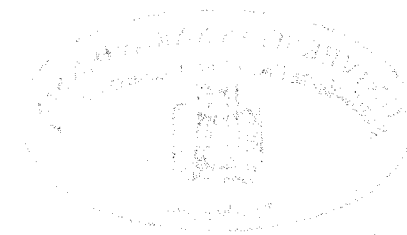


Hermann Brandstätter
Werner Güth (Eds.)

Essays on Economic Psychology

With 20 Figures



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Foreword

The chapters in this book have been written, reviewed and revised by psychologists and economists united by their interest in the dialogue between the two disciplines. The idea of contributing to this volume came up when the authors cooperated during a period of three weeks in the teaching staff of a summer school of the International Association for Research in Economic Psychology (IAREP). Each of the three workshops of the summer school running over 20 days and focusing on economic policy, consumer behavior, and entrepreneurial behavior was conducted by a professor of economics and a professor of psychology. Thus, there was a clear necessity and ample opportunity for an interdisciplinary dialogue. Of course, the chapters do not cover the whole field of economic psychology, but represent selected topics of the authors' research activities which are of a more general interest to psychologists, economists, or other social scientists who want to or have to deal with economic behavior in research, teaching, or consulting. An encouraging sign of the rapprochement of psychology and economics is provided by the fact that there are a number of chapters in the book in which the reader can not easily tell whether the author is (or was originally) an economist or a psychologist. We hope that crossing the borders between psychology and economics in both directions is encouraged by the book and that it contributes to a more comprehensive understanding of people's reactions to events and structural changes in the economic environment and of people's production of the economic environment and its changes.

Finally, we want to thank Christian Müller and Sandra Kopfberger for their assistance in our editorial work. They proved to be very careful and skilled in putting the manuscripts into a unified format, compiling the subject and author indices and checking the lists of references.

Hermann Brandstätter
Werner Güth

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Chapter 1

Introduction to Essays on Economic Psychology

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Problems to be solved in people's personal and social lives usually do not fit the boundaries of scientific disciplines well even if the development of those disciplines originally was stimulated by difficulties in providing food and shelter, securing health, protecting against crimes, strengthening the national power, facilitating trade and commerce, making sense of human life, etc. Disciplines in natural and social sciences, although at the beginning often installed and promoted primarily by practical needs, tend soon to narrow their scope in order to get a deeper theoretical understanding of segments of all too complex reality, or in order to develop a specialized technology for improving some components of the people's situation, irrespective of their interdependence with all the other components.

Although economics and psychology have some common roots in philosophy, as Wärneryd in his chapter points out, they have developed into highly specialized disciplines, and became rather isolated from each other. There have been, of course, some attempts and initiatives in bridging the gap between economics and psychology. For the past decade this is documented in the annual colloquia of the International Association for Research in Economic Psychology (IAREP), in a three years cycle of summer schools in economic psychology, in the publications of the Journal of Economic Psychology, and in a number of textbooks and monographs (Alhadeff, 1982; Earl, 1988; MacFadyen & Mac Fadyen, 1986; Kagel & Roth, 1994; Lea, Tarry & Webley, 1987; van Raaij, van Veldhoven & Wärneryd, 1988, Thaler, 1992). However, the dialogue between economics and psychology is still not an easy one. Mutual criticisms, misunderstandings and disappointments are quite common. No doubt, there is widespread segregation between departments of economics and departments of psychology at most universities.

Still, psychology and economics, in particular macro-economics, seem to be rather far apart. On the one hand there is the individual whose actions are in the focus of psychological research. On the other hand there is the national economy

whose parameters are drawn from effects of billions of individual actions channeled through institutional structures and economic policy. Is there any chance to bridge this gap? Do we not need sociology and political science, perhaps also some other social sciences, if we want to understand, how individual actions cause macro-economic effects and how individual actions are based on and shaped by social institutions? The answer can only be: yes, we do need the missing links. This was indeed the idea when in 1972, a curriculum of socio-economics was developed and installed at the University of Augsburg, combining psychology, sociology, and economics. At that time Burkhard Strümpel, a former student and assistant of Günter Schmolders, was visiting professor in Augsburg on a leave from the Institute for Social Research at the University of Michigan at Ann Arbor, where he closely cooperated with George Katona. The same idea was behind the foundation of the Society for the Advancement of Socioeconomics (SASE) in USA a couple of years ago.

