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Equilibrium, Disequilibrium, and the General Possibility of a Science of Politics

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ABSTRACT

In recent years important theoretical contributions have shown that majority rule is a very badly behaved collective choice mechanism. In the absence of artificial restraints on preferences majority rule processes are almost always in disequilibrium. Moreover, the extent of the disequilibrium is pervasive, as captured by the observation that "anything can happen". What are the implications of such nihilistic results for the study of democratic political processes? Some authors believe that the implications are major, that they in fact preclude the development of a science of politics. Other authors take a more sanguine view. This essay argues that equilibrium notions, as presently formulated, are neither necessary nor sufficient for the development of a scientific study of politics. The newly proved disequilibrium results do suggest a change in the research agenda facing political scientists. The broad outlines of that agenda, and a general strategy for proceeding are discussed.

Equilibrium, Disequilibrium, and the General Possibility of a Science of Politics*

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1. Introduction

Perhaps it overstates matters to say that there is a crisis in formal political theory, but it is apparent that much mischief has been caused by a series of theorems which depict the chaotic features of majority rule voting systems. These theorems, proved elegantly in recent papers by Cohen (1979), McKelvey (1976, 1979) and Schofield (1978), establish that the cyclicity of the majority preference relation is both generic and pervasive. To paraphase the title of a recent paper by Bell (1978), when majority rule breaks down, it breakes down completely; and it "almost always" breaks down.

Although these results are of relatively recent vintage, and their implications are only now being traced, signs of intellectual indigestion are already observable. Certainly, there is no clear consensus on the import or significance of these results. At one extreme, some scholars continue business as usual. The new results simply constitute a political fact of life -- a fact that may be artfully employed to further an interest. Thus, Plott and Levine (1978) and McKelvey (1977, 1978), posing as latter-day Machiavellis, profess to offer advice to the price of a majority-rule committee -- the chairman -- as to how he might manipulate the sequence of committee votes in order to arrive at a final committee decision identical to his ideal point.

At the other extreme, some scholars fear that the new disequilibrium results are inimical not only to current ideas about politics, but to the scientific enterprise itself. These scholars infer that in consequence of majority cycles, there are no political regularities ("anything can happen"), or that what appear to be regularities are inexplicable as equilibria in some model of politics. This latter view is advanced and discussed in a recent symposium on disequilibrium and majority rule (see Riker, 1980; Ordeshook, 1980; and Rae, 1980).

The Cohen-McKelvey-Schofield theorems are profound and, as noted above, are only now being digested by students of the science of politics. These results provide some basis for questioning the utility of equilibrium concepts and provide the occasion for us, in this paper, to explore the importance of equilibrium (and its absence) for a science of politics. We begin, in the second section, by reviewing the reasons why equilibrium concepts, in <u>all</u> the social sciences, are not what they are sometimes believed to be. Even when equilibria exist, they are often imprecise, unrelated to observable regularities, or dependent upon unjustified (if not perverse) individual behavior. In the third section, we advance a possibly controversial position -- that instances of disequilibrium, as in the Cohen-McKelvey-Schofield results, are not nearly so serious or debilitating for a science of politics as sometimes feared. We argue that the distinctions between equilibrium and disequilibrium are typically overdrawn, and that the existence of equilibrium in one model as opposed to another, or indeed in one discipline as opposed to another, is largely a matter of scholarly choice. In the concluding section, we offer a not particularly original suggestion that political theorists avoid the choices made in both economic

theory and in recent efforts in formal political theory, and instead follow a third path when formulating models to explain observed political regularities.

2. Do Equilibrium Results Provide the Basis for a Science of Politics?

We have neither the competence nor the inclination to engage in an abstract discussion of the philosophy of science. Our viewpoint in this paper is that of practioners who feel confident that at least some part of their research activity is "scientific" in nature. To us, science is a method for comprehending the world, not as a collection of unique events but in terms of <u>regularities</u> which may be observed in the world. Such regularities include the repeated occurrence of particular <u>outcomes</u> -- the regular formation of minimal winning coalitions, for example. They include the existence of <u>trends</u> -- by a variety of measures modern governments have steadily grown, for example. And such regularities include the existence of <u>patterns</u> -- the identification of an elections-economic cycle is an example. Regularities are preconditions of scientific analysis.

