context in which markets are permitted to operate, with the state standing ready at any time to upset any particular market outcome. People are not merely consumers and producers, they are also citizens in a variety of polities that are equipped not only to regulate markets, but also to expropriate directly the resources that markets allocate. Thus, it is impossible to predict market outcomes without also predicting the political responses that alternative outcomes engender. On the other hand, the hard-learned lesson of the left, and of Marxism in particular, is that whatever institutional structure is imposed for the state, the laws governing market forces cannot be abrogated -- the forces of supply and demand operate regardless of culture, socialization patterns, ideology, and political system.

But the separation into two distinct disciplines was not without its beneficial consequences. Most importantly, it permitted the economist to proceed unencumbered by the necessity for considering all of reality. Left to study the particularized forces of markets and the allocation of a specific numerarire, money, the requisite details of a paradigm were uncovered. Axioms of choice and preference, along with formal representations of preference and alternative choice contexts were developed. The separation thereby accomplished an abstraction that might not otherwise have occurred, and it permitted the economist to develop that most powerful of social theories-- classical microeconomics.

Because it is in the domain of microeconomic theory that the mathematical structure of the rational choice paradigm first appears, it thus seems appropriate to regard the adoption of this paradigm by political scientists as "economic imperialism." Of course, some persons might object to this supposition with the argument that we can discern this paradigm in the writings of political scientists such as Arthur Bentley, David Truman, Robert Dahl, Hans Morgenthau, Charles Lindbloom, and V.O. Key, and that despite academia's propensity to promulgate disciplinary boundaries within the bureaucratic structure of universities, the division between economists and political scientists was always more apparent than real. But the case for imperialism is supported by observing that Bentley and others did not incorporate the deductive rigor of economics, and that the beginnings of the interdisciplinary synergism that is most apparent to us today is commonly identified as the relatively short and remarkable period from 1957 to 1962--the period marking the publication of four seminal volumes: Duncan Black's (1958) **Theory of Committees and Elections**, Anthony Downs's (1957) **An Economic Theory of Democracy**, William H. Riker's (1962) **The Theory of Political Coalitions**, and James Buchanan and Gordon Tullock's (1962) **The Calculus of Consent** (although it is reasonable to argue that Kenneth Arrow's **Social Choice and Individual Values**, published in 1951, and Mancur Olson's **The Logic of Collective Action**, published in 1968, are members of the set).

Today the research stimulated by these volumes is published across the full gamut of journals that represent the mainstream of both disciplines, and few people can keep abreast of it and the attendant flow of articles, books, and working papers. In this flow the impact of the rational choice paradigm on political science is now fully apparent. By any accounting, an increasing percentage of essays in political science journals are

designed either to explicitly extend the paradigm of rational choice or are set in that paradigm's context. Further, the labels "formal political theorist" or "positive political theorist" are not applied to political scientists who simply use mathematics in their arguments; rather, these labels are reserved for those who specifically work within the paradigm.

It appears, then, that economics, by design or otherwise, is the preeminent imperialistic discipline of the social sciences, or at least of political science. But we should ask: Is this imperialism real? Did economics uniquely develop and refine the rational choice paradigm? And if we can detect any imperialism, does it extend beyond the mere exporting of this paradigm?

Answering such questions, and measuring the nature and extent of the influence of economics on the study of politics as well as the influences of political science on economics is important not only because we might like to contribute to the history of ideas, but also because such knowledge provides clues about the future of the two disciplines. Learning how political scientists and economists interact allows us to anticipate theoretical changes and, as part of the argument of this essay, it also permits us to anticipate the form of the eventual reintegration of two disciplines. Knowing where this integration is incomplete reveals the inadequacies of theoretical structure.

One theme of this essay is that if there is imperialism, it is of a rather simple sort in which the use of the 18th and 19th century rationalist paradigm of social theorists (political and economic) is once again serving its integrative function. The presumed imperialism of economics, then, does not take the form of the transportation of economic models and laws into political science, although some have tried this (with but modest success). Rather, it takes the form of the political scientist once again explicitly using a theoretical view that previously had unified the disciplines and that the economist has refined these past seventy five years, and modifying it to serve the substantive purposes that are of special concern to his discipline so that the study of political institutions can once again be integrated with the study of economic institutions. In fact, this imperialism takes the form of the economist realizing that his is not a special discipline that can be isolated and studied without some account of politics, and that the refinements and extensions of the paradigm offered by political scientists are an essential part of economic theory. In this essay, then, we will try to gain some perspective on these efforts and on the main theoretical currents currently coursing through both disciplines.

Our discussion, however, has a second theme, one that becomes more apparent as we examine the application of the paradigm to political phenomena. That theme is the argument that the theoretical apparatus of economics suffers from fundamental theoretical inadequacies. Moreover, those inadequacies are rendered most evident in the study of politics. Hence, the full development of the paradigm requires an attention to the substantive concerns of both disciplines. The inadequacies of special concern involve the treatment of strategic and cooperative action. Game theory, which is that part of the paradigm that concerns such actions, is only now, after languishing as a theoretical backwater of economics, being developed as in a theoretically satisfactory way. Later we show how political science contributes to this development owing to the special problems common to nearly all political processes.

