

1991

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Pierre Salmon

Alain Wolfelsperger

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Citation of this paper:

Salmon, Pierre, Alain Wolfelsperger. "From Competitive Equilibrium to Democratic Equilibrium: Has the Analogy Been Fruitful?" Political Economy Research Group. Papers in Political Economy, 16. London, ON: Department of Economics, University of Western Ontario (1991).

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FEB 15 2000

Paper No. 16

**“From Competitive Equilibrium to
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**Pierre Salmon and Alain
Wolfelsperger**

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Political Economy Research Group,
Department of Economics,
Social Science Centre,
London, Ontario, Canada N6A 5C2
phone: (519) 661-3877
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**From competitive equilibrium to democratic equilibrium:
Has the analogy been fruitful? ***

Pierre Salmon

Université de Bourgogne
Faculté de Science économique et de Gestion
4 boulevard Gabriel
F-21000 Dijon (France)

and

Alain Wolfelsperger

C.R.E.S.E.P. (Université d'Orléans) and
Institut d'Etudes Politiques de Paris
27 rue Saint-Guillaume
F-75341 Paris Cedex 07 (France)

I. Introduction: From democratic equilibrium to disequilibrium

Duncan Black concludes as follows his well known 1948 article on voting: "The theory, indeed, would appear to present the basis for the development of a pure science of politics. This would employ the same theory of relative valuation as economic science. It would employ a different definition of equilibrium. Equilibrium would now be defined in terms of voting, in place of the type of definition employed in economic science. We could move from the one science to the other with the alteration of a single definition" (p. 146). These ideas are elaborated in a less well known paper entitled "The unity of political and economic science" (1950).

The equilibrium referred to by Black corresponds to the alternative (e.g. motions, candidates, platforms) which would be able to get at least as many votes as any other in pairwise comparisons, i.e., which would win or at least ties (the "Condorcet winner") in a procedure consisting of setting each alternative against each other in a vote by simple majority rule (the "Condorcet procedure"). The analogy between that concept of equilibrium - hereafter "democratic equilibrium"- and competitive equilibrium seems striking. In both cases equilibrium corresponds to a state of the system in which endogenous variables remain constant so long as nothing happens on the side of exogenous variables. But both equilibria are Nash equilibria and the analogy goes deeper. Competitive equilibrium refers to an allocation which cannot be changed by agents in a mutually acceptable way, and thus the concept seems in perfect agreement with the basic principle of a market economy. "Democratic equilibrium" refers to an alternative which cannot be changed by a majority of voters and is thus in perfect agreement with what appears to be the basic principle of a democratic society.¹

The programme suggested by Black has two main components. First, majority-rule equilibrium, self-evidently put at the centre of the analysis and equated with "democratic equilibrium". Second, a methodological perspective inspired by an analogy between the nature and function of "democratic equilibrium" in political science and the nature and function of competitive equilibrium in economics. It is certainly not the case that all the researchers in economics and political science who have worked on majority-rule equilibrium have adopted the programme, but, to various degrees of explicitness or self-consciousness, it seems clear that many have. In addition, such a programme has been perceived from the outside, in particular by political scientists unfamiliar with public choice, as central to the economic approach to politics. Hence, we may talk of a "Blackian research programme", characterized by the two features above, and may consider it as one of the important trends in public choice over the last four decades.²

Such importance, however, is somewhat surprising, given the fact that the main objection to Black's suggestion could have been found in a manuscript that he himself completed, together with R.A. Newing, one year only after the publication of his analysis in 1948 and which they published as a monograph in 1951 (pp. 21-22). The idea - that it is likely that equilibrium will not exist when choice involves more than one dimension - is stated more clearly in Black (1958, chapter 16) and rigorously by Plott (1967), but it is only after intense shelling, in the form of a long series of theorems,³ and after the finding by McKelvey (1976) of a nicely ominous word for it - chaos - that the profession has become fully aware of its implications.⁴

What makes cycling (disequilibrium, instability) so serious in the multidimensional case? First, in that case, the existence of a Condorcet winner should be considered as really exceptional.⁵ Second, when a Condorcet winner does not exist, any alternative within the feasible set can be reached by the Condorcet method of pairwise comparisons; cycling is not limited to a subset. In the words of Fiorina and Shepsle (1982, p. 49): "When majority rule breaks down, it breaks down completely; and it 'almost always' breaks down". Third, the cycling theorems are robust under a variety of assumptions or in a variety of theoretical frameworks (e.g. logrolling, game theory).⁶ Fourth, although "chaos" is apparently compelling in theory, it is not really detectable in reality. This is good news for democrats, but necessarily troublesome for a theory which has gradually become to a large extent a theory of democratic disequilibrium or instability.⁷

A fifth reason for the seriousness of cycling could have been formulated without reservations until recently. It seemed clear that the negative results derived in the context of committees should also apply to electoral competition (platforms being obviously multidimensional as a rule), and thus affect the theory of representative democracy built on the (unidimensional) model of Downs (1957) and the extensive empirical work based on the model of the median voter. Hence, the whole set of democratic institutions could appear as condemned either to perpetual instability or to arbitrariness (if the time set for voting is finite). But this may have changed. The theory of probabilistic voting, starting in the early seventies but becoming really explicit in the early eighties (Coughlin and Nitzan, 1981), offers a solution to the cycling problem when "sufficient" uncertainty can be assumed - e.g. in the Downsian context of two-party competition. According to Mueller (1989, p. 214), that theory "drives a giant wedge between the public choice literature on committee voting and that on electoral competition" (the same opinion is formulated by Enelow and Hinich, 1984). We consider the probabilistic voting approach as very promising indeed. Still, we do not feel sure that so far either the implications of the assumptions on which the available results rely or their exact consequences on the

existence, nature and relevance of equilibrium have been analysed sufficiently in depth for Mueller's view to be quite compelling yet. Whether, if equilibrium exists, it is close or not to something like a median voter position, what is exactly the level of uncertainty which determines the proposed dividing lines between the world of committees and the world of elections, how it is that uncertainty can be brought in both to derive disequilibrium when otherwise equilibrium exists and might be reached and equilibrium when otherwise disequilibrium would obtain, or even whether an equilibrium position is important at all under probabilistic voting (see note 25), these and others are the questions whose answers are still not perfectly clear to us. Anyhow, probabilistic voting is glossed over in our discussion of "democratic equilibrium" (as well as other possible departures from the assumption that each voter votes for the alternative that is the closest to his or her ideal position; see, e.g., Nurmi 1987).⁸ We are conscious of the fact that this neglect might entail some limitation of the empirical domain to which that discussion applies.