There is no doubt that linking psychology and economics can profit a lot from mediation through sociology and political science, and that orientations within economics which take explicitly the function of institutions and their change (North, 1990) into account are particularly open to this kind of mediation. However, there are many problems of psychological as well as of economic research where a direct dialogue between the two disciplines turned out to be fruitful and has further promises for the future. Game theory as a family of normative models of rational, socially interdependent decision making can be confronted with psychological explanations of social problem solving. Much of so called experimental economics is open to this connection between (social)psychology and economics (cf. Kagel & Roth, 1994, as well as Güth's chapter on game theory in this volume). Individual decision making under risk is another field of psychological research (cf. Payne, Bettman & Johnson, 1992) which is of special interest to economists (cf. Machina, 1987). Attempts at explaining social processes beyond the economic sphere with economic models should be mentioned here, too (cf. Becker, 1976; Ehrlich, 1982; Frey, 1990).

There is a real need for cooperation between psychologists and economists in establishing economic psychology or behavioral economics, because each of the two disciplines has its restrictions and shortcomings in explaining, predicting and influencing individual and collective economic behavior. By the way, the term 'economic psychology' is preferred by psychologists crossing the border to economics, whereas economists usually speak of 'behavioral economics' when they confront their economic models with psychological assumptions about human behavior.

Can we say that a well defined economic psychology as a discipline of its own already exists, or is economic psychology still just social psychology applied to economic transactions? The answer would be "no", if neither the kinds of processes studied nor the way they are studied justify a separation of economic psychology from other fields of applied social psychology like legal, educational or

organizational psychology. Indeed, speaking of legal, educational, organizational, or economic psychology, we have obviously in mind some kind of professional specialization, i.e.; the lawyer, the teacher, the manager, and the economic policy maker. Should applied psychological research match this professional specialization? Obviously, teachers, lawyers, managers, or policy makers differ widely in the special knowledge they need. For example, geography or history is needed by the teacher, accounting and finance by the manager, legislation and procedural rules by lawyers, and economics by the policy maker. But, this does not mean that they need a different psychology in dealing with people or in considering the effects of their decisions on people. A closer look on the diverse fields of applied psychology shows that virtually the same processes are studied everywhere with the same theories and the same methods.

In terms of psychological theory, there is no real difference whether a person chooses a school curriculum or a job, rents a house or decides for carrying on a lawsuit. Why should studies on decision making be called educational, industrial, economic, or legal psychology, depending on the setting where the decision is made? Of course, each kind of decision needs special knowledge and relates to specific goals, but the processes by which a decision is reached can hardly be perceived as specific. Again, in terms of psychological theory, there is no real difference whether one tries to influence a pupil, a worker, a customer, or a judge. Are the laws of cooperation and competition really different when we move from the classroom to the factory, or from the market to the courtroom? It would be foolish to deny that the structure of the larger system has some impact on the way people make decisions, try to influence each other, settle conflicts, etc. However, this should stimulate the search for a more general theory in cooperation with other disciplines, and should not be taken as an excuse for particularism. For example, decision making in the behavior setting of a courtroom is a highly formalized, legally regulated procedure with a well defined and differentiated role structure, often watched suspiciously by the public, having effects not only on those immediately involved, but also on citizens' conceptions of justice and their attitudes towards the legal system.

Family decision making on the other hand; e. g., choosing the appropriate school for the children or a suitable car, is not watched by the public, but by relatives or friends, and is structured according to different social roles and implicit rules. However, psychologists must refer to the same theories of information processing and social influence and apply the same methods when they study legal or family decision making. Why, then, should it be called on the one hand legal psychology, and on the other hand economic psychology?

Let us look at another example: The Minister of Justice may be interested in the peoples' attitudes toward some planned changes in legislation on life imprisonment for murder, and the Minister of Economics wants to know whether the investors would favorably respond to a tax cut. Should the Minister of Justice look

for a legal psychologist, and the Minister of Economics for an economic psychologist for this job? No, both should entrust this task to a social psychologist or sociologist who is specialized on attitude measurement and public opinion polls, and who is prepared to cooperate with a lawyer and with an economist, respectively. In terms of relevant psychological theories and methods, studies on decision making in families and in the courtroom belong to one category, while studies on the citizens' perception and attitudes towards planned change in legal or economic policy belong to a different category. The tasks within a category are obviously more similar than the tasks between the categories.