Traditionally, the scientific method has aimed at formulating theories which would account for observed regularities. And a generally accepted condition for a theory to be judged scientific is that its implications be clear and specific. It is this condition that excludes "the will of God" or, according to many, psychoanalysis, from the class of scientific theories. The implications of a scientific theory must be sufficiently clear and precise that competent scholars can agree upon real world data which could in principle be inconsistent with the theoretical implications. The question we pose is whether existing social science equilibrium theories generate such clear and precise implications.

Certainly, if the concept of equilibrium present in social science equilibrium theories were akin to the concept of a "black hole" in space, social science theories would have clear and precise implications. Such equilibrium would have irresistible attracting power, and once attracted, nothing would escape them. Unfortunately, there are myriad equilibrium concepts in social science theories, and few put one in mind of black holes, even gray holes for that matter. Some of the problems with social science equilibria are well known and the subject of scholarly concern. Others are obvious but by general agreement not discussed -- the soft underbelly of social science. In this section, we will briefly review the state of social science equilibrium theory as a means of reminding our colleagues of how fragile most equilibrium theories are.

We begin by noting the manner in which equilibrium concepts differ from "black holes," lacking either their drawing power or their capacity to retain. Were a concept to lack <u>both</u> attraction and retention properties, then it certainly would be inappropriate to describe it as an equilibrium. If, on the other hand, it possessed both, then in some respects, it would be on a par with black holes. With this in mind, consider the set A of alternatives, the <u>majority dominance relation</u>, $>: A \times A \to A$, and an alternative, $x^* \in A$, with the following property:

> x^* satisfies the condition that
>
> $x^* > y$ for every $y \in A - \{x^*\}$

We can assert, though not without qualification, that if "black hole" equilibrium concepts are to be found in social science, then x^* must be included among them. For surely it retains in the sense that, once arrived at, it is never departed from. Thus, if a <u>majority trajectory</u>

$(x_1, x_2, \ldots)$, where x_{i+1} differs from x_i only if $x_{i+1} > x_i$, ever reaches x^* it remains there, since x^* is undominated, viz., $(x_1, x_2, \ldots, x_i, x_{i+1}, \ldots, x^*, x^*, \ldots, x^*)$. Its attraction characteristics, however, must be couched in a more contingent fashion. In particular, the attraction of x^* depends not only on the majority dominance relation, $>$, but on the rules for comparison as well. Even if an $x^* \in A$ exists, it may never be "reachable" from some specific initial status quo if it is excluded from comparison by features of the agenda-construction process. It may be said, however, that if there are no agenda obstacles, so that every majority trajectory must pass ultimately through x^*, then, by stipulation, x^* is an attractor, or sink.

This distinguished point, along with the added stipulation, identifies a scientific ideal for the class of equilibrium notions in the context $(>, A)$. But, on the strength of the Cohen-McKelvey-Schofield theorems, it is not very interesting in the following sense: if $\{(>, A)\}$ is the family of contexts consisting of a set of alternatives and a majority dominance relation, and μ is an appropriate measure on subsets of this family, then the subset consisting of contexts in which x^* exists is of measure zero. For our purposes, then, equilibrium concepts must have strong attraction and retention properties, but we cannot require x^* as stipulated for then we risk (with near certainty--hardly a risk!) coming up empty-handed.

It is well-known that most current equilibrium ideas in political science and economics fall far short of the "black hole" desiderata in that they entail relaxing either the strong attraction property or the strong retention property that "black holes" possess. Consider first the <u>core</u>,

$C \subseteq A$, consisting of undominated outcomes: $C = \{x \in A | y > x \text{ for no } y \in A\}$. When x^* exists, it is an element of C, but the requirements for membership in C are not so stringent. C consists of the set of strong retainers. If a majority rule trajectory moves into C, it will not depart; indeed, it will not leave the particular element of C at which it arrives.

Yet, the following example from Ferejohn, Fiorina and Packel (1980) illustrates the well-known weakness of the core's attraction properties. Let there be four voters and five alternatives described by the following schedule of preferences:

Voters			
1	2	3	4
x_1	x_2	x_3	x_4
x_2	x_3	x_4	x_1
x_5	x_5	x_5	x_5
x_3	x_4	x_1	x_2
x_4	x_1	x_2	x_3

$>$ is the strict majority preference relation according to which $x > y$ if and only if x obtains three or more votes against y. According to $>$, x_1, x_2, x_3, and x_4 cycle. None strictly majority dominates x_5 -- hence $C = \{x_5\}$. However, x_5 strictly majority dominates none of the remaining alternatives. Thus, if the institutional matrix into which $(>, A)$ is embedded designates some $x \in A$ as the initial status quo, then only if x_5 receives this designation will it ever be observed as the group choice.