Some researchers are not really worried by the cycling issue. Holcombe (1989, p. 123) argues that "there is a large amount of theoretical and empirical evidence supporting the median voter model as a good foundation for the development of the theory of public sector demand." Others take it more seriously. Ordeshook (1980, p. 450) expresses the view that "twenty years of research compel us to conclude that traditional definitions of equilibrium are unsatisfactory, or at least that they are insufficiently general", "that theorizing about [democratic processes in particular and political processes in general] requires developing new concepts", and "that the optimism of the past about the ease with which the economists' paradigm could be transplanted in politics must give way to the realization that political scientists themselves must contribute to the development of that paradigm." The conclusion of Riker (1980, p. 443) is simultaneously less and more ambitious: One should not attempt to find a new concept of equilibrium but admit that "disequilibrium... is the characteristic feature of politics."

A common trend in the response of most specialists to the theoretical problem raised by disequilibrium has consisted in engaging into comparisons between the concepts of "democratic equilibrium" and competitive equilibrium. In this they have remained faithful to a major characteristic of the Blackian programme as defined above. But, as a result of the chaos results, the comparisons have tended to become mainly defensive. As everybody knows, competitive equilibrium raises many problems but economics has found various ways to live with them. Why could not comparable solutions or treatment be applied to "democratic equilibrium"? If competitive equilibrium is so central to economics despite its well-known shortcomings, why could not also "democratic equilibrium" remain central to the theory of democracy in spite of the cycling results?

In Section II, we argue that this kind of defense, although clearly convincing to many economists and political scientists, is basically flawed. The nature of "democratic equilibrium" and the role the Blackian programme gives this concept in the study of politics are different on essential points from the concept of competitive equilibrium and the function it plays in economics. We argue in Section III that the two main directions that have been followed to amend "democratic equilibrium" within the Blackian programme mitigate the disequilibrium results but do not really address the issues raised in Section II. In Section IV, we plead for a different analogy between politics and economics, one that focusses on organisations, and note that this line of thought is consistent with other main trends in public choice. The conclusion summarizes our answer to the question included in the title of the paper.

II. From competitive equilibrium to "democratic equilibrium": what has been lost

In the quote above Ordeshook expresses the usual objection against transfers of concept from one field to another: Adaptation to the new setting has been insufficient. We argue here that the main problem is the opposite one: The positive analogy between the two concepts of equilibrium has remained superficial. As used in economics, competitive equilibrium has important characteristics that are not to be found in "democratic equilibrium". Three differences are particularly worth stressing. First, while the concept of equilibrium is derived from the analysis of processes in the theory of competition, in Blackian models it defines the process. Second, contrary to competitive equilibrium, "democratic equilibrium" is inspired by normative considerations, which explains that it generates models ill-suited for positive analysis. Third, the empirical fruitfulness of competitive equilibrium models can be assessed in the context of comparative statics while the empirical assessment of "democratic equilibrium" models relies on simple statics.

First difference: The relation between equilibrium and processes

How can one say that the concept of competitive equilibrium is derived from an analysis of processes? Such an assertion may seem surprising to contemporary economists. To explicate it, let us make a distinction between what we may call here a theory and its models (both words have other meanings, of course). The theory here is a somewhat loose and general analysis (as typical for instance in sociology) of some aspect or mechanism of the real world. The models are tight, often formalized, interpretations of the theory (cf. Mingat, Salmon, and Wolfelsperger, 1985; Salmon 1987a). Thus, the theory of competition purports to describe the nature and consequences of market processes of a particular type - i.e., information-seeking,

negotiation and exchange relations, relative to "close" (substitutable) products, between fairly numerous and small agents, and involving small transaction costs. The main result from this theory is the variations in prices. The variations are related to the differences between the quantities demanded and the quantities offered at the prices that obtain initially. As a consequence, when there are no more differences, there are no more variations, and one can speak of equilibrium. But such a situation is a limit one, that obtains if no further change occurs in the parameters. Being certainly not more likely to be observed in practice than disequilibrium, it does not deserve particular attention for the purpose of explanation or description.

In the course of their teaching, research, or reflexion on real-world issues, economists often have such a theory (or something close to it) in mind. It is a very illuminating theory, applicable in many areas, probably (in competition with the partial-equilibrium model considered shortly) the most enduring contribution of economics to the understanding of society. But present-day economists keep the theory for themselves and put forward models of competition. Although the models are usually presented in the shadow of the theory, that is not said. Consequently it is hardly surprising that outsiders do not perceive the existence of the theory and conflate the whole analysis of competition with the set of models. In doing this, they neglect a dimension of the analysis which is essential for its methodological interpretation.

Contrary to the theory of competitive markets, the model of partial equilibrium focusses on the specification of the equilibrium and not on the processes that lead to it. The variations in price are now expressed as a consequence of the displacement of equilibrium, and not the other way round. But, let us insist, that is a feature of the model, to be explained by the latter's purpose, not of the theory. In general equilibrium models, the focus on equilibrium is reflected by the importance given to existence as compared to stability theorems. Still, the recent literature on competitive bargaining, e.g., shows that here also economists have remained aware of the need to derive the equilibrium from the processes rather than the contrary.⁹

The programme based on "democratic equilibrium" is clearly inspired by a different attitude. Black does not start, even implicitly, from a positive theory of the working of democratic institutions but with a model designed to display a particular concept of equilibrium. The analysis begins with the demonstration of an existence theorem. The reference to a particular process is purely instrumental to that task. For instance, he notes (1958, p.10) that "in practice in any committee never more than a few motions are put forward on any one topic", but then adds without further comment: " We may suppose, however, that an infinite number of motions are put forward and that each member of the committee values each of these motions in relation to every other." It is obvious here that we may because we must - if the concept of

equilibrium that is chosen is to make sense. The same logic can be found in the more recent literature on the derivation of disequilibrium theorems. To make sure that some characteristic of the procedure could not lead to the occurrence of equilibrium, the procedure is defined in a completely abstract way - a perfectly random order of presentation of all the possible motions, typically.¹⁰