1. The Early Resistance to Intellectual Invasion

Although its details are constantly questioned, criticized, defended, and reformulated,

the rational choice paradigm -- summarized by the assumption of methodological individualism and the hypothesis of individual action motivated by self-interest -- forms the thread uniting the two disciplines. Indeed, it is the adaptation of this paradigm to the study of political processes that is commonly cited as the essence of the presumed imperialism in political science of economic theory. As we note earlier, it is tempting to assert that this imperialism is illusionary, that Bentley, Dahl, Key and many others also abided by it, and that the economist's contribution has been merely to supply a mathematical formalization -- aided in no small part by non-economists such as von Neumann and Savage. Nevertheless, we cannot ignore the fact that the writings of Black, Downs, Riker, and Buchanan and Tullock were not received with broad acceptance in political science. Scholars who followed the behavioralist tradition and who gained their theoretical sustenance from disciplines such as psychology and sociology, for example, were at best skeptical about the paradigm's relevance in the belief that its definition of rationality was too restrictive, that its concept of self-interest assumed away motivations such as altruism, and that the hypothesis of methodological individualism precluded consideration of "group-oriented" ideas such as socialization, norms, and culture. Also, those who were concerned with the substance of public policy and foreign affairs saw the formalism associated with the paradigm -- the assumptions required to render a mathematical argument tractable -- as lethal impediments to any adequate understanding of their subject. Hence, instances of the paradigm's explicit use in political science were often isolated at professional meetings with panels devoted to "formal political theory," "mathematical models," or "public choice." Only infrequently in the 1960's or the early 1970's did we witness the participation of the paradigm's proponents on panels dealing with traditional topics such as legislative processes, elections, the presidency, international affairs, the courts, or the analysis of the formation of public policy.

How do we reconcile the fact, then, that although central practitioners of the trade implicitly used the paradigm, its explicit adaptation to the discipline was strongly resisted? The speculative answer to this question is two-fold. First, because those who followed in the intellectual footsteps of Downs et al emphasized deductive rigor -- even pure mathematics -- at the apparent expense of substantive content, many applications were viewed as mere mathematical manipulations. Even when such rigor was applied to the development of a consistent body of theory -- most notably, the various election models based on Downs's **An Economic Theory of Democracy** (referred to as "spatial models of elections") -- the results were unappreciated because the basic theory under development was deemed to have questionable legitimacy by those who abided by a different paradigm. But the use of pure mathematics alone cannot account for the political scientist's skepticism -- statistical methodologies gained broad acceptance, and often legitimated learning the mathematician's craft. Instead, the second part of our answer concerns the political scientist's lack of understanding about the role of mathematics in scientific explanation, and about the nature of the development of general theory. Our answer also concerns the natural and healthy reluctance to abandon one approach in favor of another until the usurper's relative value is evident.

We ought to keep in mind that the paradigm's entry into the discipline was not preceded by any readily apparent insight that lit the way for all to see. No understanding of a specific empirical phenomenon compelling others to follow. Neither a great discovery such as the identification of DNA nor a critical experiment such as the

measurement of the speed of light preceded whatever imperialism was to follow. Instead, the paradigm's entry was marked imperceptibly at the time by the formalization of ideas that seemed, at best, 'reasonable' -- that candidates are drawn, unless constrained by special interests and the threat of abstention, to advocate the preference of the median voter on an election's salient issue; that one should not build too large a coalition or there will be no losers from whom to expropriate; that committees might agree to some middle position when debating a single issue; and that political institutions are the product of the self-interest of those who establish them. Although the authors of these ideas began a revolution within a discipline, these ideas hardly grab the intellectual imagination. Indeed, in a discipline possessing a surfeit of ideas but not of theory, they are easily lost in the noise or, as is almost always the case with general theoretical ideas, their intellectual antecedents can be found in a great many places.

It was not a particular substantive insight that was brought to political science from economics; rather, it was a method for the conduct of research tied to a general and malleable theoretical structure. Because this method was distinct from many of the established research methodologies in political science, Downs, Riker, Buchanan, and Tullock were not viewed as the intellectual kin of Bentley, Truman, Morgenthau, Dahl, and Key. Understanding gained of experience and time consuming empirical study, and explanation based on wisdom and ad hoc speculation no longer suffices with this paradigm. Instead, hypotheses had to be shown to follow logically from explicit assumptions before they qualified for a test of empirical validity. Hence, the initial rejection of this method as mere mathematical manipulations failed to appreciate the fact that the mathematics surrounding this structure merely represented the desire to understand phenomena generally, logically, and scientifically.

We might assert that the 'innovations' introduced by Downs, et al went largely unrecognized and were even boldly resisted because political scientists was simply too unscientific a discipline to appreciate its promise. Certainly, many natural scientists do not consider political science today (or even economics) to be a science, but the accusation of 'unscientific' as a pejorative label neglects the fact that political science is a discipline that has had its share of innovative thinkers. We can sympathize with those who viewed the erection of mathematical edifices as more often than not constituting an exercise in logic with a substantive significance not exceeding the mathematical puzzles in the back of any issue of **Scientific American**. More fundamentally, however, the early skepticism seems warranted and legitimately in the spirit of any scientific enterprise. If it is a general theoretical perspective that is the 'new idea' -- a more efficient route to explanation and understanding -- then no single research effort proves the case. Instead, unswerving cynics as well as potential converts can rightly demand an extensive theoretical development before acquiescing. Data and ideas must be demonstrably organizable in some more useful form, and new unanticipated insights must follow before paradigms change or one becomes dominant.

2. Successes of the Paradigm

We can speculate and write about the presumed imperialism of economics today, nevertheless, because of some very real accomplishments in political theory accomplishments that include the development of a formal theoretical structure for studying elections, the impact of congressional procedures and organizing principles, the genesis and maintenance of political institutions, the flow of international processes, and

the formation and justification of public policy. A review of those accomplishments is enlightening, but not because it illustrates the imperialism of economics. Rather, any such review brings into question whether such imperialism actually exists or, if it does exist, whether it is the imperialism of 19th century social theory.