Lest the reader think this critique of the core (even when it exists) is limited to politics in general and majority rule in particular, it should

be observed that the core of a private economy is plagued by similar defects. Weintraub (1979, p. 35), describing the grand edifice of general equilibrium thoery elaborated by Arrow, Debreu and McKenzie, concedes that "a particularly curious dynamic process was needed to ensure any robustness of [the core]." Noting, moreover, the state of flux in which stability theory, the study of dynamic adjustment, currently resides, he quotes one of its more eminent students, Frank Hahn, to the effect that the theory of dynamic adjustment to a general equilibrium consists of a "collection of sufficient conditions, anecdotes really."[1] So it seems, then, that even when an equilibrium with strong retention properties exists, its accessibility depends upon the institutional matrix in which it is embedded; and our theories, thus far, of this larger context are only "anecdotes really." The core, in sum, is not only plagued by familiar existence problems; additionally, it may not be a very attractive equilibrium (pun intended!).

Other cooperative equilibrium concepts fare no better. Whereas the core is retentive but not necessarily attractive, the stable set, bargaining set, and competitive solution (to select some of the more prominent alternatives to the core) tend to be attractive but not retentive. We restrict our remarks to the von Neumann-Morgenstern stable set, though they apply to the others as well. The stable set, or V-solution, is a collection of alternatives no one of which is dominated by any other and any nonmember of which is dominated by some member. Thus, for the 4-voter, 5-alternative example of Ferejohn, Fiorina and Packel given above, there are two (nondiscriminatory) V-solutions,

$$V_1 = \{x_1, x_3, x_5\}$$

$$V_2 = \{x_2, x_4, x_5\}$$

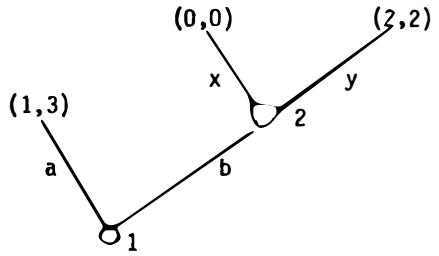
The V-solution may not exist, as Lucas (1969) showed, but the more typical difficulty is one of nonuniqueness. In the particular example above, it turns out that every alternative is a member of some V-solution so that the process is tautologically characterized by a V-solution concept of equilibrium (no evidence could be adduced from this example for the proposition that non-V-solution forces were at work). More to our point, however, is that either of the V-solutions in the example is attractive but not retentive. The process is always at a V-solution, as noted above, and always heading for the other, unless the process hits the core, x_5 (which, if it exists, is always part of every V-solution). Indeed, in our example, unless at x_5, the process will bounce between V_1 and V_2, reflecting the cycle among x_1, x_2, x_3, x_4.

These remarks extend to any collection of alternatives cum equilibrium. They attract but they do not retain. Indeed they may not even attract if local cycles are present. In the earlier example, if we add x_6, x_7, x_8, which cycle among themselves but each of which is dominated by $x_1, \ldots, x_5$, it is possible to observe the process caught in a local cycle $x_6 \to x_7 \to x_8 \to x_6$ -- with neither V_1 nor V_2 ever being reached. Thus we arrive at equilibrium concepts which "sometimes" attract and do not retain, hardly a firm basis on which to predict, explain, or control.

Frustration with the V-solution convinced John Nash (1951) that noncooperative, unilateral behavior was both more basic and offered more promise for the formulation of fruitful equilibrium notions. But are noncooperative equilibria any more successful in attracting and retaining than their cooperative counterparts? A Nash equilibrium consists of a collection of individual choices, that jointly produce an outcome, in which

which no individual, acting independently, has any incentive to alter his choice unilaterally. The cooperative generalization of this equilibrium is the core, in which no coalition of players has an incentive to change their individual choices jointly. Like the core, and subject to the proviso of noncooperation, the Nash equilibrium retains but does not necessarily attract. If a social process arrives at a Nash equilibrium, then it will not depart since unilateral behavior -- the only kind allowed in this context -- will not support any change. But do individuals have any incentive to play their Nash strategies? Not necessarily.