We may design a matrix with classes of psychologically homogeneous tasks in the rows and classes of contexts or professional fields in the columns. Such a matrix is presented in Table 1.1.

Table 1.1
Matrix of Tasks and Contexts

Classes of Tasks	Environments (Contexts)				
	courtroom	school	business organization	market	economic policy
Making Decisions					
Measuring Attitudes and Forecasting Behavior					
Influencing People					
Training					
Resolving Conflicts					

From Brandstätter (1985, p. 520)

As far as applied social psychology is concerned, *research* can be easily organized around types of tasks, each task studied in a variety of contexts, and in

cooperation with specialists in the respective fields. However, *teaching* applied social psychology to students who later will work in traditional professional settings (schools, courtroom, business organization, government, etc.) necessarily has to take into account the full range of situations within that field of practice and to show that psychology can contribute to a better understanding of problems encountered there. Thus, teaching economic psychology to students of economics will probably be most efficient if it is based on general and social psychological theories as well as on more specific psychological research on predominantly economic settings. Knowledge of economic theory and a certain familiarity with practical problems economists have to solve during their professional career would be helpful, too. As yet, unfortunately, little is known about how practitioners can and do combine knowledge derived from research in different disciplines and practical knowledge derived from their own experience when they have to solve practical problems (cf. Muncas & Secord, 1983; Schuler, 1982).

The fact that economic psychology is not yet a full fledged discipline of its own does not mean that it couldn't become such a discipline in the future. Above all, it will be necessary to link psychological theories of individual and small group behavior to theories of micro- and macro-economic processes. Social dilemma research seems to be among the promising routes to that goal: it gives an idea of how individual behavior contributes to the production or destruction of common resources. Katona's idea of predicting changes in aggregate economic variables on the basis of aggregate consumer expectations (cf. Katona, 1972) and McClelland's (1967) ambitious endeavor of explaining differences in macro-economic growth between nations with foregoing differences in achievement motivation as a national characteristic should be mentioned here, too. Any other model will be useful if it relates individual behavior to collective effects, which in turn make up the environment to which large numbers of individuals respond in a similar way (cf. e.g. Duesenberry, 1949, who links the increase of aggregate consumption with increase of aggregate income to psychological concepts of a need for positive self-evaluation and social comparison). Economic models based on game theory are probably the most promising meeting place of psychologists and (micro-) economists who use rationality assumptions as a starting point for empirical investigations. An example of this approach is given by Güth in his chapter on game theory which points out that psychological constructs and methods could be helpful in clarifying the subjects' beliefs about (the rules of) the games they play. Obviously, micro-economics can be more easily integrated with psychology than macro-economics.

Up to now economic psychology was concerned almost exclusively with individual responses to some characteristics of the economic environment, and not with the production of the economic environment by the many acting and interacting individuals and groups; although both ways of influence have been explicitly stated as the object of economic psychology (van Veldhoven, 1981; van Raaij,

1981, 1984; Wärmeryd, 1981). A change of perspective and some new theoretical and methodological efforts seem to be necessary for the development of a genuine economic psychology that is open to micro- as well as to macro-aspects of economics. Such an economic psychology would have to show that simple concepts like self-interest and utility maximization are not sufficient for explaining an individual's economic behavior and the economy as a state of equilibrium of individual forces. What people want, how their desires develop and change, and what they perceive as behavioral costs in striving for their goals are genuine psychological questions that have to be answered in order to understand what self-interest and utility might mean.

In the following we give brief introductions and comments to the single chapters dealing with theoretical and empirical research questions concerning economic behavior which are of interest to psychologists as well as to economists. Some of the chapters may be rather difficult to readers not familiar with the mathematical language used by economists. The introduction should give some help in overcoming this barrier by spelling out the main ideas behind the formulas in ordinary language. Therefore, the reader will find more extensive remarks in the introductory chapter on these contributions than on others which are less mathematical.