Second difference: the impact of the normative on the positive

If the concept of "democratic equilibrium" is not derived from an analysis however rough or implicit of a process, where does it come from? In fact, its origin is not mysterious. If we consider both the "will of the people" and the "principle of majority" as consubstantial with the democratic ideal, a motion selected by majority rule could hardly be said to reflect the will of the people if it is the case that recourse to the same rule could have resulted in another motion being preferred. Whatever the order of presentation of the motions, this cannot happen to the Condorcet winner, which consequently stands out as the only motion fulfilling what appears to be an essential normative condition. It also corresponds to equilibrium, but here equilibrium itself has a strong normative connotation - what would become of the "will of the people" in the case of cycling? The process which is referred to, i.e. the Condorcet procedure, is only a necessary ingredient of the solution.¹¹

All this may be quite acceptable from a normative point of view - though the "will of the people" and "principle of majority" approach has always been known to lead to various paradoxes and is rejected by, e.g., a philosopher like Popper (1945, chapter 7).¹² But what we are interested in here is positive analysis. Although inspired by normative considerations, and in spite of an obvious discrepancy between the assumed process and the observable working of democratic institutions, can we use "democratic equilibrium" as a central building ~~stone~~^{block} for the purpose of positive analysis, as implied by the Blackian programme? Our answer to that question will be largely negative. This will need elaboration because two methodological considerations could suggest a positive answer. First, is not the source or cause of a theory irrelevant to the assessment of its validity or fruitfulness? To a large extent, we accept this. The only purpose of the foregoing discussion about the origin of the concept of "democratic equilibrium" was to provide an (admittedly quasi-sociological) explanation of the tenacity displayed by the researchers working within what we have called the Blackian programme.¹³

The second consideration is the more important one. Is not the discrepancy between analysis and reality of comparable size in both fields? By analogy with the way competitive equilibrium is often interpreted in economics, we may consider "democratic equilibrium" either as a polar case, empirically relevant for the analysis of the real-world institutions which rely on

procedures that depart as little as possible from the Condorcet procedure, or as the only concept currently available for the purpose of predicting the consequences of majority rule in all democratic institutions, whatever their particulars. The first alternative is favoured, e.g., by Holcombe (1989), who thinks that, starting from the polar case, one could, by reintroducing in the analysis various "imperfections", build in the course of time models that would describe more complex processes and thus be better suited to the analysis of real-world democratic institutions. Inman (1978), among others, argues in favour of the second alternative. As is clear from the title of his paper, "democratic equilibrium", or the median voter, should be interpreted as an "as if" proposition. Models must be judged on the predictive and explanatory power of the propositions derived from them, not on the realism of their assumptions. To use a tripartite distinction suggested by Alan Musgrave (1981), Holcombe's position consists in using simultaneously a domain assumption (the model should be applied to some classes of phenomena only) and a heuristic assumption (the first version of the model will be followed by better ones), Inman's in using a negligibility assumption (a characteristic of the real world has no impact on the phenomenon to be explained). In previous work (Mingat, Salmon and Wolfelsperger, 1985, pp. 386-96), we have argued that scientific practice in economics consists often (but not always) in using simultaneously the three assumptions. If we are correct, positions such as Holcombe's and Inman's can be quite close in practice.

Thus, the typical reaction here is one that is familiar in economics. Models using "unrealistic" assumptions are acceptable provided they yield good predictions (and to some extent explanations) for a class of phenomena. We shall not discuss here the merits of this methodological position (see Salmon 1976, 1987a; Mingat, Salmon and Wolfelsperger, 1985; Mongin 1987, 1988). It implies that the most relevant question (to be addressed shortly) is empirical success but certainly not that all the other characteristics of models are irrelevant. There are features one may wish to find in any model purporting to contribute something to the analysis of the real world, whether that model has a good predictive record or not. By comparison with competitive equilibrium models, two such features seem badly lacking in the models based on "democratic equilibrium".

Let us consider first the relation between the assumptions of existence in the real world and in the model. For any hypothesis referring to a model, it would be an unrealistic assumption of an unusual kind that which would consist in supposing the existence in reality of as central a relation as equilibrium when that existence is precluded in the model. Now, we have seen that existence in the model is demonstrated to be very unlikely in the multidimension case. Fiorina and Shepsle (1982, pp.60-61) suggest that "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 and production sets by economists." But the two situations are not comparable. In the case of general equilibrium one seeks sufficient conditions for the existence of an equilibrium. In the case of "democratic equilibrium", as soon as one can observe directly that multidimensionality obtains, it is a necessary (and sufficient) condition which is defined (the existence of a median alternative in all directions), and one that is highly unlikely to be fulfilled. The number of dimensions is too clearly a crucial characteristic of real-world situations for unidimensionality not to be interpreted as a "domain assumption" (the conflation of Musgrave's three assumptions is impossible here). Outside its domain, the unidimension version cannot be used. We cannot assume that phenomena that involve more than one dimension can be analysed "as if" there were only one dimension because the model itself, in the form of the "chaos theorems", tells us that they will behave very differently.

Second, when they play an important role in the model, unrealistic assumptions should have at least a vague resemblance to the corresponding real-world relations underlying the phenomena considered. This second feature may be less indispensable than the first if one gives priority to the model's predictive power. But, it is certainly desirable, especially when predictive power is not warranted. Although the famous tâtonnement process in competitive equilibrium is unrealistic, it can be interpreted as a stylized or idealized description of the mechanisms relating variations in price to excess demand or supply, at least in the neighbourhood of equilibrium (Balasko 1988, p.30). Contrary to tâtonnement, the Condorcet procedure implies no restrictions on the path followed by the variable concerned. But, more seriously, it cannot be interpreted as a stylized or idealized description of a real-world process. It makes sense only if votes are numerous, while the paucity of voting is one of the basic characteristics of real-world committees or assemblies. To be unable to account for such a salient fact renders justification by predictive power unlikely. Of course, there is the problem of the auctioneer. That fiction is sometimes considered as necessary to make tâtonnement more precise, but we are not concerned here with precision. In the absence of an auctioneer, tâtonnement remains a stylized description of real-world mechanisms (Balasko, 1988). And the non-fictitious existence of an auctioneer in the case of voting, in the person of the chairman, gives no superiority to "democratic equilibrium" over "competitive equilibrium" because the chairman cannot be assumed to be concerned with finding the Condorcet winner.