First, and perhaps of only minor importance, is the problem of imperfect Nash equilibrium. Imperfect equilibria, in Harsanyi's (1978, pp. 50-51) words, "assume highly irrational behavior [emphasis included] on the part of some players, yet they fully satisfy the mathematical definition of an equilibrium point." He illustrates this idea with a two-person example in which, in the extensive form, player 1 chooses a or b and, if he chooses the latter, player 2 then selects between x and y. The game tree is



There are two Nash equilibria, easily identified in the normal form:

		2: x	2: y
1	a	1,3	1,3
	b	0,0	2,2

Both (a,x) and (b,y) satisfy the mathematical definition of a Nash equilibrium; yet (a,x) is imperfect. As a glance at the extensive form provides, it depends on player 1 choosing irrationally[2] inasmuch as he can reasonably assume player 2's self-interest will lead him (Mr. 2) to select y if 1 selects b. And, since 1 prefers (b,y) to (a,x), the latter does not attract. The bottom line, then, is that some Nash equilibria are inaccessible by rational choice unless a social process should find itself in that state, ex ante.

A related, but more serious criticism addresses the incentive problem directly. To make this point, we consider the quintessential noncooperative circumstance -- the two-person zero-sum game. We offer the following partition of this class of games and speculate that the reader will find the Nash equilibrium established by von Neumann's Minimax Theorem an increasingly less persuasive equilibrium concept as he or she descends the list:

(1) Both players possess dominant strategies

(2) Exactly one of the players has a dominant strategy

(3) There are no dominant strategies, but there is a pure strategy equilibrium point and

  (i) one player believes the other will play a security level maximizing strategy

  or (ii) one player believes the other believes the first will play a security level maximizing strategy

  or (iii) there are higher-level conjectures about beliefs as in (ii).

(4) There are no dominant strategies, there is a pure strategy equilibrium point, but (i) - (iii) in (3) do not hold.

(5) There are no pure strategy equilibrium points.

Class (1) situations are hardly games at all since each player has a maximal element of his choice set unaffected by the choice of the other player; the resulting equilibrium is both attractive and retentive. Class (2) games differ only slightly. They require the same modicum of intelligence in players as do class (1) games regarding dominant strategies, viz. select them when they exist. In addition, they require in players a capacity to recognize when the other guy has one. Again, the attraction and retention power of this equilibrium point is strong, though perhaps weaker than its predecessor class. For class (3) the attraction is weaker still since it is contingent on fairly specific beliefs of one player about the choices or about the beliefs of his opposite number. By class (4) there is only a flicker of attraction left and the persuasiveness of the Nash equilibrium is probably strong only in situations involving incredibly cautious players. In class (5) there is neither attraction nor retention (except in a very weak form) since, if one player believes the other is playing his "optimal" mixed strategy, he (the former) is nevertheless free to choose any of his pure or mixed strategies: all have the same expected value.

Noncooperative equilibria, we believe, suffer defects similar to those we discussed in the cooperative realm. Their persuasiveness, especially regarding their attracting power, depends upon highly contingent circumstances of play. And the only way to determine whether these circumstances are reasonable to assume or are likely to transpire is to look at the world. There may be no accounting for tastes, as the old Latin expression tells us, but the necessity of some accounting, of an empirical sort, regarding attitudes, aversions, beliefs, or whatever becomes apparent -- a point to which we return in our concluding section.

We are not yet finished with our critique of equilibrium concept since we have not yet discussed some areas of discretion available to actors that are normally stipulated to be exogenous in many equilibrium theories. To begin this discussion we note that economists generally regard theirs as a science of voluntary exchange in which the "hold harmless" rule applies. Accordingly, the choices by individuals to buy and sell, produce and consume, spend, save, and invest are regarded as voluntary acts that reflect individual assessments of their well-being. Ex ante, the individual believes his cricumstances will have improved, ex post, if he buys or sells or . . . compared to what it would be if he did not.

Politics on the other hand is, in Riker's (1980) view, a considerably more dismal affair since it transcends the voluntary and is not constrained by the "hold harmless" rule. There are winners and losers, and losers lose not only in the sense of bearing opportunity costs, but often in the sense of being denied property rights, freedom, or even their lives. Under these circumstances (and we are not convinced it is that much different in economics) losers have strong incentives to alter undesirable equilibria. And this possibility is clearly feasible if the equilibrium in question is nonretentive. Somewhat surprisingly, even if an equilibrium is "well-behaved" in terms of attraction and retention, the theoretical framework in which this obtains often has held constant a number of behavioral dimensions which, in fact, are variables.