Certainly, Downs's seminal contribution to the study of elections can be credited as the first major inroad into political theory of the economist's paradigm with its associated mathematical formalism. Although an exhaustive literature search might require the citation of prior inroads, it was the research into the spatial theory of elections and its subsequent publication in the 1960's in traditional political science journals that mark the sustained effort at developing a complete theoretical structure for at least one important political as against economic institution. But as we begin to penetrate this theory, the contributions of economics beyond the supply of its paradigm in the form of the assumption that candidates and parties are motivated principally by the desire to win elections and that voters act to maximize the benefits that they derive from government, become obscure.

One important contribution of the spatial theorist is the formal conceptualization of preferences that it offers. In microeconomic theory, the representation of commodity bundles by an Euclidean coordinate system and the summary of preferences over these bundles by preference sets or indifference curves are, together, a powerful analytic tool. The simple proposition that commodity bundles that maximize a consumer's utility are characterized, under suitable assumptions about preference and people's tradeoffs across commodities, by the tangency of an indifference contour and a budget constraint, marks the beginning of the use of mathematics in economics and the scientific generality that mathematics affords. Indeed, the simple notion of an indifference curve in conjunction with the idea of tangency to a budget constraint gives rise to the application of Kuhn-Tucker maximization conditions and to a plethora of ideas that have a mathematical representation, such as homogenous goods, consumer surplus, and elastic and inelastic demand.

Similarly, the representation of political issues by a similar coordinate system and the summary of voters' preferences over the issues by indifference curves with interior satiation points (e.g., concentric circles or ellipses) is an equally powerful device for those who seek to model political processes. Instead of viewing voter's decisions as "the product" of childhood socialization or partisan loyalties, those choices are explained by mathematical proximity to candidates on issues, broadly defined, and victorious candidates are characterized by the positioning of "median lines" and the like in this space. This representation, like the economist's, immediately gives rise to the development or application of mathematical ideas such as multidimensional medians, distributions of ideal points, and metrics for the representation of preferences.

But we should ask about the source of this preference representation. Briefly, that representation begins with Duncan Black's notion of "single-peaked" preferences on a political issue -- preferences that are characterized by an internal satiation point-- which he introduced as a means for escaping the dilemma of welfare economics that Arrow's Impossibility Theorem posed. Today we can deduce this idea from classical microeconomic representations. In the classical representation, consumers are characterized by the assumption that "more is preferred to less" and by indifference contours that represent tradeoffs between distinct commodities. The preferred commodity

bundle -- the point corresponding to the tangency of the "highest" such curve to a budget constraint -- is then determined by the consumer's income and the market prices of the goods in questions. Since, given income and prices, each consumer is dictator over his choice, this tangency marks the consumer's decision and the "market outcome" with respect to that consumer. But if, as in politics, the goods are publicly supplied and if their level of supply is dictated according to a mechanism such as majority rule, then we must have a complete accounting of preferences over the feasible set (budget constraint) since a consumer (voter, committee member, or legislator) may have to make compromises with others before a final bundle is chosen. If we make the usual microeconomic assumptions about preference and tradeoffs, then preferences over the budget constraint are "single-peaked" -- the consumer's ideal lies at the point of tangency, and preference decreases as we move along the constraint on either side away from this point (Ordeshook 1986). The central questions of politics, then, concern how political institutions such as elections, representative assemblies, and committees, in conjunction with the procedural details of these institutions, translate such preferences into a social decision.

This derivation of "political preferences" from "economic preferences" might be taken as evidence of economic imperialism, but in fact the derivation occurred long after the use of a spatial preference representation gained widespread acceptance in political theorizing. Indeed, it is this author's experience that many economists regarded the notion of an internal satiation point as merely a peculiar special case, and thereby resisted the supposition that general theorizing could proceed with it. But with this derivation, we now see that such preferences are not merely a special case; rather, they follow from the character of political institutions that distinguish them from decentralized markets.

With the formal representation of election processes that spatial preferences provides, formal political theorists maintained the analytic mode of the rational choice paradigm by hypothesizing a primary objective for key decision makers. Replacing the behavioralist view of people as black boxes that somehow reflected earlier experiences, political actors were modeled more clearly as active decision making agents. Candidates maximized the probability of election, and voters maximized the consumptive utility of the candidates' policies. The study of electoral processes then devolved less on measuring and weighing potential elements of the black box, and more on generalizing the structure of models, and on testing the implications of alternative hypotheses about people's goals--the goals of citizens and of candidates.

Despite this parallelism, however, initial developments gave rise to what appeared to be a great disappointment. Specifically, nearly all election models failed to yield the simple type of equilibrium found in microeconomic models of perfectly competitive markets (for a general survey see Enelow and Hinich 1985). Unless very restrictive conditions are imposed (such as that the election concerns a single issue or that the distribution of voter ideal points in the policy space was radially symmetric), there is no equilibrium platform for candidates -- every election platform can be defeated by some other election platform -- and, thus, there is no specific outcome that can be described as directly implied by preferences and institutional arrangement. And matters become even more muddled if we admit the possibility of more than two candidates or parties, if various forms of abstention are permitted, if we look at campaigns as a sequence of elections in which candidates must first secure the nominations of their parties, and if we take

account of the incomplete information about politics that characterizes voters.

This initial disappointment, however, soon gave way to new theoretical developments. Briefly, political theorists concluded from their unsuccessful attempts at replicating the equilibrium results found in microeconomic theory that research should pursue two avenues that were generally regarded in economics as refinements of their theory, and not as centerpieces -- an elaboration of the abstract description of elections to include a more dynamic element (cf. Kramer 1977), and the development of more general notions of equilibrium (cf. McKelvey and Ordeshook 1979). The result of such efforts to date has been a focus on the second avenue (but not a rejection of the first), accompanied by the application of ideas drawn directly from noncooperative game theory and aided by the development of other ideas drawn from social choice theory such as the uncovered set (cf. Miller 1980, and McKelvey 1986).

