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Chapter 9

Summary and Conclusions

The objective of the research presented in this thesis is two-fold. First, to develop models of interest groups that allow for bounded-rational behavior by agents, account for the emergence and development of interest groups and engender well-established macro-phenomena, like fluctuations in group sizes, clustering of political opinions or separation in electoral parties’ platforms. The second research objective is to explore the nature of motivations that drives individual behavior in certain experimental economic environments, to test the existing models, and to develop a theory that formalizes and better fits the data. Parts I and II, respectively, present the results of our efforts to meet these objectives. The importance of this research is related to the substantial evidence - in particular, generated by scrupulous research in the laboratory - falsifying the paradigm of the fully rational and narrowly self-interested agent (‘homo economicus’). The models developed in this thesis should be seen as tentative building blocks to the development of a more satisfactory theory of (political) economic behavior.

9.1 Part I: Individual-based models of interest groups

Our first model, presented in Chapter 2, puts interest groups in a context of redistributational politics. It departs from existing game theoretical models of interest groups in three respects. First, our model provides a mechanism of collective action based on decentralized private incentives, which differs from those put forward in the current literature.
on collective action (Sandler (1992)). The individual decision to participate in collective action is taken to reflect the dissatisfaction with the status quo and represents satisficing instead of optimizing behavior. Second, instead of assuming groups of fixed sizes, we allow interest groups to be endogenously formed. Third, instead of presenting the interaction between the government and interest groups as a properly defined game we model it as a dynamic adaptive system. Analysis of the model shows, for example, that due to the endogeneity of the size of an interest group an increase in the contributions by its members need no longer be beneficial to this group, even though it would in case of fixed sizes. Our model predicts that using the (relative) numerical strengths of social groups as a proxy for political influence produces mixed results, in line with empirical findings (see Hettich and Winer (1999)). Similarly, an increase in the size of a social group - say, the number of retired - may or may not be detrimental to the group, from a redistribution point of view. All of the above results illustrate the restrictiveness of the common assumption of exogenously given interest group sizes. Furthermore, the dynamic analysis of the model shows that equilibria may not be stable. Hence, reliance on equilibrium results - which are typically focused on models of interest groups - can be misleading. The evolving policies and interest group sizes may exhibit path-dependent patterns. Moreover, the model can generate by itself - that is, without the help of any exogenous shocks - the types of spurts and declines in organizational density that are observed in reality. The results obtained from the comparative-statics and dynamic analysis seem interesting and realistic enough to warrant further theoretical and empirical investigation.

Chapters 3, 4 and 5 are concerned with the emergence and role of interest groups in a spatial framework of electoral competition. According to Hinich and Munger (1997, p.6), “Spatial theory is the only theory that provides an integrated model of voter choice, party platforms, and the quality of outcomes. For a complete model, formal spatial theory is the only game in town.” Spatial models of electoral competition are indeed a powerful tool for representing the interaction between voters and political candidates in elections. However, they have some serious drawbacks as a realistic description of elections in democratic societies. Our research aims at extending the spatial competition model in several directions that may give it a more plausible nature. These directions are centered
around two important features. First, our models allow for the adaptive behavior of political parties as well as the adaptive nature of voters' opinions. To model the adaptive behavior of parties, we borrow from Kollman, Miller and Page (1992), while the adaptation of opinions (political preferences) is captured by introducing socially interacting voters. In our models voters learn about other voters' beliefs and adapt their own beliefs (or preferences) accordingly. Second, we introduce interest groups into the spatial competition framework. The functions of these interest groups are multiple: first, to coordinate voting behavior; second, to transmit information about voter preferences to the political parties through (financial) contributions to opinion polls; and third, to influence the election outcome by imposing conditions on polling. Therefore, our models of interest groups integrate both support and exchange motives for campaign contributions. Furthermore, we allow for the endogenous emergence of interest groups, a feature which, until now, has been neglected in the vast literature on interest groups. To sharpen our results, we assume that voters are uniformly distributed in a symmetric policy space. Thus, in the basic model the existence of a dominant point - which is the center of the space - is guaranteed.