In economics, the notion of voluntary acquiescence or participation in economic activity is reflected in the ability of an individual to pick up his marbles and go home. If the terms of trade are unfavorable, he need not participate. In politics, this same option is (sometimes) available, though it is normally not accorded great import, in the form of

emigration. To "avoid harm," an individual may resign from a club, withdraw from a school, organization, or private society, or move to a different political jurisdiction. And emigration, broadly construed, is one of the more extreme measures by which individuals may destroy an otherwise prevailing equilibrium. It may be argued, of course, that if emigration is an available choice, then the so-called equilibrium is not really an equilibrium at all.[3] And this is precisely our point. Nevertheless, how many equilibrium theories begin "Consider a set $N = \{1,\ldots,n\}$ of players..."? The context of most equilibrium theories is one in which the set of actors is fixed so that a political outcome or decision must be endured by all.

Even if we stipultate that N is fixed, however, there still may be the means by which to upset an equilibrium. They include generating new alternatives or destroying existing alternatives (that is, not requiring the set A to be fixed and immutable), altering the dimensions of choice (fixing A but changing the basis for evaluating or just thinking about its elements), changing the rules of choice (allowing the dominance relation, $\succ$, defined on AxA, to change or to be determined endogenously), or moving the decision to some new arena of choice. Each of these strategies for upsetting some prevailing equilibrium is commonly observed in everyday political life. This suggests that even the prospect of an undesirable equilibrium at one level provides the incentives for losers under that equilibrium to agitate for some form of change in the institutional fabric. In sum, even under the most convincing of our equilibrium concepts there exist features of any political or economic situation which are subject to strategic exploitation. No law of logic requires that losers accept their status as losers, and empirical observation tells us that some do not.

This brings us to a related point, the final one we shall raise about the fragility of social science equilibrium. Most of our concepts are highly static -- even examples of dynamic equilibria presuppose a considerable amount of constancy. We take as given the set of actors, the set of alternatives, and the distribution of preferences. But just as losers have incentives to attempt to alter such initial conditions, so, too, may exogeneous influences change those conditions. Even if it were in strong general equilibrium, we daresay that the U.S. economy would be perturbed dramatically by the sudden invention of cars which run on water. Or to take an example from the political realm, even if the early 19th Century Democracy were a reflection of a majority rule equilibrium (who knows?), how can we predict the kind of evangelical protestant revivalism which swept the Midwest in the ante-bellum period and made the slavery issue so much more potent in the 1850s than it had been a generation earlier? Or to take a more short-term example, how do we predict the swings in popular preference which lead to a heavy economic emphasis in mass voting behavior in the late spring of 1980 after a heavy foreign affairs emphasis in the winter and early spring? Even the chimerical x* introduced at the beginning of this section is woefully dependent on the absence of such changes.

Thus, for any number of reasons, we conclude that social science equilibrium concepts, examined in an abstract context, fall short of providing the kind of predictability desirable in scientific theories.

3. Do Disequilibrium Results Dash Hopes for a Science of Politics?

Having argued that social science equilibrium theories often fail to provide the predictability desirable in a scientific study of politics, we will now argue the obverse, namely, that the absence of such equilibria does not preclude the desired predictability. Our argument hinges on the simple distinction between "equilibrium" in the world, and equilibrium in our models. Conflation of the two is an understandable occupational hazard, but that does not excuse the confusion.

Models provide partial descriptions of phenomena. A model which gave a complete description would be identical to the phenomenon of interest and therefore of little use to us. Thus, all models involve a restricted focus, a choice to examine or emphasize some aspect(s) of a phenomenon but to ignore or deemphasize others. This much is old hat (PS 401 at the University of Rochester). But the obvious implication is often overlooked: because models provide only partial descriptions, several can be applied to the same phenomenon and the resulting inferences may differ, even conflict. Ordeshook (1980) provides a simple example. Several plausible models applied to a simple market of one seller and two buyers produce several rather different predictions. A classical economic model predicts that the seller captures all the added value from exchange, while a game theoretic model predicts all the Pareto optimal allocations.