This change in research intent is important for understanding the influences of the two disciplines on each other. The early applications of game theory to economics generally sought to show how old results could be reformulated and generalized with an alternative structure (for example, that the core of a market game contracts to the competitive equilibrium as the number of consumers increases). But as it became apparent that the classical equilibrium results of microeconomics could not be replicated in political models, the development and generalization of game theory itself became a central activity of political theorizing. Although the general idea that key actors are rationally pursuing various objectives is common to the economist's models of markets and the political scientist's models of elections, profoundly important differences in theoretical emphasis then emerged.

A closer look at a particular model permits us to emphasize these points. Presently, we are seeing the importation of ideas from rational expectations models of markets so that we can better understand how democracies function in the incomplete information environments that characterize electorates (McKelvey and Ordeshook 1985, 1986). But aside from the initial insight that information is often obtained from cues and that these cues follow a dynamic that can yield, under the right circumstances, an equilibrium that corresponds to what we would observe if everyone were perfectly informed, no specific law or theorems can be borrowed to complete the theoretical enterprise. Instead, modeling must proceed "from scratch" so that it is adapted to the specific situation under consideration. In the case of elections, although we can think of the cues provided by public opinion polls and the endorsements of interest groups as offering signals similar to the signals provided by price in financial markets, the precise way in which these mechanisms operate is quite different owing to the fact that markets and elections are organized differently. Because there may be decision makers with a special influence on parameters (e.g., candidates and interest groups), we must look at the opportunities for strategic misrepresentation of preferences; and because elections are "infrequent events," we must pay closer heed to the temporal dynamics governing convergence to an equilibrium.

What we want to emphasize here is not the details of theoretical developments, but rather the fact that political scientists (and at least one statistician) were at the forefront of the effort to formulate a rigorous deductive theory of democratic elections, and that this research did not consist of the mere application of ideas borrowed from economics. First, we see a radical revision in the representation of preferences. Second,

we find either the development of new equilibrium concepts that appear wholly within the discipline of political science, or we see that preexisting notions of equilibrium that are originally deemed tangential to the economist's principle concerns becoming central to political theory.

It is also interesting to note the different responses of economists and political scientists to the election theory that Downs's volume began. Perhaps the simplest, least general, but most widely cited result is the **Median Voter Theorem**, which states that if a two-candidate majority rule election concerns a single issue, if the information of voters about candidates and of candidates about voters is perfect, if all citizens vote, and if there are no constraints on candidate strategies, then both candidates should converge to the electorate's median preference. Political scientists agree that such a model captures but a small part of the forces that operate in even the simplest election and thus their instinct has been to generalize the result to include multiple issues, nonvoting, incomplete information, interest group influences, and nomination procedures. Economists, on the other hand, often take the result as an excuse to eliminate politics altogether from their analyses. With a quick reference to the result, levels of consumption of goods and services in a consumer's utility function that are labeled public, as well as the taxes that constrain individual budgets, are assumed to be dictated by a median preference. Thus, the economist's contribution falls short, and provides but the initial structure and perspective.

The empirical testing and application of spatial theory tells much the same story. Although we might again cite the spread of econometric techniques as another instance of economic imperialism, the imperatives of political theory caused those methods to be modified, and in some instance completely supplanted by new technologies. The very nature of spatial theory requires a distinct methodology for estimating preferences. But economics was largely devoid of appropriate methods, and although early efforts leaned heavily on the multidimensional scaling technologies borrowed from psychology, it is now apparent that new and more sophisticated methodologies must be developed. Thus, although we cannot dispute the extensive application of econometrics, we also find the adaptation of methods to the particular theoretical structures of politics.

This story is repeated again by a review of the research inspired by Arrow and Black, except that here the effort involves an even more diverse collection of scholars drawn not only from economics and political science, but from philosophy as well. To give some coherence to this research, it is convenient to divide it artificially into two categories -- social choice theory and the study of committees. We can take social choice theory to mean the normative study of social welfare and its axiomatic relationship to individual preferences. Again, the mode of analysis is primarily 'economic' with its dependence on individuals as the primary units of analysis and its assumption that individuals seek to maximize utility based on these preferences. But after this is said and after the seminal contribution of Arrow is cited, it is difficult to assert that economics is the "home base" of even a majority of subsequent research.

Perhaps the most important and general result to follow Arrow's (1951) Impossibility Theorem -- the proof that most reasonable rules for aggregating individual preferences into a social preference need not yield a transitive social preference relation -- is Gibbard (1971) and Satterthwaite's (1971) result about the manipulability of aggregation mechanisms. Following Arrow's axiomatic approach this result establishes that reasonable

rules are manipulable -- that for a broad class of social choice mechanisms, circumstance can arise in which one or more persons will not find it in their interest to reveal their sincere preferences over alternatives. This result, of course, places strategy, and thus game theory, at the heart of the study of decision making in social processes. But Gibbard is not an economist; rather, he is a philosopher teaching in a philosophy department. And the extension of Arrow's theorem to cyclic preferences (as against the weaker form of intransitive social orders) was accomplished by another philosopher, Thomas Schwartz, teaching in a political science department and published in the eclectic journal, **Theory and Decision**. Economists such as Amartya Sen, Peter Fishburn, and Charles Plott have made seminal contributions to social choice theory, but what followed the publication of Arrow's volume is less the imperialism of economic thought and more the development of a new subdiscipline with roots in a great many disciplines.