Competitive equilibrium models, however "unrealistic", remain idealized descriptions of some aspects of real-world phenomena because they are themselves the outcome of an endeavour to understand and describe real-world phenomena. That can hardly be said of "democratic equilibrium" models.

Third difference: the role of simple and comparative statics in the empirical validation

One of the main justifications of the competitive equilibrium models is that they yield empirical predictions and explanations. The questions we want to answer with the help of the models, or on which we want to test their predictive power, come more often under comparative than under simple statics. Typically, they are of the kind "In what direction will the price and the traded quantity of a good change as a result of a given change in the exogenous variables?" rather than of the kind "How to explain the actual levels of price and traded quantity of such and such good?" That is why the partial equilibrium model, particularly well suited to deal with the first type of questions, seems more interesting to many economists than the general equilibrium model (except when the latter is simplified in such a way that it also can answer them). The main reason why economists have confidence in the partial equilibrium model is that it has proved its worth and robustness in this use. But there, in most circumstances, the equilibrium assumption, although logically necessary, can remain purely instrumental and carry very little information, in particular on preferences.

The role of "democratic equilibrium" in empirical work (e.g. on local finance) is quite different. Typically the model of "democratic equilibrium" is requested there to provide a prediction of the democratic decisions themselves. This is not impossible in principle. If the median position (in all directions, in the multidimension case) exists, and if the agenda ensures that the corresponding motion will be considered, the knowledge of that position is sufficient to predict the outcome of the democratic process. In practice, if the position of a voter can be assumed to be a median one in all directions, and if the decisions of the institution (e.g. a local government) that is studied are assumed to reflect the voters' preferences, it can be predicted that these decisions will reflect the preferences of the median voter. In a way, simple statics is more acceptable here than it would be in the case of markets. Since political decisions are usually more stable than market prices, equilibrium can be more easily observed (provided the conditions of applicability of the "democratic model" are fulfilled),¹⁴ the identification of equilibrium values is easier.¹⁵

Yet, three serious problems remain. First, while all the explanation rests with the preferences of the median voter, we have very limited information on these preferences. To cope with this, empirical work assumes the median voter to be the median income recipient. But the price for falling back in that way on familiar variables (income and tax-price) is the making of very strong assumptions (see Bergstrom and Goodman, 1973, who specify five sufficient conditions for identifying the median position with the voter with the median income). As a result of the assumptions needed here, the prospects of corroboration of the median voter would probably be jeopardized if its genuine testing did not meet the methodological obstacles

analysed by Romer and Rosenthal (1979). A second obvious problem is the almost certain absence of equilibrium in the multidimension case, entailing the impossibility of using the model to predict. Even when, by chance, an equilibrium exists, this obtains only with regard to the dimensions that have been specified. If another dimension is added (because it had been forgotten in reality or remained undetected by the observer, or as a result of a "Riker-move", i.e. a clever ploy used by potential losers), decisions with regard to initial dimensions may change substantially and equilibrium vanish (cf. Grofman and Uhlaner, 1985). The third problem arises when the procedure consists in voting on one dimension at a time. In that case, it is only when voters have separable preferences that the voting sequence will be irrelevant to the outcome. Without such a strong assumption, a prediction of the outcome will require knowledge both of preferences and of the agenda.

We see that the source of the three problems is the same. Although "democratic equilibrium" models rely on assumptions that are less robust than competitive models, more information is necessarily sought from them. That may explain why perhaps the only context in which "democratic models" can really be tested is one which we have not considered so far, i.e. laboratory experimentation. There, the competitive model is well corroborated (Plott 1982) but the "democratic equilibrium" does almost as well. When a Condorcet winner exists, the average outcome of final votes in a series of experiments comes quite close to it (Fiorina and Plott, 1978). But the experiments are organized in such a way that the subjects follow a very simple procedure which almost ensures that the Condorcet winner will be put forward and selected if the subjects are rational and selfish. One may wonder whether the experiments here are not more about rationality and preferences than about the outcome to be expected from using majority rule.¹⁶ Thus, although the experimental results are important, they cannot provide much support to the Blackian programme as defined in this paper.

III. From "democratic equilibrium" to other varieties of equilibrium: minimal fixing-up

Leaving aside methodological excuses and probabilistic voting, the response of research to theoretical disequilibrium-cum-observed stability has followed two lines. The basic model has been replaced by a number of institution-augmented models in which the concept of "democratic equilibrium" remains unchanged; or that concept has been replaced by some other, with no institutional structure added. It seems clear that in both cases minimal departure from the Blackian programme is sought. The objective is to derive or establish equilibrium with no profound reassessment of the processes that may lead to it.

Equilibrium constrained by institutions

According to that first approach the problem with the "democratic equilibrium" model is that its institutional content is too poor - Spartan is the word used by Shepsle (1986b). Equilibrium will exist when appropriate structures (i.e. rules or other institutions) are taken into consideration in the models. With agenda control, for instance, it is not too difficult to prove that an alternative exists that could have been defeated by no other among those allowed by the agenda (a subset of all those feasible in the standard model). In the enriched models, it is suggested, equilibrium is "structure-induced", as opposed to standard equilibrium, which is "preference induced". But this is not quite exact. With institutions added, what equilibrium becomes is rather "induced by structure-constrained preferences", with a different type of equilibrium for each kind of structure. The concept of equilibrium itself remains unchanged.

The construction of institutionally enriched models is certainly to be recommended. Still, now that (thanks to structures) equilibrium exists, how do we know that (and how) it will be reached? The question seems worth raising since the focus remains on equilibrium rather than on the process or path that may lead to it, on existence rather than on stability. To that familiar objection let us add two queries. First, the way structures are introduced in the models seems clearly *ad hoc*. Both unconstrained voting and constraints appear in the final analysis as theoretical constructs or artefacts that have been specially chosen or shaped in such a way that disequilibrium follows (as a rule) from the former and equilibrium from the latter. Constraints produce equilibrium by *construction*. To mitigate *ad hoc*ness, it would help if models could be tested. Although not done too often yet, this is not impossible in principle on the basis of a comparative statics analysis of the relation between differences in structures and differences in characteristics of the equilibrium solution.¹⁷ The main difficulty is that, although real-world democratic institutions have many features, existing models consider the effects of one at a time. Could that be otherwise? The demonstration, in general terms, of the existence and stability of equilibrium would become impossible or exceedingly difficult if the complexity of the institutional structure were to be acknowledged to a really large extent. More generally, substantial institution-enrichment could easily lead to a description yielding no prediction of any kind. It seems thus that the models discussed here are better suited to an abstract study of the consequences for equilibrium, under a *ceteris paribus* clause, of precise structures taken one at a time than to a more general attempt at understanding how real-world institutions achieve stability.¹⁸