Even more interesting in our view are situations in which a disequilibrium inference from one model stands opposed to an equilibrium inference from another. In The Theory of Political Coalitions, for example, William Riker questioned the advisability of modelling political situations as games and of searching for possibly nonexistent or infinitely large

V-solutions (1962, pp. 36-39). His suggestion was to look at a different aspect of political situations, namely the coalitions which support the winning outcomes rather than the outcomes themselves. Riker believed that disequilibrium at the level of outcomes was compatible with equilibrium at the level of coalitions:

> It may be, of course, that the reasonable outcomes in an n-person model or an n-person real situation are in fact so numerous and diverse that systematic analysis and prediction is impossible. But it may also be that game theorists have not asked the questions most useful to social scientists and that by exclusive emphasis on the attempt to delimit admissible imputations they have overlooked the possibility of delimiting coalition-structures directly. (1962, pp. 38-39)

Another example comes from the article by Ferejohn, Fiorina, and Packel (1980). After noticing that the nihilistic McKelvey et al. result did not appear to describe the dynamics of experimental processes in which cycling and instability had free room to operate (Fiorina and Plott, 1978), these authors proposed a Markov model which provides a limiting probability distribution over the feasible outcomes of a majority decision process. Again, this work involves a shift in focus from past studies of majority decision making. Rather than examine the question of existence of stable outcomes, it attempts to examine in a systematic way the relative "difficulty" of moving from one (typically unstable) point to another, and the constraints such relative difficulties might place on the majority decision process.

As a third example, consider several recent papers by Kramer (1977, 1978). Instead of considering an election as a discrete decision in which the McKelvey result is known to apply, Kramer embeds each electoral

decision in a time sequence of elections. This approach yields inferences other than "anything is possible." To wit, the model implies that any particular outcome is part of a "trajectory" that leads to the minimax set, which in this case is likely to constitute only a small part of the feasible set.

As a final example of the simultaneous existence of disequilibrium and equilibrium inferences, consider Shepsle's (1979a,b) work on structure-induced equilibrium (the product of both tastes and institutional arrangements). Shepsle establishes conditions under which structure-induced equilibria exist while the set of preference-induced equilibria is empty. This work follows in the spirit of Duncan Black who in his early paper on the unity of political and economic science observes that "equilibrium in Politics is 'the resultant of tastes and obstacles'; and these are the words Pareto used of equilibrium in Economics." (1950, p. 118). "Obstacles" is Black's nomenclature for the forms of committee procedure that, in his view, combine with the preference scales of committee members to determine formal decisions. It seemed obvious to Black that political equilibria were inextricably linked to institutional arrangements which constrain political processes, just as economic equilibria are linked to often implicit institutional arrangements which constrain economic forces.

In discussing the preceding examples, our purpose is not to assert that coalition models, stochastic models, dynamical models, or institutionally rich models are sure-fire means of exorcising the spectre conjured up by McKelvey and friends. Rather, our purpose is to establish that the existence of equilibria has as much to do with the choices made by the

scholar as with the characteristics of the phenomenon he or she is studying. We describe and comprehend empirical phenomena through the lenses of particular models. And the particular lenses we use are partially at least, a matter of choice.

Continuing with this argument, it seems to us that our colleagues in Economics have deliberately chosen a research program different from that embraced by the younger generation of positive political theorists. As members of interdisciplinary "shops" we have both attended numerous economic theory seminars. In reflecting on these, it seems to us that they generally proceed under the constraint that only equilibrium-preserving extensions of models are of interest (i.e., publishable). The typical budding theorist adds to or generalizes an existing model and makes his or her personal contribution by showing that with the given addition or generalization an equilibrium continues to exist. When questioned as to why the modification or generalization was not done in some other way, theorists typically respond that the suggested alternative entailed either intractable problems or that no results could be established under the suggested alternative, i.e., in either case the alternative was "uninteresting."

In contrast, McKelvey, Schofield and other political theorists have followed a path blazed by "Arrow's Mathematical Politics" (note the choice of terminology by an economic theorist, Paul Samuelson) through the "impossibility" terrain of social choice theory. Positive political theory did not always follow this path. In the early development of spatial models, the emphasis was on equilibrium results. The basic model was extended to different voter distributions, abstention was introduced, and sequential elections were considered, but always the

symmetry conditions which would produce equilibrium were imposed. Why this research program was abandoned is not clear; perhaps those involved became convinced that reasonably interesting equilibrium models of political situations constituted a set of measure zero. But it is not obvious that the situation is any different in economics.