Employing simulation methods as a first step of analysis, in Chapter 3, we find that in the presence of interest groups winning platforms, provided they exist, are selected faster than in the basic model. The challenger's probability of winning the election increases if there are interest groups, which appears to be due to an increase in the winning set. The first appealing result in that chapter is that the policy outcome eventually converges to the center of the distribution of voter preferences only in the basic model. The second interesting outcome is the emergence of clusters of political opinions in our dynamic model of social interaction. Whether the dynamics generate one, two or more big clusters, depends on the stochastic elements of the model. However, minority opinions do survive. The most notable effect of the social dynamics seems to be that it prohibits, (also in the long run) party platforms to converge to each other or to some position in the policy space. The non-convergence results represent a new property that may emerge via social interactions: the dominant position in the space may disappear. In our view, this is the third interesting result of Chapter 3.
Chapter 4 develops deterministic dynamic Markov processes of the stochastic models, with and without interest groups presented in Chapter 3. We show that the dynamics of the distance between the policy outcome and the center of the space, and the probability that the challenger wins an election, replicate qualitatively the respective dynamics generated from the detailed individual-based models of Chapter 3. The first result obtained from the mean-analysis shows that, in the long run, the election outcome of the Markov model with adaptive uninformed agents will be the center of the space for the basic model. However, the long-run result for the model with interest groups will be either the center of the space (with probability 0.22) or the set of positions closest to the center (with probability 0.78). To our knowledge, this is the first study pointing at, and providing a micro-foundation for, the possibility of a voting cycle in the presence of a dominant point. Although for different reasons, our work contributes to research in the spatial theory of elections that advocates that "...democratic voting is characterized by forces that keep outcomes reasonably close to the center of voter opinion..." (Enelow and Hinich (1984, p.223)). A further investigation shows, however, that if the size of the population is lower than some threshold (1000 for our specified model) voting cycles become frequent phenomena and expand all over the issue space. Thus for small populations, our model positions itself in the series of models that point at the electoral instability of voting outcomes (McKelvey (1976, 1979), Schofield (1978)).

The inherent property driving our results is that the winning set (i.e., the set of policy platforms that will defeat the current incumbent) increases in the presence of interest groups. This happens in all the stochastic and numerical simulations. Chapter 5 presents a general result showing that, for slightly modified versions of the Markov models, the winning set of the positions in some neighborhood of the center of the space increases in the presence of interest groups. This is our last result in Part I. In our view, it is a challenging result that hints at interest groups being one of the forces that keep electoral outcomes away from the center of voters' preferences.

The framework set out in this part of the thesis presents a first step towards modeling the complex relations between adaptive voters, adaptive political parties and interest groups. We hope that we have succeeded in arguing that such models can contribute
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significantly to our understanding of the complex political-economic world we are living in. The development of more elaborate models analyzing these issues is required. Furthermore, the agenda of a future research may include: (i) further investigation and exploration of the determinants of individual decision-making using both laboratory and field experiments; (ii) mappings of specific concrete economic interests of individuals in public economic policies (van Winden (2002)); (iii) modeling the emergence of leaders and political parties.

9.2 Part II: Other-regarding preferences

The shape and content of the utility function is important for rational choice theory, and, in particular, for the detailed individual-based modeling of political and economic phenomena. The work we present in Part II of this thesis aims to contribute to the recent research departing from the narrow self-regarding preferences assumption. There is now substantial experimental evidence against the traditional 'homo economicus' model. This evidence leaves the profession with the task of constructing alternative models in order to gain consistency with the empirical data. We believe that the task of building a plausible theory cannot be undertaken successfully unless we can discriminate among the observable implications of alternative possible motivations. The game triad experiment reported in Chapter 6 is helpful in this respect because it makes it possible to discriminate between trust, fear, reciprocity, and altruism in at least one particular environment: the moonlighting game. Motivations like these may also play an important role in collective action and group formation, phenomena that Part I was concerned with. Abbink, Irlenbusch, and Renner (2000) had previously reported data for the moonlighting game that are consistent with reciprocity but inconsistent with the traditional self-regarding preferences model. However, we cannot disentangle and identify players' motives that influence behavior in their experiment. As we argue in Chapter 6, the game triad experiment provides such an opportunity. Results from our experiment support the conclusion that 39% to 47% of first movers in the moonlighting game are motivated by trust in the positive reciprocity of second movers. Furthermore, this trust is in line with rational expectations.
because the behavior of second movers who received positive amounts from first movers is characterized by significant positive reciprocity. Indeed, positive reciprocity causes trusting behavior to have positive expected profit: first movers who send positive amounts to second movers make an average profit of 1.93 euros in our moonlighting game treatment. The behavior of 47% of the first movers is inconsistent with fear of negative reciprocity. This absence of fear is also in line with rational expectations because the behavior of second movers who are confronted with money being taken away from them by first movers is not characterized by significant negative reciprocity. Indeed, the absence of significant negative reciprocity causes taking behavior to have a small positive expected profit: first movers who take money from second movers make an average profit of 0.15 euros. This conclusion needs some qualification, though, since the average profit of first movers who give money is notably higher than the average profit of those who take money.