With respect to the study of committees, Duncan Black's research is seminal and is closely connected to Arrow's in that Black sought to formulate an empirically viable violation of Arrow's "universal admissability of preference" axiom that escaped the dilemma of intransitive social preference. But the differences between Black and Arrow's research are important, and for that reason Black's work has had a more profound impact on political theory's development. Arrow sought to delineate the general properties (if not the impossibility) of social welfare functions. But normative theory can play only a small role in a discipline not yet armed with a universally accepted descriptive theory. Black's research, on the other hand, was more descriptive in its intent, and can be viewed retrospectively as following in the tradition of classical microeconomic theory. There, specific topological assumptions about consumer preferences permit us to deduce the existence of equilibrium outcomes (and their properties) in a particular institutional arrangement -- unregulated competitive markets. Black's seminal contribution -- in **The Theory of Committees and Elections** as well as in his short monograph with R.A. Newing (1951), **Committee Decisions with Complementary Evaluation**, which predates even Arrow's work -- was, as we have already noted, to supply a conceptualization of preferences (single peaked and spatial) that is especially germane to politics and which provides the basis for a genuine "micropolitical theory."

But before we cite Black's work as an instance of economic imperialism, we should also note that Black can hardly be classified as a 'mainstream' economist, and were it not for the interest that his research generates in political science, that research almost certainly would go unappreciated. Indeed, much of the research on committees that follows Black's lead is conducted by political scientists. Fused with the insights into the theoretical nature of voting provided Robin Farguharson in **The Theory of Voting**, and fused as well with the Downsian hypothesis that the primary objective of legislators is to secure reelection, that research is the basis for what political scientists call the "new institutionalism."

Briefly, this new institutionalism is a response to the implicit determinism of the behavioralists, whose "revolution" after World War II is seen as a response to the nearly atheoretical, descriptive mode of political science then dominant. But that earlier mode focused also on political institutions -- the structure of legislatures, electoral rules, Constitutional provisions, and the like -- which is a disciplinary emphasis that somehow was lost in the definition, measurement, and correlation of "social class," "partisan identification," "attitudes," "childhood socialization," "norms," "socio-economic status," and

the like. Contemporary research is a synthesis that does not reject the insights of behavioral research. Indeed, it depends on them for understanding the underlying nature of preferences and perceptions. But this synthesis examines attitudes, preferences, and perceptions in the context of the constraints set by institutions. These institutions, in turn, (e.g., legislative committee structures, the existence of regulatory agencies, budgetary procedures, agenda and other voting rules) are viewed as endogenously determined by individual preferences, tradition, and transaction costs as well as important determinants of individual preferences alternative actions. Thus, "with everything connected to everything else," the study of institutions that emerges as the hallmark of modern political theory holds the potential of being the base for a grand synthesis of all of the intellectual traditions in political science.

Once again, then, although economics provides the paradigmatic structure and perhaps even the initial insight about the conceptualization of outcomes (Euclidean) and the corresponding representation of preferences, its "imperialism" stops short of the actual taking-over of a discipline. No theorems from economics are grafted onto political theory. Rather, after the initial insight about outcomes, preferences, and the motivations of choice are accepted, assumptions particular to the political institutions under investigation are developed and theorems are established by political scientists, with at most the occasional foray of an errant economist into the area. Indeed, the results established here are generally of a different flavor than those found in economics. For example, it is now understood that with multiple issues, majority rule equilibria in the form of Condorcet winners -- outcomes that defeat all others in a majority vote or which cannot be defeated -- are generically rare. However, various procedural devices, such as voting on issues one at a time, or agendas such as those found in the U.S. Congress that pair alternatives against each other in some specific order, can yield a determinate outcome. But now, learning the properties of such outcomes must follow the logic of Gibbard and Satterthwaite's results about strategic manipulability -- a logic that is irrelevant to microeconomic models in which the topic of interdependent choice is rendered mute by "appropriate" assumptions. Specifically, this logic must model the strategic sophistication of voters and their knowledge and beliefs about the preferences of others.

3. The Political Scientist's Contribution

To this point our survey suggests that although individual economists and their theorems are not imperialistic, the spread of the paradigm is a "one way street" of fundamental ideas -- from economics to political science. However, economics itself is being changed in the process. Although politics does not have a paradigm to transport to economics, it does contribute a concern, as we have just seen, with specific types of institutions and with the paradigm's inadequacies for studying those institutions.

Perhaps no assumption contributed more to the development of classical microeconomic economic theory and to contemporary economic thought than the assumption that there are a sufficient number of consumers and firms in markets that no decision maker affects price. This assumption decouples decisions and removes from the scene a great bugaboo -- interdependent decision making. With this bugaboo absent, classical decision theory is all that the economist needs to pursue his craft, and a single course in calculus or in real variable analysis suffices to permit the student to comprehend professional manuscripts.

The specific part of the paradigm that is designed to wrestle with the issue of

interdependent decision making is game theory. However, despite the fact that this bugaboo cannot be removed from any other social process, all but a handful of economists, for nearly twenty years, resisted an extension of their paradigm to this theory. And in this resistance we find a failure of the economist to develop his paradigm fully.

This resistance has several explanations. First, there is the fact that game theory once again reveals the necessity of cardinal, as against the weaker ordinal conceptualizations of utility. Although the theory survives without cardinal utility (preferences must then be defined over lottery spaces rather than over a simple outcome space), the paradigm's parsimony is greatly diminished. Moreover, experimental, theoretical, and empirical explorations into cardinal utility reveal problems with the concept of utility, both ordinal and cardinal. Second, the early applications of game theory to economics revealed few new theoretical insights. The various parts of game theory were deemed as either irrelevant to the central problems of duopoly and oligopoly, or as merely providing unsatisfactory answers. (There is, of course, a "chicken-and-the-egg-problem" here that we cannot resolve. Did game theory fail to yield new theoretical insights because economists did not pay sufficient heed to that theory, or vise versa?)