In fact, it is not outrageous to speculate about alternative scenarios. Political theorists might have decided early on that unidimensionality was a basic assumption of all political models, akin to the regularity conditions imposed on the consumption set by economists. Alternatively economists might have followed up Scarf's (1960) examples of instability which led Nikaido (1969, p. 337) to observe that "global stability is so special a dynamic property that contrary to the Walrasian view, one can hardly expect it to be shared by all competitive economies... [I]nstability seems to be a universal phenomenon in competitive economies, rather than an exceptional one, whereas global stability is expected to prevail only in very well-behaved systems." Perhaps some of the differences between economic and political theory arise less from the greater instability of political phenomenon than from the attraction of stability-loving personalities to economics vis-a-vis the gravitation of chaos-loving personalities to political science.

If equilibrium is necessary for scientific prediction and explanation, and lack of equilibrium is fatal for those activities, it would seem that economics is scientific because its practioners have chosen to be whereas political science is not because its practioners have chosen not to be. In that case, the route to science is clear: we can choose to be scientists. This answer is facile, however. It is our belief that the common element in the choices of both the economic and political theorists is that their

theoretical choices arise from considerations mostly unrelated to and uninformed by the real world. Some attention to the latter provides the grounds for a "third way" to a science of politics, an old way too often overlooked by economic and political theorists.

4. Conclusion and Modest Proposal

Our tentative conclusions are several. First, most social science equilibrium concepts are, at best, distant cousins to what we have termed "black hole" equilibrium. Specifically, they rarely are so conspicuous, so centripetal, or so retentive as their physical science ideal. And even when they are both attractive and retentive, they are embedded in a larger net of institutional and social relationships which, themselves, are not immutable. Outcomes, whether equilibria or not, distribute gains and losses. Losers may not be able to replace a prevailing outcome with one more to their liking, but surely they may agitate for change in the broader institutional matrix and, when successful, destroy an earlier outcome, whether an equilibrium or not. In consequence, the link between equilibrium and scientific predictability is both weak and tenuous.

The same may be said about the link between disequilibrium and unpredictability. Disequilibrium, unpredictability, and chaos are certainly possible at some levels of political description. Indeed, they are generic and all-encompassing at the level of outcomes, if the theorems of Cohen, McKelvey and Schofield serve as plausible descriptions of majority rule. But notice the qualifications. First, we might observe in a committee's decisions over time no apparent pattern as it moves hither and yon through Euclid's space. Yet we might also observe that the decisive coalition each time, though different in composition, always contained no more members than necessary. Alternatively, we might discover that the changing decisions

of the committee were perfectly associated with the ideal point of the chairman, whose identity rotated among committee members over time. (More incriminating, still, we might witness each chairman having a drink at the bar with Dr. McKelvey just prior to his committee's deliberations!). In each of these instances, disequilibrium, chaos, and unpredictability, at one level are transformed into predictable regularities, explicable in terms of rationality and equilibrium, at some other level of conceptualization.

There is a second important qualification to the interpretation of a disequilibrium result like those of Cohen, McKelvey and Schofield: it may not constitute a plausible description, even at the conceptual level at which the disequilibrium is established. Equilibria and disequilibria are properties of models. It remains to be demonstrated whether they are descriptive of empirical phenomena. One of the objectives of the Fiorina-Plott experiments was, in fact, to discover whether a host of ideas bearing on equilibrium and disequilibrium were empirically plausible under the best of experimental conditions; many failed their test.

While we have hardly "proved" our dual conclusions that equilibrium models in the social sciences (including economics) are less than wholly persuasive for, and disequilibrium results less than wholly inimical to, a science of social phenomena, we think these conclusions rest on a solid base and would be agreed to by reasonable men. Arguably more controversial are several related points. The first is that an equilibrium concept should be regarded as a conceptual invention -- the property of a model, not of the world of phenomena. As a consequence, scholars have some degrees of freedom in choosing levels of analysis, models, and equilibrium concepts. And the usual philosophy-of-science criteria apply in this choice and in

the evaluation of the resulting scientific product. In our view, a model without equilibrium constitutes no more of a scientific improvement in the state of knowledge than wholly complete description of the phenomenon in question. Each suffers the debility of failing to inform. (Parenthetically, however, we admit that the discovery of disequilibrium serves the same constructive purpose as a "detour" sign; it cautions the traveler about trouble ahead and may even urge that an altogether different route be contemplated). Each fails to isolate that which is regular and hence understandable (so-called "complete descriptions" fail in that they do not discriminate regularities from idiosyncracies and other attendant circumstances).