Falk, Fehr, and Fischbacher (2001) report on experiments with a design that includes the moonlighting game and a control treatment in which the amounts taken or sent by first-movers are randomly generated. Their design uses the “strategy method” where second movers have to choose responses to all possible first-mover decisions, or random determinations of first-mover amounts, before observing the actual amounts taken or sent by first movers. Another way in which their design differs from ours is that they use a single-blind payoff protocol in which individual subjects’ decisions are known by the experimenters. They conclude that their subjects’ behavior is characterized by both positive and negative reciprocity. It is unclear which of the differences between their experimental design and ours accounts for the different conclusions about the significance of negative reciprocity. However, both experiments generate data that support the conclusion that attributions of intentions are a significant determinant of behavior in the moonlighting game. This leads to the conclusion that behavior in the moonlighting game cannot be fully explained by models of preferences over outcomes, such as models of inequality aversion (Fehr and Schmidt (1999), Bolton and Ockenfels (2000)) or egocentric other-regarding preferences (Chapters 7 and 8 of this thesis).

Papers reporting experiments with some other games also lead to the conclusion that both intentions and outcome preferences play a role. These other games include the ul-
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timatum game (Blount (1995)), the investment game (Cox (2001)), and the punishment game (Falk, Fehr, and Fischbacher (1999)). Some other experiments yield mixed results. Bolton, Brandts, and Ockenfels (1998) report that intentions are an insignificant determinant of behavior. Charness (2001) and Offerman (2001) report that intentions are significant for negative reciprocity but not for positive reciprocity. Cox and Deck (2001) find that negative reciprocity is not significant in the punishment game and that positive reciprocity is significant in the trust game with a single-blind protocol but insignificant with a double-blind protocol. Thus, the experimental evidence supports the conclusion that models that allow for both intentions and preferences over outcomes are needed to fully explain behavior. Models that include both have been developed by Rabin (1993), Dufwenberg and Kirchsteiger (1998), Levine (1998), Falk and Fischbacher (1998) and Charness and Rabin (2002).

However, we think that the development of a new outcome-preference model is important for two reasons. First, as we have demonstrated in Chapter 8, there are many game environments that are central to economics that produce data that can be explained by the egocentric other-regarding preferences model. And according to the principle of Occam’s razor, or parsimony, one prefers a simpler to a more complicated model when both can explain data of interest. Secondly, in the effort to develop models that can explain intentions-driven behavior, such as positive and negative reciprocity, one needs to decide what approach to take. Our view is that the most promising approach to take is to extend models of preferences over outcomes, by allowing the preferences to be conditional on perceived intentions, for application to environments that are not “reciprocity-free.” In taking this approach to theory development, one wants to extend a model that can successfully explain data from reciprocity-free environments.

In Chapter 7 we test models of (unconditional) preferences over outcomes, while Chapter 8 is concerned with developing a new model, the egocentric other-regarding preferences model, that is consistent with more data from “reciprocity-free” environments than alternative models. Results from dictator game experiments, reported in Chapter 7, show that the large majority of subjects are not averse to unequal outcomes. The majority of subjects choose very selfish unequal outcomes, or very generous unequal outcomes, depending
upon the price of generosity and other properties of the feasible set. Thus, instead of aversion to inequality, the dictator game data support models of other-regarding preferences with substantial price elasticity of demand for altruism. We apply the quasi-maximin model of Charness and Rabin (2002) to explain laboratory behaviour in our dictator game experiments and find that their model is of no help in 50% of the subjects' choices. A model of other-regarding preferences is developed that can rationalize dictator game data. In Chapter 8, we demonstrate that the egocentric other-regarding preference model can rationalize data from several other types of experiments, including experiments with proposer competition, responder competition, and voluntary public goods contributions. The present version of our model, however, cannot explain second-mover vetoes in the ultimatum game. Formally, models of inequality aversion can explain such vetoes (Fehr and Schmidt (1999)). An extended version of our model that permits the $\theta$ preference parameter to be negative for some agents could also explain the vetoes. But experiments reported by Blount (1995) make this a questionable approach. Her data lend support to the view that ultimatum game vetoes are a type of behavior that cannot be explained by models of preferences over outcomes, whether they be inequality-averse preferences or other-regarding preferences of the extended type; instead, explaining the vetoes requires introduction of perceptions of others' intentions into theoretical models, which presents the focus of our next work.