The first chapter by Karl-Erik Wärmeryd reflects on the history of economic psychology and gives an overview on the main current issues of economic psychology. Stephen Lea's first paper is on the biogenetic roots of economic behavior, his second chapter clarifies the central concept of rationality as it is interpreted by economists and psychologists. Klaus Grunert discusses the contribution of cognitive psychology to the scientific analysis of economic processes. Wärmeryd shows in his second chapter that searching for the antecedents and consequences of new telecommunication techniques is not of less significance for economic policy than for marketing. Grunert's view of strategic management, too, links the organization to the larger socio-economic systems and their more or less predictable changes. Following the economic approach of formal model building, Werner Güth elaborates on the social-psychological theory of equity by stating the conditions for the applicability of the theory in resolving distributional conflicts. An overview to the economic theory of politics is given by Friedrich Schneider who in his second paper discusses the causes and consequences of an increasing shadow economy, again a field of research which calls for close interdisciplinary cooperation, in particular between economists and psychologists. Arthur Schram and Frans van Winden report on their model of decisions to vote (or to abstain from voting) from an economic perspective suggesting that such decisions follow from rational cost-benefit analysis in the economic interest of the reference group. The final chapter by Güth presents reflections and some new ideas about the relationship between normative and behavioral game theory.

Karl-Erik Wärmeryd: Psychology + economics = economic psychology. Connecting psychology and economics is difficult for a number of reasons. Although almost everybody would agree that aggregate supply and demand, prices, interest rates, unemployment figures, the ups and downs of stock exchange are heavily influenced by human action, there is a gap between dealing with individual behavior, the focus of psychology, and dealing with changes in collective economic measures, the focus of (macro-)economics. For sure, macro-economic theory is usually based on common sense assumptions about how people in general (as consumers or producers) tend to react toward changes in income, prices, interest rates, etc. Therefore, psychologists could confront the economists' common notions about human behavior with psychological theory by reformulating the economists' implicit psychological assumptions in terms of hypotheses derived from psychological theory and by testing the hypotheses in experiments and field studies. Of course, both approaches face the so-called aggregation problem whether or not a certain type of individual behavior is also true for a larger group of different individual actors. Notice that there usually exists another aggregation problem, namely to reduce the wide variety of different goods to a few categories. Although the latter aggregation problem usually receives less attraction, it might be psychologically relevant. Since aggregate commodities are artificial constructs, one does not encounter them in the economy.

However, it would not matter much for economic theory, if psychological research falsified the economists' assumptions or restricted their validity to specific conditions. Psychology seems to be in many (although not in all) respects irrelevant to economics mainly for the following reasons. Neither the usefulness of prescriptive economic models nor that of descriptive (and explanatory) models depend necessarily on the psychological validity of their premises. The prescriptive models are logically (analytically) true or false and do not need an empirical test. The descriptive macro-economic models are meant to explain covariation as well as causal relationships at the macroeconomic (aggregate) level. What counts here is whether lawful relationships between macroeconomic (aggregate) measures, for example between total income and total consumption, can be established. Whether psychological explanations of individual consumption and saving behavior can be given and whether these psychological explanations are in agreement with the economists' intuitive ideas about human behavior, is rather irrelevant. Descriptive economic models can be valid even if the psychological assumptions from which they originally were derived or the post hoc psychological interpretations by which they should become more plausible are wrong. The reason is that laws of individual behavior are one thing and laws on the collective (aggregate) level are another. Aggregate variables may come up with relationships that have little in common with the relationships of the same variables at the level of individuals. For example, individual behavior may employ and imply not much rational reflection, whereas on the collective level aggregate measures may "behave" as if the

(representative) economic agents acted rationally. If individual actions were more dependent on habits and/or affective impulses than on rational calculations, but if these irrational forces were more idiosyncratic, i.e. specific to certain combinations of personality structure, developmental stage, past experience, social role, and environmental conditions, then the grain of rationality, shared by most people, would render models based on rationality assumptions quite effective on the level of economic aggregates. As yet, psychology has little to say about how individual intentions and actions result in macroeconomic effects. Wärneryd's call for a macro psychology suggests to take into account not only what psychologists know about individual behavior, but also what they know or should learn to know about how social structures and institutions influence individual behavior and how individual behavior in the specific social context affects the economy as a whole. This is as central an issue for the future development of economic psychology and psychological (behavioral) economics as it is difficult to put into practice.