While economists fretted over the necessity of cardinal utility and over alternative ideas for modeling the specific forms of interdependent choice occasioned in markets by the presence of but a few firms in an industry, political scientists such as William H. Riker in **The Theory of Political Coalitions** cast a broader substantive net while focusing on the cooperative game theory that von Neumann and Morgenstern offered. This net ranged from predicting coalitions in legislatures and parliaments to understanding stability in international systems, but the focus revealed the far more disquieting fact that the concept of rationality itself, of utility maximization, was ill defined. If von Neumann and Morgenstern, aided by Nash's definition of noncooperative equilibria, showed that interdependent choice per se was not a bugaboo, they failed to show that the paradigm had anything whatsoever to say about cooperative decision making.

Ostensibly, von Neumann and Morgenstern's **The Theory of Games and Economic Behavior** makes two profound contributions. It shows us how to model and analyze two seemingly distinct situations: those in which interdependent decisions are noncooperative (participants cannot form binding contractual arrangements), and those that are cooperative (in which binding agreements are possible). Although their theory of the first situation, when expressed in the context of its extensive form representation, could be connected to the classical form of the paradigm, no such connection was evident for their theory of the second situation. Both their characteristic function representation of the value of coalitions -- of alternative contractual arrangements -- as well as the V-set solution that they proposed to treat cooperative games were ad hoc, with virtually no theoretical justification. Certainly, these two ideas could not be deduced from any general assumptions about utility maximization. Even one of the theory's founders surmised that, as formulated, it had a fundamental flaw -- that if people learned the theory, they might seek to avoid its consequences, thereby invalidating its predictions (Morgenstern and Schowdiauer, 1976).

Not surprisingly, then, aside from the reformulation of classical microeconomic theory as a cooperative game (see the contemporary research of Herbert Scarf, Robert Aumann, and Martin Shubik and the classical foundation established by Edgeworth), economists largely ignored cooperative game theory as originally formulated. But rather than grapple

with the problem of developing a better theory, the economist's research here-- consisting of the formulation of market games, of nonatomic games, and of the development of value theory -- was less fundamentally conceptual and more an exercise in pure mathematics. Instead, a fuller explication of the paradigm's inadequacies and attempts at resolving unanswered theoretical questions were left in the decades of the 60's and the 70's to game theorists such as John Harsanyi, and to political scientists, sociologists, and psychologists (e.g. William Riker, Robert Axelrod, Anatol Rapoport, Howard Rosenthal, James Laing, Robert Caplow, Abraham DeSwann). Much of this research, admittedly, was no less ad hoc than von Neumann and Morgenstern's original effort. Certainly the notions of connected or minimal variance coalitions that some hypothesized to explain and predict coalitions in parliamentary governments in a spatial context could not be deduced from rationality postulates, Riker's "size principle" dealt with a special case and assumed the inadequate characteristic function representation of situations, and theories about "coalitions in the triad,' or theories applied to majority voting games were hardly general. Moreover, the mathematical formulations of these ideas were frequently inelegant. But what these efforts reveal is a groping, not on the part of economists, but on the part of others, with the paradigm's fundamental inadequacy.

Today we see a rekindling of the economist's interest in the problems that cooperative game theory seeks to treat. This interest is in part brought on by the development of new equilibrium concepts and more sophisticated methods for studying sequential and extensive form games. The distinction between game theorist and economist is now blurred, and the result is the promise that noncooperative game theory will soon accommodate those choice situations that von Neumann and Morgenstern sought to treat with their cooperative theory. However, even if we admit that economists are on the forefront of developments in game theory, that development is not confined to the discipline of economics. Although most political scientists are content with consuming the game theorist's product, the problems that political science brings to this research has a profound effect on theory.

For example, if we approach cooperative game theory along the same avenue as the one proposed originally by von Neumann and Morgenstern (via the characteristic function representation), we learn quickly that derivative solution hypotheses such as the V-set and the various bargaining sets do not exist in general or that they make silly predictions when preferences are of the sort introduced by Black. Indeed, the spatial preferences that Black introduced provides, in conjunction with a specific concern with majority rule voting games, a general setting for developing and testing alternative hypotheses. Political science is also especially concerned with the impact of institutional structures or, in the case of international relations, with systems that either have no structure or with structures that must be treated as endogenous. This concern forces the game theorist to be especially cognizant of those aspects of the environment that constrain individual decisions, and with developing a theory that permits institutional structure to be an element of the strategy set of decision makers.

What ought to be emphasized here is that research into cooperative game theory differs importantly from previous applications of the paradigm to politics. In following Downs, Black, and Arrow, political scientists accepted the paradigm, and molded new theoretical results around it. Here, however, we see the joint development of a fundamental part of the paradigm itself. There cannot be any economic imperialism in

this context simply because the economists did not have a fully developed theoretical structure to transmit.

4. **Public Choice**

If an argument can be made that the imperialism of economics is something more than the mere transmittal of its paradigm, that argument must be made in the context of research that we can loosely place under the rubric **Public Choice**. Foregoing an accurate accounting of the contributions of numerous contemporary scholars, and with all due apologies to historians such as Charles Beard, we might date the infusion into political science of ideas from such fields as public finance and social welfare with the publication of James Buchanan and Gordon Tullock's seminal volume **The Calculus of Consent**.

Buchanan and Tullock's volume begins with a simple and seemingly self-evident premiss: The variety of institutions, procedures, constitutions, and the like that are designed to effect the allocation of scarce resources are human creations, and as such their development, form, and existence can be understood only by a general understanding of the purposes they serve, of the individual objectives they satisfy, and of the consequences to individual decision makers of alternative institutions. Even so simple a procedure as pairwise majority-rule voting has many challengers, such as unanimity, dictatorship, 2/3's voting, 3/4's voting, etc. To understand why majority rule is adopted in lieu of these alternatives, then, we must understand, from each participant's perspective, the potential costs and benefits of one rule as against another.