The second question is how are the structures to be explained? Under methodological individualism, implicit here, it seems undesirable to invoke rules or other institutions without attempting to explain them (cf. Boland 1986). The logic underlying "democratic equilibrium"

suggests that they should also be viewed as the outcome of democratic processes, but then we must expect to meet the same obstacles on the higher, constitutional, level. Passing the buck from one level to the next one up may lead to infinite regress, obviously, but, if we follow Riker (1980), there may be an even more serious objection. Inasmuch as preferences for rules reflect preferences for their observed or predicted consequences, a disequilibrium that is "repressed" on the level of current decision-making is likely to re-appear on the level of decisions about rules. Whether that argument is convincing or not, it seems to have led Shepsle (1986a) to the view that a completely different theory should be used for institutional choice. For various reasons, in particular the higher uncertainty that prevails on that level, cooperative behaviour and hence equilibrium (in a different sense than the one used in the context of voting) are seen as more likely and the relevant framework as being the theory of repeated games and conventions. One might feel wary, before some elaboration of that suggestion, to exchange democratic disequilibrium for the multiple equilibria of game theory but our main objection is about consistency and can be formulated in the form of the following dilemma. Either one wants to keep to the conception of democracy underlying the model of "democratic equilibrium" and it seems inconsistent not to apply that model to the choice of rules, which makes the infinite regress and Riker objections difficult to meet, or one accepts to break away from the Blackian programme and there is no compelling reason to remain faithful to the concept of "democratic equilibrium" for the analysis of decisions about current issues.¹⁹ Most of the reasons we may find to use a different theory at the higher level are relevant also at the lower one.

The search for other concepts of equilibrium within the same framework

The rationale for following this second path to the rescue of the Blackian programme is to be found in the observed results of laboratory experiments. Since experimentation is concerned with the consequences of majority rule under conditions that are as "pure" as possible, the institutional layout is minimal. As already noted, experiments provide valuable information on the consequences of majority rule when a Condorcet winner exists. When it does not, what they say consistently is that outcomes of voting are not haphazard, that they display notable regularities. The problem then is to define or discover in the existing literature (e.g. in game theory) a concept of equilibrium which can account for the observed regularities.

There are many possible candidates for that function. Grofman *et al.* (1987) argue the case for the "Copeland winner" (known since 1951) - or "strong point" - defined (roughly) as the alternative "which is majority-preferred to the highest proportion of other alternatives in the space." As enumerated by the authors, the merits of the Copeland winner are really impressive. Among other virtues, it always exists, is generally unique, can be located relatively easily

(thanks to Grofman *et al.*), is identical with the Condorcet winner if the latter exists, and has long been known in social choice theory to have various desirable axiomatic properties. Still, it is clear from their paper that what matters most is that "it turns out to predict outcomes in the Fiorina and Plott (1978) experimental voting games better than do more than one dozen previously proposed models" (p.541). In other words the case is stated in terms of expediency, normative connections and accounting for past results rather than in terms of derivation from a positive theory about real-world processes or of prediction of novel facts.

Another illustration of that kind of reasoning - verging on verificationism - can be found in McKelvey (1986). The idea now is that trying to derive theoretically a single point as the outcome to be expected from majority voting might be too ambitious. We should be content with a theoretical specification of the subset (preferably not too large) of the possible alternatives in which the final outcome should be found.²⁰ The "uncovered set" proposed by Miller (1980) qualifies for being such a subset. It is the set of all feasible alternatives x such that for any other feasible alternative y , either x is majority-preferred to y , or there is another feasible alternative z such that x is majority-preferred to z and z is majority-preferred to y . Again, McKelvey's justification for the superiority of that concept is not that it is derived from a positive analysis of voting or can be submitted to real empirical testing, but only that it accounts for two regularities observed in experiments: final outcomes of voting fall in a circumscribed area and the position of that area is central vis-à-vis the ideal points of the subjects.²¹

The methodology underlying the two kinds of work discussed in this section is retrodiction, i.e. accounting for regularities that are already known. We do not agree of course with Fiorina and Shepsle (1982) that retrodiction could be enough for a discipline to qualify as scientific.²² But that is not the main problem here. What should be concluded, it seems to us, from our brief discussion of an already large literature is that, even if it succeeds to some extent in solving the theoretical disequilibrium-cum-empirical stability puzzle, it remains subject to the three more basic objections that we raised in the preceding section to the Blackian programme. If, as argued there, the root of the problem is to be found in a mistaken analogy with competitive equilibrium, it seems worth exploring another kind of analogy with equilibrium in economics: analogy with the equilibrium of the firm.

IV. From competitive equilibrium to equilibrium of the firm: a more relevant analogy?

What the Blackian programme offers us is a series of models increasingly well suited to the study of a collective decision-making rule - taken in isolation and given a specific form. If we turn to the real world, what we observe is a vast array of complex political institutions in

which we have no compelling reason to think that recourse to the majority rule is an essential characteristic - the majority rule usually followed, moreover, not taking even approximately the form of the Condorcet procedure. Theoretical disequilibrium-cum-empirical stability is a puzzle in a world limited to models and laboratory experiments, not a serious anomaly in the context of the relation between theory and non-experimental observation. Whether there is equilibrium or not in the standard or structure-augmented "democratic equilibrium" models cannot have much bearing on the question of stability in real-world political institutions - the bearing there is being mostly of a counterfactual kind as we shall see.

Since for analytical purposes it is difficult to do without a concept of equilibrium, the example of the theory of competitive markets, as interpreted in Section II, suggests that we should aim at deriving the concept needed here from a positive analysis of decision-making processes. In fact, many mechanisms involved in the processes are already known - e.g. exchange of information, change in opinions, various kinds of manipulation, negotiation between leaders. It is likely that they often have more influence on the final outcomes than does the voting rule, although the two are clearly related. Some of the mechanisms have been modelled but a general study of how they interact to produce the final decision lies beyond the reach of our analytical tools (including game theory). Thus, deriving a concept of equilibrium from an analysis of processes of the kind that is relevant here does not seem possible for the time being.