Nevertheless, trying to link psychology and economics is not a fruitless endeavor. As Wärneryd points out, economics and psychology have common roots in philosophical ideas about human nature and its manifestation in socio-economic processes. Also ideas on human behavior, put forward by early economists a hundred years ago when psychology was hardly established as a scientific discipline, have later been implemented in psychological research. Wärneryd's chapter on the relationship between psychology and economics suggests that there are still very few attempts to confront economic theory with modern psychological theory. Among the many psychological theories only those which state highly general laws - Wärneryd mentions Skinner's reinforcement principles, Kahnemans' and Tversky's prospect theory and Simon's bounded rationality idea - have a chance to be considered by economists in improving or reformulating economic theory. Obviously, not many economists are familiar with psychological research and, maybe, even less psychologists are interested in and acquainted with economic theory. An interdisciplinary dialogue between a psychologist and an economist is fruitful only if both understand each other's field of knowledge. If economic psychologists want to do more than what a traditional social psychology of individual economic behavior can achieve, they have to find out which economic models and which psychological theories are open to a linkage with a chance for scientific progress.

Stephen E. G. Lea: The evolutionary biology of economic behavior. Usually economic psychology tries to derive economic behavior by basic assumptions about human cognition and decision making. The limitations of human cognition and the processes of decision emergence are therefore considered as given, i.e. exogenously determined. If one wants to explain why humans decide in such a way, the most promising approach seems to be the one suggested by evolutionary biology. In essence, evolutionary biology tries to explain behavior by natural selec-

tion. Imagine, for instance, that behavior is genetically determined, as has been predominantly assumed in evolutionary biology. If for a certain species one genotype earns higher reproductive success than all other possible mutants, it certainly will become more and more frequent so that after some time one can expect a monomorphic population. Thus one can justify a certain type of behavior by demonstrating its superior reproductive success which, in evolutionary biology, is the expected number of offspring.

Stephen Lea tries to explain why humans rely on "economic behavior" where, in spite of its frequent use in economic psychology, the term "economic behavior" is usually vague. One way of specifying economic behavior would be, for instance, the maximization principle. It is, however, convincingly demonstrated by Lea that animal behavior can often be qualified as optimal. In abstract terms this is also clearly revealed by the concept of evolutionary stable strategies which are optimal replies to populations consisting of their own type (see Maynard Smith & Price, 1973). In an evolutionary stable population all existing mutants have to earn the same reproductive success, i.e. they all are best replies to the given population composition.

Due to the difficulty of defining economic behavior, Lea prefers to specify what it means to "live in economy", i. e. in a social and (relatively) closed interaction network with an over time increasing degree of labor division. Again it can, however, be demonstrated that high degrees of labor division can also be observed in the animal kingdom. Therefore Lea discusses whether or not one has to include trade (and money) as an additional requirement for a human economy. Since he sees no essential degree of trade in the early hunter-gatherer societies, Lea refuses the idea of identifying human societies as trade or exchange economies since trading economies occur only at a rather later stage of the human development.

A prerequisite for developing trading or exchange economies is certainly intelligence and more specifically language. Again intelligence and language do not seem to separate human and non-human labor division but clearly, according to Lea, labor division plus intelligence plus a carnivorous mode of life (by its effect on leisure time) seem to provide favorable conditions for the evolution of complex economies which characterize the late stage of human societies. Other aspects of human economies are toolmaking and the long period of immaturity. Both aspects are already observable in chimpanzee societies (see van Lawick-Goodall, 1974), but more typical for humans.

In summary, there seems to be no single aspect of economic behavior for which one cannot find an analogue in the animal kingdom. The main reason for complex human economies is probably that humans have developed many favorable conditions like intelligence and language which allow them to develop such sophisticated tools as airplanes, and to engage in complicated networks of trade and social interaction. According to Lea such complex economies can only function sufficiently well if humans can do both, be genuinely selfish but also cooperative and

trustworthy, where again these properties seem to have ancestors in the animal kingdom.

We do not completely understand the need to define economic behavior or what living in an economy means. One reason could be to justify the neglect of (evolutionary) biology when trying to explain economic behavior. Humans have developed certain capabilities like intelligence and language, the use of money, and toolmaking to levels of sophistication which make their societies genuinely unique, i.e. one can find at most a rudimentary analogue in the animal kingdom. As a consequence, understanding animal behavior provides only little help for economic psychology.