As simple as this idea appears, its consequences escape the minds of a great many people. Neo-liberals who argue or act in the mistaken belief that the "public good" can be achieved by willing it -- that all one need do to implement as particular policy is to devise, say, a regulatory agency empowered to implementing that policy -- fail to grasp the implications of Buchanan and Tullock's argument. Those who implicitly or explicitly suppose that disarmament ends war, that welfare programs end poverty, or that education expenditures terminate illiteracy and ignorance, fail to understand that whatever institution we establish to achieve some end, that institution will endure only if it serves the purposes of various individuals. And to understand how those institutions might function, or how they might be subsequently modified, we must understand what individuals are relevant, what ends they pursue, and how other rules and institutions mold their interactions.

A central thrust of Buchanan's research has been to render the field of public finance less normative and more descriptive -- or at least to ensure that its normative prescriptions are descriptively realistic. With Tullock, his call for descriptive realism is extended to the study and evaluation of all institutions -- fiscal or otherwise. The consequences of these ideas have been profound not just for economists but for political scientists as well. We can trace to them the establishment of the interdisciplinary **Public Choice Society** and its journal **Public Choice**, the writings of Mancur Olson, especially his **Logic of Collective Action**, as well as William Niskanen's **Bureaucracy and Representative Government**, and even perhaps the development of models of principle-agent relationships (although here, the ideas seem to come more from accounting). And since the publication of **The Calculus of Consent**, political scientists have felt compelled to become more familiar with indifference curves, supply and demand curves, concepts of elasticity, market equilibrium, efficiency, public and private goods, and consumer surplus, as well as journals such the **Journal of Political Economy, Public Finance, The National Tax Journal**, and **The**

Journal of Public Economics. Thus, if we are to find the imperialism of economics in a pure form, we ought to find it here.

But if we examine the flow of ideas more deeply, the nature of this presumed imperialism becomes less clear. Consider Olson's influential **Logic of Collective Action**. From the view of formal economic theory, there is little in this monograph that we cannot attribute instead to economists such as Samuelson, Pigou, or Pareto. Its contribution, however, is the interpretation given to the concepts of public vs. private goods, externalities, and the causes of market failure. Quite directly, Olson transforms our thinking about interest group politics, neo-Marxist theories, and the nature of revolution, while simultaneously questioning the Madisonian premise that threatened interests in a democracy will defend those interests and produce an efficient balance of forces. New ideas entered the political scientist's dialogue such as political entrepreneurship as well as the possibility that the causes of government "failure" may be as general and as theoretically identifiable as those linked to market failures.

But if we ask what cleared the path to this achievement in political science, our answer seems to be that political scientists somehow forgot their roots even as they studied them. Consider, for example, the following well known quotation from Rousseau's **Discourse on the Origin and Basis of Inequality Among Men**:

> If a group of [men] set out to take a deer, they are fully aware that they would all have to remain faithfully at their posts in order to succeed: but if a hare happens to pass near one of them, there can be no doubt that he pursued it without qualm., and that once he had caught his prey, he cared very little whether or not he had made his companions miss theirs.

or this insight of David Hume in **A Treatise on Human Nature**:

> It is very difficult, and indeed impossible, that a thousand persons should agree in any such action: it being difficult for them to concert so complicated a design, and still more difficult for them to execute it; while each seeks a pretext to free himself of the trouble and expense, and would lay the whole burden on others.

or, finally, this passage from Hobbes's **Leviathan,**

> Whatsoever therefore is consequent to a time to Warre, where every man is Enemy to every man; the same is consequent to the time wherin men live without other security, than what their own strength, and their own invention shall furnish them withall. In such condition, there is no place for industry; because the fruit thereof is uncertain: and consequently no Culture of the Earth; no Navigation; no use of the commodities that may be imported by Sea; no commodious Building; no instruments of moving and removing such things as require much force; no knowledge of the face of the Earth; no account of Time; no Arts; no Letters; no Society; and worst of all, continuall feare and danger of violent death; and the life of man, solitary, poore, nasty, brutish, and short.

If anything, these well known passages raise the question: How did political scientists fail to develop the ideas of private vs. public goods, in conjunction with a formalization of ideas such as the prisoners' dilemma implicit in classical writings? And since equivalent quotations can be found in the writings of such practical political scientists as James Madison or Thomas Jefferson, we are lead to ask: Why was it not the case the political science assumed the imperialistic mantle attributed to economics?

It is beyond the scope of this essay to seek satisfactory answers to this question, but this much is evident: Although 20th century economics provides the precise definition and formal refinement of ideas such as Pareto optimality, externalities, and jointly supplied goods, as well as an exact formulation of the relationship of these ideas for decentralized social processes, "refinement" is the proper word since many of those same

ideas were perceived by social theorists at least two hundred years earlier. But even if those concepts remained central to political thinking in this century, with no explicit paradigm to hold them in place, we were not assured that all thinking remained consistent with them. Economics appears imperialistic, then, not merely because of its formalism, but because its paradigm cements these concepts into an integrated theoretical structure that allows us to see their generality.

A useful example of the economist's and the political scientist's relative contribution in this area concerns the issue of government growth. One of the principle empirical regularities that we can observe about social processes today is the increasing size and domain of the public sector in nearly all countries. This growth is especially perplexing if we also accept the proposition that much of what governments do is economically inefficient -- that there are a variety of decentralized mechanisms for achieving equivalent ends at considerably reduced social cost. The question then becomes: What accounts for this seemingly pervasive and increasingly prevalent form of "social irrationality?"