We have in the economy an important institution that raises or could have raised similar problems: the business firm. Except in the case of the autocratic arrangements whereby one single person takes all the decisions, actual decision-making in firms is a very complex matter indeed. Yet, mainstream economics has found, at least for many analytical purposes, ways to by-pass that complexity, the main one being the assumption, introduced directly in the models, that profits are maximized. That shortcut to equilibrium modelling is usually justified both by competition, seen as a constraint imposed on decision-making by markets, and by the fact that only limited predictions are sought from the models. Could the modelling of democratic institutions find inspiration in the equilibrium of firms rather than in the analogy with competitive equilibrium that is a characteristic of the Blackian programme? The main objection to that suggestion is of course the view that democratic institutions behave as monopolists. This implies that, contrary to the firm, internal decision-making is not constrained by competition, and an essential justification for glossing over it is lacking. That objection can be met, and has in fact been met, in two ways.

First, the objection reflects a perspective on democratic institutions which, although widespread, seems to us very questionable. Far from constituting a monopoly, each democratic

institution is, to a degree which is not incommensurate with that of private firms, submitted to very powerful competitive forces. There is first of all what we can call intra-level and inter-level competition between central, regional and local governments, in the form both of competition for resources and power (see Breton, 1987), and of emulation or competition for performance (tournament or yardstick competition; see Salmon, 1987b and forthcoming). Inside each government, there is competition between the legislative branch (inside that, between committees, e.g.), the executive branch (inside that, between bureaux), the judiciary, and various other authorities such as the central bank (cf. Breton *et al.*). In democracies, it is not easy to disentangle that intergovernmental and intragovernmental competition from the electoral or party competition with which we are more familiar. That is why, paradoxically, we perceive it more clearly, and understand better that it is not merely a corollary of electoral competition, when we turn our attention with that idea in mind to non-democratic regimes such as the totalitarian ones (see Breton and Wintrobe, 1986, on the Nazi system; Wintrobe, 1988, on Soviet Union) or the French Ancien Régime. If an institution's performance is poor, one should also take into account potential competition by institutions that could be set up, often as a result of political entrepreneurship, to replace it or to reduce it to insignificance (the word used by political scientists is "encapsulation"). Finally, in all societies, political institutions as a whole are submitted to the actual or potential competition, for the satisfaction of demand, of private institutions such as firms, associations, churches, co-operatives and families (see Breton 1989) - not to speak of organizations such as the Mafia.

If we see all political institutions, on whatever level, as living in a competitive environment, it becomes clear that their survival is conditional on a minimum of stability and efficiency in their decision processes. Now, not only is the Condorcet procedure costly (a point made forcefully already by Sloss, 1973) but, more seriously, it always implies a non-negligible risk of instability. It is a great achievement for the cycling theorems that they have shown this, but it is highly unlikely that the result would not have been already embodied in common wisdom. Institutions which attempt (or would have attempted) to reach "democratic equilibrium" reduce (or would have reduced) their life expectancy or their chance of remaining important as compared with institutions unconcerned with such an objective. It is not surprising then that observation provides few examples of institutions of the first kind, nor that the most salient feature of voting is its paucity.²³

As is always the case with arguments that are partly based on natural selection, as is the foregoing one, there is a risk of functionalism. Without discussing that methodological point, let us note that reasoning along the same lines has become important recently in the theory of

the firm for the explanation of organisational forms (see in particular Jensen and Meckling, 1979; Fama and Jensen, 1983a, 1983b).²⁴

There is a second way to meet the objection that political institutions do not live in a sufficiently competitive environment for the analogy with the equilibrium of the firm to be relevant. The managerial theory of the firm, as formulated by Baumol (1959), Williamson (1964) and others, eludes the need for a detailed analysis of decision-making within firms even when they are assumed to be unconstrained by competition and thus decisions to be to some extent discretionary. The authors remain content with a simplified account of the power structure of the firm and derive from that an equally simple model of maximisation of a utility function under a constraint. For instance, the power of the shareholders is taken into account in the form of a minimum profit level constraining the discretionary power of managers, who maximize a function whose arguments are generally specified. Although the analogy with the managerial theory of the firm has perhaps remained mostly unnoticed or implicit, the same modelling strategy has been in fact at least as typical of the economic approach to politics as has been the Blackian programme. Government behaviour is no longer to be explained by a model of equilibrium à la Downs - that is, à la Black - inspired by competitive equilibrium, but by a model of individual equilibrium - that is, of maximization under constraints - quite similar to the managerial model. The governing team is assumed to pursue its own objectives within the limit set by various constraints - e.g., the need to reach a level of popular support sufficient for remaining in power.²⁵ The model of Frey and Lau (1968) and more generally the vast literature on the political business cycle and the interaction between the government and the economy (see, e.g., Van Winden 1983) are an illustration of that research strategy.²⁶

A merit of the analogy with the equilibrium of the firm is that it does not suggest too precise a dividing line between the political institutions which deserve the label "democratic" and those which do not. The perspective it offers is one of a variety of political institutions, all more or less oligarchic but constrained to various extents by the wishes and actions of "ordinary" members and by competition from the outside, and in most of which majority rule is not the single or most decisive influence on choice. This allows us to break away from the idealized or utopian conception of democracy that underlies, as a result of its normative presuppositions, the Blackian programme. Yet, the main merit of the analogy with the equilibrium of the firm is that it helps us to keep our attention focussed on the need to build models that can be used- which implies both predictive power and no excessive contradiction of salient facts of the real world.

V. Conclusion

The foregoing objections and comments should not be misunderstood. We do not deny the importance of what has been achieved by voting theory over the last forty years. Most of the methodological aspects on which we expressed reservations, such as the ascendancy of models over theory, of normative over positive considerations, of equilibrium over processes, of simple over comparative statics, or of retrodiction over prediction, can also be found to various degrees as characteristics of recent research elsewhere in economics.

The question is what exactly are the objectives, or, alternatively, what exactly is stated about the real world. Our objections are addressed to what we have called the Blackian research programme, that is, the claim, already formulated by Black, that the concept of "democratic equilibrium" (as defined) should play as central a role in the analysis of democratic institutions as does competitive equilibrium in the analysis of markets. Inasmuch as this was or still is the objective, we think that the programme has failed, first of all because democratic institutions have many features, of which majority rule is but one aspect. Excessive concentration on that aspect (or, worse, on a particular form of it, "democratic equilibrium") tends to suggest that it is the essential one which, we argue, is misleading for a real understanding of democratic life and institutions.