To be more specific consider, for instance, that one is interested in entrepreneurial behavior. Clearly, leadership is also an important aspect of socially living species in the animal kingdom. The respective observations by van Lawick-Goodall (1974) for chimpanzees undoubtedly have some relevance for leadership among humans. But, in our view, they specify at most favorable conditions for becoming a leader, e.g. a socially rich and playful phase of immaturity. Important as such prerequisites might be they often provide little guidance when trying to explain why a certain individual has become a successful entrepreneur.

Stephen Lea: Rationality. The formalist view. Traditionally the main difference between psychology and economics is that economic theory, e.g. classical and neoclassical theory, assumes perfect rationality whereas psychology would only rely on rationality if its requirements can and will be satisfied by human decision makers. In his essay "Rationality: The formalist view" Stephen E.G. Lea clearly states: "The axioms of rational choice look like assertions about human behavior and human cognition, and if they are interpreted in this way, they are plainly untrue at the psychological level." This may not convince an economic hardliner who simply would deny all empirical evidence by adding additional constraints to the optimization problems, e.g. limited recall, or by inventing new determinants of utility (see for instance, Bolton, 1991). One can always defend the concept of rationality in this way, but at the price of making it tautological. An extremely illuminating example of such attempts is Lea's description of how economists reacted to the observation that money is unacceptable as a gift although non-monetary gifts imply mostly an inefficiency since only the consumer himself can make the best use of a given amount of money. Actually many attempts to reconcile economic theory and empirical evidence are nothing else than posterior and often rather ad hoc-adjustments of the optimization tasks which make the solutions consistent with the available evidence (see, for instance, McKelvey and Palfrey, 1992). We suggest to name this the "neoclassical repairshop".

One may add that such studies are not always useless in view of economic psychology. In economics or econometrics one often does not validate a model by testing its assumptions but restricts oneself to testing only its recursive form

which often looks like a psychological hypothesis. The concept of a demand function which usually is derived by household utility maximization can, for instance, be considered as a behavioral hypothesis describing how human decision makers react to the various stimuli involved.

Also perfect rationality is often a useful point of departure and reference for more realistic approaches to explain economic behavior. To ban studies of social interaction based on rationality since they do not explain real behavior would therefore mean to throw away the baby with the bath water. As Lea describes it, there may not even be a psychological alternative at present or, if alternatives exist, there may be an abundance of them with little guidance available when having to choose one.

To avoid any misunderstanding Lea distinguishes between mechanistic and descriptive rationality. Whereas mechanistic rationality refers to the process of choice by weighing consequences, descriptive rationality only claims an optimal outcome. Another distinction he makes is between substantivist and formalist rationality: According to the substantivist's point of view rationality is a hypothesis about human behavior which can be falsified empirically, e.g. by appropriate laboratory experiments. The formalist approach relies on rationality purely as a way of framing human decision behavior. Whatever the empirically observed behavior is thought to look like, one can and has to find an optimization task which implies it. So in spite of the inefficiency of non-monetary gifts one often refrains from giving money as a gift since non-monetary gifts express that one cares more. Very often the dialogue between psychologists and economics suffers from the fact that economists are relying on the formalist approach without realizing it. Or in other words: Economists often pretend that rationality is an empirically testable hypothesis, but they do not accept any falsification.

When discussing substantivist rationality Lea reviews the usual justification for rationality, like that rational behavior is the only one surviving in the process of Darwinian selection or of cultural evolution, or simply the result of competition among the possible behaviors, or that it can be implemented as human behavior by appropriate training, e.g. in business schools. They all have their basic flaw: To become, for instance, a rational chess player all the resources in the world do not suffice. So rational chess playing can neither be the result of biological or cultural evolution or competition nor will one ever be able to train people to play rationally (the capacity of the human brain is much too small to store a sophisticated chess strategy).