Numerous economists have sought answers to this question using the tools normally at their disposal, such as the concepts of fiscal illusion and the relative costs of labor versus capital intensive activity. But none of these explanations is generally adequate, and instead research has focused on more game-theoretic ideas such as the inefficiencies associated with prisoners' dilemma situations (for a survey see Aranson and Ordeshook 1985). If markets fail whenever costs are private and certain goods are public, then the public sector can "fail" as well because, even in regulating the supply of public goods, it must confer private benefits (e.g., benefits to interests groups) at public cost. Thus, the public sector is the dual of the private sector, and the inefficiencies possible in markets find their counterpart in governmental activity.

Understanding how such inefficiencies arise and are maintained, however, cannot rely on the mathematical relationships among marginal utilities that show the inefficiency of the private sector with respect to public goods. Instead, the processes of representative government must be modeled, and the imperatives of election and voting uncovered. All of this, of course, takes us back to the problem of applying the paradigm to non-economic institutions, in which case essentially non-economic notions of equilibria must be applied. Once again, then, we see that although economics provides the initial insight in the form of a precise theoretical representation of key concepts (externalities and public goods), theorizing must proceed anew.

Today, then, Rousseau's ideas or Hobbes's or Hume's or Riker's or Dahl's or Key's or Buchanan's or Tullock's or Olson's can be compared, and their logical connections assessed. What emerges from volumes such as **The Calculus of Consent** and **The Logic of Collective Action** is an economic imperialism that takes the form, not of the transmittal of specific laws or the adaptation of theorems about supply and demand or even, necessarily, insights that were not gained by less precise methods, but rather of the extension of a paradigm that formalizes those ideas and establishes their generality.

5. Conclusions

Although the rational-choice paradigm may not yet be the dominant paradigm of political science, it is the most prominent. It serves today as the successor to the "behavioralist revolution" of the 50's and 60's and so it seems only reasonable to anticipate that the study of politics and of economics can once again become wholly

integrated disciplines. This is not to say, of course, that we can anticipate the eminent demise of disciplinary boundaries within universities. Bureaucratic inertia is a heavy burden, and political scientists and economists do not always share substantive concerns. Nor should we suppose, even as the perceived domain of the paradigm expands, that complete integration is a necessary byproduct. Other paradigms will remain active in political science.

A simple example illustrates this point. It is now generally believed that voting in mass elections is, for the rational-choice paradigm at least, a paradox. Given that the probability of one's vote being decisive for choosing a winner is so slight (even zero) and given the evident cost of voting, no rational calculus seems capable of explaining the act of voting without recourse to assumptions about benefits from citizen duty and the like (Riker and Ordeshook 1968). But scholars whose research lies imbedded in the paradigm regard such assumptions as ad hoc and unacceptable. They contest the assumptions with the argument that although costs are evident and probabilities are an essential component, admitting the presumed psychic rewards of merely choosing an act renders the paradigm tautological. Such attitudes, however, highlight the myopia that emerges if we depend on a single paradigm that cannot explain everything. Voting is a paradox only if we assume that the sole "objectively real" utility terms are private costs and the public benefits associated with choosing one candidate as against another. Private benefits associated with interpreting an action as a consumption good are not "objectively present" and thus seem out of place if they are included in the analysis. However, this reasoning also leads to the conclusion that eating, for example, is a paradoxical act. After all, eating is costly (in terms of time and the resources spend for food), and these costs must exceed the benefits associated with one's slight contribution towards the public outcome of reducing world hunger. Hence, the choice of nearly any act becomes paradoxical with an appropriate conceptualization of the decision problem. Of course, common sense tells us that we eat to ensure the private benefit of survival; but is that any more real than voting to ensure the private benefit of feeling good that we have participated in a democratic act.

We can be led to inaccurate conceptualizations of a person's decision calculus, of course, because the paradigm is silent on the basic sources of people's preferences and on the determinants of their perceptions of the situations that confront them. Designing alternative conceptualizations and making an appropriate selection are inventive enterprises, and the paradigm places only modest constraints on our creativity. And more to the point, we may have to rely on other disciplines (e.g., psychology) for guidance about these matters.

There are indications, moreover, that other paradigms will influence that of rational choice, but if some of these changes occur outside of economics, then whatever imperial position we are willing today to attribute to this discipline will dissipate. Because economics is arguably more advanced in the use of the rational-choice paradigm -- at least in the mathematics of that paradigm -- it is reasonable to anticipate that we should see changes there first. One source of change may be the increasing realization, deriving from a considerable body of experimental research in economics, decision theory, and psychology that the axioms of cardinal utility abstract greatly from the actual decision processes of people. Alternative axiom systems may be devised, but the fact remains that the simplification that von Neumann and Morgenstern proposed ignores much of reality. A

second source of change derives from Herbert Simon's early notions of bounded rationality and from attempts at incorporating some of the lessons of modern genetics into a more comprehensive theory of economic systems. Although a variety of dynamic models abound in economics, economic theory has little to say about truly dynamic systems in which alternative institutional structures emerge and decline. Just as the state has expanded in this century and regulatory activity increased and declined in the United States, market structure is not static, and the truly profound questions concern that nature of this dynamic -- the dynamics of social organization.

Hints of changes in the paradigm appear in economics, but generally we see a natural resistance to such ideas. It took game theory nearly forty years to enter the mainstream of economic theorizing, and today most practitioners still prefer using some standardized tools of their craft, such as Kuhn - Tucker conditions for optimization, classical formulations of consumer preferences and firm objective functions, and arguments drawn from basic notions of supply and demand. These are powerful tools; nevertheless, we should predict that economics will be changed by its success in transmitting to other disciplines the parts of that paradigm that it develops. To the extent that practitioners from other disciplines become skilled in the application and interpretation of the rational-choice paradigm, they will begin changing the paradigm to suit their needs and in response to their disciplines' prejudices about reality. Put simply, even the formalism of the paradigm no longer belongs to economics -- it has become fair game to a great many disciplines not merely as a target for criticism, but now as a target for theorizing.

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