But if the claim is not made, that is, if the purpose of the work on voting in general and on "democratic equilibrium" in particular is seen as the study of one aspect only, not necessarily the major one, of democratic institutions, then the scientific fruitfulness of that work cannot be doubted. To interpret it then, a useful methodological framework may be the one offered by the "semantic conception of theories", as formulated in particular by Frederick Suppe. For that philosopher, a theory provides a "comprehensive characterization of the behaviour of phenomena" under "idealized conditions". "When coupled with an appropriate experimental methodology, the theory can also predict or explain phenomena that do not meet these idealized conditions by displaying how these phenomena would have behaved had the idealized conditions been met" (Suppe 1989, p. 67). In other words, the truth that is sought is counterfactual truth with regard to the actual world - which implies empirical truth with regard to some possible artificial or isolated system. If we accept this view, we should expect laboratory experimentation - particularly well suited to test models that address phenomena under idealized conditions - to play a major role in the "appropriate experimental methodology" mentioned here.

The theory of voting is not the only one to rely on experimentation. Roth (1987) concludes a survey centered on other areas (such as bargaining) with the opinion that "there is

good chance that one of the things that will be remembered about science in the latter part of the twentieth century is that laboratory experimentation entered the portfolio of tools that economists use to study the world." (pp. 295-96). This may be so in general. But, in the case of voting, the question is whether a theory about one isolated aspect of the world, even when corroboration in the laboratory or otherwise allows one to decide that the theory is or may be true, should be considered also as interesting (in a pragmatic sense) when that aspect is non-existent or minor in the actual world. It is clear from the way they discuss the merits of theories that economists in particular, rightly or wrongly, are not exclusively concerned with their truth. They also want relevance, that is, explanatory and predictive power about phenomena that are actually important. The Blackian programme, as conceived originally, satisfied that requirement. But, inasmuch as it still does, that is in a paradoxical sense. The pragmatically relevant achievement of the theory of "democratic equilibrium" (in the sense that we gave it) is that, by describing what might be the consequences of a particular decision rule or procedure were it to be followed, it offers or suggests an explanation for the observable fact that it is not actually used. In terms of relevance, the conclusion should be thus that fruitfulness there is, but not of the kind that was intended.

Footnotes

* A first version of this paper, in French, was presented at the 3rd Conference of the Association Charles Gide pour l'Etude de la Pensée Economique, "Emergence et fondements des concepts d'équilibre en économie", Strasbourg (France), 21-22 September 1989. We are grateful to the participants - especially our discussant, Jean-Dominique Lafay - for their valuable comments. We must also thank Manfred Holler for his detailed remarks and suggestions. Many are not reflected in this paper but they will certainly inspire our future work. The errors and shortcomings that remain are our sole responsibility.

1. Black (1948, pp. 141) also notes the following analogy between majority-voting equilibrium and competitive equilibrium: "No matter in what manner the preference curves or optimums of the other members alter or move about, if it is given that one optimum remains the median optimum, the decision of the committee must remain fixed. The analogy with economic science is that, in the determination of price in a market, price remains unchanged so long as the point of intersection of the demand and supply curves is fixed and given, irrespective of how these curves may alter their shapes above and below that point". Böhm-Bawerk's so-called marginal pair, to which Black refers here, has not had a success in supply and demand analysis that is comparable to the success of the median voter in voting theory.

2. Forerunners such as Condorcet, Borda, Laplace, and Dodgson are known now as a consequence of the presentation of their work by Black - who discovered their analyses after having published his own ideas (see the preface of Black 1958). One should also mention the article of Bowen (1943), which Black did not know either when he published his first papers. Progressively, the implications of the work of Black (consisting increasingly in negative results, as described in the text) have been associated with the theorem of Arrow (1951).

3. McKelvey (1976,1979), Schofield (1978), Cohen (1979), Slutzky (1979), Schwartz (1981), e.g.

4. One should compare, e.g., the textbook of Riker and Ordeshook (1973), in which the problems raised by the result are glossed over, with their subsequent volumes - Riker (1982) and Ordeshook (1986) - in which they are at the centre of the stage. In the new edition of his well known survey, Mueller (1989, p. 88) writes: "That majority rule leads to cycles is a (some would say the) major theme of the public choice literature".

5. For equilibrium to exist, there must be a median alternative in all directions (Davis et al. 1972).

6. Cf. Bernholz (1973) for cycling under the assumption of logrolling.

7. In other words, the theory cannot escape the often repeated question first raised by Gordon Tullock (1981): Why so much stability?

8. Enelow and Hinich (1984) and Mueller (1989) are obviously enthusiastic about the implications for the cycling issue of probabilistic voting. Although Ordeshook (1986) refers to the major papers published on this subject and to Enelow and Hinich (1984), he devotes less than two pages (out of five hundred) to it and seems clearly not very interested. Our feeling is that probabilistic voting is really important but that, perhaps, it will prove most fruitful for the analysis of issues that are to a large extent independent of "democratic equilibrium", median voters, and so on - e.g. for dealing with an issue such as moral hazard. Casual or implicit use of probabilistic voting in the context of moral hazard (or incentives of politicians), is quite natural and has probably been frequent for a long time (Salmon, 1987b, pp. 33-34, is an example that crosses our mind).

9. For instance, in some recent models of trading processes, the market is conceived as constituted by a large number of agents who engage in two-party bargaining under the pressure resulting from the possibility that either party interrupts the talks and turns to another agent. Under some assumptions, such a process leads to Walrasian equilibrium (see Rubinstein and Wolinsky, 1985; Gale, 1986; and, for a survey of the more general approach, Wilson, 1987).

10. The twin ascendancies of models over theories and of equilibrium over processes characterize a large part of game theory. As we have noted both "democratic equilibrium" and competitive equilibrium are Nash equilibria. But the former is not much more than that, while the latter can also be derived from a theory of competition centered on processes.

11. Another normative argument in favour of the Condorcet winner, or perhaps another formulation of the same argument, is provided by Black (1958, p. 57): "Certainly this criterion appeals to our sense of symmetry; and the connexion between mathematical symmetry and what is ethically right may be closer than has been recognized".

12. In addition, we may note that the ethical appeal of the Condorcet solution and of the symmetry argument in favour of it (note 10) is notably less compelling when intensities of preferences are taken into account and interpersonal comparisons of utility allowed.