So much for critical commentary. But what do we offer by way of positive recommendations? Our position is that scientific progress reflects (a) the scholarly choice of models which (b) possess equilibria which (c) correspond to observed regularities. This entails neither constructing equilibrium models ex ante, generalizing and refining subject to the constraint that equilibrium be preserved (the path traveled by most general equilibrium theorists in economics), nor retaining disequilibrium models only to be tongue-tied when asked to say something positive about the world of phenomena (the path recently traveled with seeming relish by some political theorists). To travel the first path is to say little that applies to the world of phenomena, and to travel the second is to say little, period. Instead, we recommend a third path, one termed "retroduction" by Charles Peirce.

As exposited by Goldberg (1968) retroduction emphasizes the construction of theories, but it similarly emphasizes the importance of empirical regularities in that process. Put simply, the retroductive process begins with an empirical regularity, X, and poses the question "How

might the world be structured so that X is an anticipated feature of that world?" The answers (and there should be several) are models, all of which have in common the regularity X as a logical implication. We understand that most theoretical work resembles the retroductive process in that pure deduction seldom occurs; usually some desired result determines the choice of premises. What we are saying that is different is that the desired result should be based primarily on empirical regularity (at least on "stylized facts"), rather than on its strength, neatness, or other aesthetic criteria. Thus, regularity in the world should motivate scholars to construct the theoretical worlds in which that regularity exists. Construction of a world without regularities constitutes a failure, not an achievement, though as we have earlier noted, such failures may serve a useful purpose in identifying paths not worth pursuing and in suggesting enrichments by which to augment disequilibrium models in order to accommodate observed empirical regularities. Indeed, the central constructive feature of the Cohen-McKelvey-Schofield theorems is precisely that "other features," not the majority rule mechanism, are decisive in democratic institutions.

FOOTNOTES

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1. Most mechanisms of dynamic adjustment in general equilibrium theory are highly artificial "stories" -- artful fictions. The classic mechanism, of which there are several variations, is the tâtonnement, a process in which a mythical market auctioneer calls out a vector of prices, observes the plans rational economic agents intend to follow subject to those prices, computes excesses of supply and demand, and then announces a revised price vector according to some adjustment rule. The actual implementation of economic plans is permitted only after this price adjustment process converges to an equilibrium. Arrow and Hahn (1971), after two chapters of discussion and results on dynamic adjustment in

their treastise on general economic equilibrium, are quite frank in their appraisal of tâtonnement:

> Some of the difficulties we have encountered may be due to the abstraction of a tâtonnement; this will be discussed in the next chapter. Even if it had been possible to show that in a perfectly competitive economy a tâtonnement is always stable, it is not clear that such a result could have been given much weight in forming a judgment of the performance of the price mechanism in actual economies. The fiction of an auctioneer is quite serious, since without it we would have to face the paradoxical problem that a perfect competitor changes prices that he is supposed to take as given. In addition, the processes investigated in this chapter assume that, disequilibrium notwithstanding, there is only a single price for each good at any moment. It is also postulated that at each moment, the plans of agents are their equilibrium plans. Lastly, of course, there is no trade out of equilibrium. All of these postulates are damaging to the tâtonnement exercise. It may be that some of the theorems and some of the insights gained will have application when a more satisfactory theory of the price mechanism has been developed. At the moment the main justification for the chapter is that there are results to report on the tâtonnement while there are no results to report on what most economists would agree to be more realistic constructions. (Arrow and Hahn, 1971, pp. 321-322. [emphasis added]

2. In some models it would not necessarily constitute irrationality for player 1 to believe his adversary may deliver on a self-damaging threat.

3. Assume for a moment that political science had developed theories built around a "black hole" equilibrium concept, and that past experience had shown such theories to be devilishly accurate predictors of future states of society. Then the very act of making a prediction could be the stimulus for a mass emigration

to Canada and/or Mexico, or even a violent revolution in the United States. If sufficiently powerful (i.e. credible) a social science theory could provide human actors with the incentive to change the ceteris paribus conditions on which the theory's predictions depend.

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