13. When used for ideological purposes, the existence theorems of general equilibrium deliver a message that is unclear. Inasmuch as they show that completely decentralized decision-making and order at the aggregate level are not as incompatible as one might think, they are employable as an argument for capitalism and markets. But they are probably more often used as an

argument for extended government on the grounds that they depend - for the logical proof that a decentralized economy produces order - on assumptions and conditions that are unrealistic or unfulfilled. To the extent that the programme centered on "democratic equilibrium" has evolved into one focussing on "democratic disequilibrium", and has thus cast doubt on the possibility of deriving additional meaning for the "will of the people" from the rule of majority, it should logically work more against government than in favour of it. Is that another instance of a bias in economics against government? No if Tullock (1987) is correct when asserting that the major contributors to the cycling literature are liberals (in the American sense of the term). But insofar as, thanks to the tenacity of the same voting theorists, "democratic equilibrium" has been ultimately rescued by the recent work to be discussed in Section III, should not the tenacity itself be explained by the political inclinations that exist according to Tullock? Clearly such an explanation would not be very convincing, in addition of being somewhat unpleasant. In fact, the feeling that "democratic equilibrium" gets close to the essence of democracy is widespread, which explains among other reasons that one need not be a liberal to be annoyed by the pervasiveness of the cycling result and to endeavour to do something about it (e.g. Tullock 1967).

14. It is exactly because democratic equilibrium seems more directly observable that it cannot be dealt with in the way suggested by Weintraub (1985) for competitive equilibrium, that is, as a part of the hard core of a Lakatosian research programme, immune by methodological fiat from any attempt at refutation.

15. When studying the economy, economists can generally do without a precise knowledge of the equilibrium values of prices. Where that knowledge is necessary (e.g. for the prediction of variations in exchange rates), the predictive record of economics is usually disappointing.

16. The authors report an instance in which the equilibrium alternative had been selected by a majority vote before it was defeated in the final vote by another alternative (admittedly, a close one). The alternative whose definition is that it cannot not be defeated by any other was in fact defeated - a result clearly implying that one of the subjects at least voted against his or her self-interest. In other words what is deemed impossible by the model can happen. One should put to the model's credit that retrospective demonstration that it is empirically refutable.

17. Ingberman and Inman (1988) mention only one econometric test.

18. According to Ingberman and Inman (1988), structure-induced equilibrium models have implications that are not in agreement with casual observation. In particular, small changes in the specification of exogenous variables entail an excessive variation in voting outcomes. More structures being brought in would probably mitigate that, but, it can be feared, at the price of increased ad hocness.

19. In other words, recourse to a different theory for each level is ad hoc given the characterization of the Blackian research programme. Hands (1988) argues that ad hocness in that sense (i.e. lack of consistency within a research programme), different from the ad hocness discussed earlier in the text (untestable assumptions added for the purpose of saving a theory), is the one economists dislike most.

20. Modesty here is born of necessity since the results of experimentation show that subjects with the same preferences and in the same settings do not usually select the same alternative. We have thus an interesting instance of a feedback effect from empirical testing on a major feature of the theory - a practice quite common and probably unavoidable in empirical research (see Mingat, Salmon and Wolfelsperger, 1985, chapter 5), but not highly praised in methodology. It should be added that circumscribing an "equilibrium area", although it allows

the derivation of empirical predictions, is not usually considered in economics as being quite enough (as illustrated by the comments often made on the elementary bilateral monopoly model).

21. Other authors derive an area in which final decisions are expected to be circumscribed - e.g. Ferejohn, Fiorina and Packel (1980).

22. In their attempt at justifying retrodution, Fiorina and Shepsle (1982) refer to the views of Goldberg (1968). That is somewhat surprising since this political scientist (like many others) insists that in his field also one should not remain content with achieving retrodution but should seriously try instead to derive empirically falsifiable predictions.

23. The significance of the status quo is much greater in political processes than it is in market processes. A price value, one among many within a general process of price adjustment to new conditions, has no particular meaning. Changes in price are usually continuous and often involve only a small subset of traders at a time, while all the members of the group are involved in a democratic decision. Thus one does not change a democratic decision as one changes a price. It can even be argued that one of the functions of democracy consists in checking whether the status quo should be kept. Hence the privileged position of that alternative, emphasized for instance by Shepsle (1986b, p. 145) - who notes that "voting the status quo last" is "the practice of every legislative body with which [he is] familiar." It may be added that, assuming uncertainty, the status quo is the only alternative we can know with certainty, from direct experience.

24. Fama and Jensen explain in that way why co-operatives have not had much success or why more democratic arrangements within firms have seldom been adopted. For a critical assessment of that approach, see Putterman (1988).

25. The analogy with the business firm and the view that political institutions live in a competitive environment may suggest a modelling of democracy in terms of supply and demand of policies or goods and services (not the only possible rationale for such modelling of course). It is often argued that the median voter model is about demand, other considerations underlying supply (see, e.g., Holcombe, 1989). Actually, one finds a reflection of this view in many models in which a distinction between supply and demand is made explicit or is implied. But one may doubt whether, in most cases, the reference to the median voter should be taken seriously. In a large part of the literature, the main concern is with the causes and consequences of variations in supply, in the context for instance of the political business cycle, or of the growth of government, and in terms of comparative statics or dynamics rather than of simple statics. Thus, while an assumption on demand (or on support or popularity) is clearly indispensable, it needs refer to some properties and arguments of a demand (support, popularity) function and to the stability of that function rather than to a particular synthetic indicator such as the median or representative voter - although reference to such an indicator is apparently more convenient.

26. Parallels with the theory of the firm can be found in other lines of theorizing. The relation is particularly explicit for instance in Weingast and Marshall (1988). In other cases the similarities are more hidden. In a number of models, the behaviour of a democratic institution is viewed as the pursuit of a common interest (assumed to be maximized) rather than as the outcome of a specified process of aggregation of diverging preferences. Thus some aspects of decision-making in the US Congress have been explained by the assumption that a norm exists in that institution which ensures that all its members benefit from its decisions. According to Weingast (1979) and Niou and Ordeshook (1985), compliance with such a norm can be the result of the

rational individual behaviour of each member. It accounts better than "democratic equilibrium" for observations such as the surprisingly high frequency of quasi-unanimous votes.

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