We disagree with Stephen Lea's plea for the formalist view of rationality although it may have some advantages, e.g. the one that it might inspire more cooperation between economists and psychologists. Rephrasing all human decision making as a static, i.e. non-dynamic optimization task will, however, prevent us from exploring more thoroughly the dynamics of human cognition and decision making. When facing a decision problem we often try to relate it to some previ-

ous decision problem with which we have some experience. If there exists such an analogous situation, we might rely on our previous decision behavior if this has been reasonably successful. Otherwise we might try to develop some simple cognitive model which allows us to predict the consequences of the main decision alternatives. Only if all of them are rather unacceptable but important, will we engage in a more thorough cognitive task to make as sure as possible at reasonable costs that we do not make a serious mistake. Clearly, the resulting behavior will crucially depend on the stage of this cognitive process on which a decision is made. Thus the same economic decision maker would have to be described by different optimization tasks depending on whether his decision is made after a superficial analysis, i.e. in the early stages of his dynamic decision making process, or after a more sophisticated one in the later stages. Strict adherence to the formalist approach therefore has to face some trouble.

We, however, completely agree with Stephen Lea's claim that the model of economic man or homo economicus provides a unique point of reference to systematize and judge the great diversity of psychological theories about human behavior. To use the concept of rationality in this sense, however, does not require the formalist point of view since it would mean to buy a cow just to drink a cup of milk. But Lea is, of course, right in his plea that the unproductive and probably never ending dispute about the rationality issue should not prevent us from exploring economic behavior. There is a risk that economic psychology and also experimental economics behave like Buridan's donkey which could never decide between two bushes of equal attraction and which consequently died of starvation. To expect that there ever will be only one accepted methodology in economic psychology is probably an illusion and may be an unwarranted one since a broader diversity might bring about novel and unusual ideas about how to model economic behavior.

Klaus Grunert: Cognition and economic psychology. Research on Economic Psychology is stimulated primarily by problems to be solved in fields of practice like economic policy and marketing. One wants to know, for example, what kind of experiences and expectations people have in order to predict how they respond to changes in taxes and subsidies, allocate their time to work or leisure, choose among different supplies of goods and services, etc. In answering such questions, it would be a waste of time, if ad hoc theories were constructed and tested without looking around what basic psychological disciplines like general psychology and social psychology have to offer. Thus, the main stream of economic psychological research is heavily influenced by theories and methods developed within the basic disciplines of psychology, often with a time lag of five to ten years.

Grunert's chapter on psychological models of cognition is meant to focus the reader's attention to recent developments of cognitive psychology, thus accelerating the process of implementation and applications of new theoretical develop-

ments. Mental processes have been the principal object of psychology in its early stages at the end of the 19th and at the beginning of the 20th century. The behavioristic orientation, originating in Pawlow's and Watson's ideas, which widely determined the psychological research up to the sixties, did not care about mental processes, although there have always been psychologists who did not follow the behavioristic verdict on theories of mental (conscious) processes. A widespread interest in modeling cognitive processes came up again when the availability of advanced computer technology arose some hopes for programming artificial intelligence. Referring to the consecutive stages of information processing (selective attention, interpretation and integration, storage, retrieval, and problem solving), Grunert reflects on the usefulness for economic psychological application of models developed in basic research. He stresses the point that cognitive psychology is not restricted to conscious information processing but comprises also the functioning of cognitive processes of which the subjects are not aware and not able to talk about.

Typically, cognitive psychology did not pay much attention to the functions of emotions and mood in analyzing behavior and its cognitive antecedents and correlates. Bower (1981), and a number of other researchers following his ideas, found a) that people do better in remembering if they are in the same mood as they were when they stored the information to be remembered, b) when the affective tone of the event to be remembered corresponds to the subject's mood at the time of storing and of remembering. Thus, we may expect, for example, that people who are discontent, because of their present economic situation or because of some other reasons, remember bad things from the past and are particularly sensitive to bad news. This, of course, influences their expectations for the future, and any effort to bring about a more optimistic view has to overcome these self-reinforcing tendencies of pessimism. One obviously can design models of business cycle based on such basic psychological observations: Bad news, e.g. about tax increases, inspires discontent and reduces thereby demand whereas good news can trigger a boom.

How mood affects people's readiness to follow or to resist a persuasive argumentation has been studied in a series of sophisticated experiments during the past ten years (see Schwarz, 1990; Forgas, 1992, for overviews). There is a clear evidence that people in a positive mood are less inclined to a thorough and detailed analysis of the available information than people in a neutral or (mildly) negative mood. In particular, the quality of arguments in evaluation of and action toward an attitude object makes a difference only for persons in a neutral or negative mood. It is assumed that bad mood functions as a kind of warning that there are problems around which need a careful scrutiny and suggest a deeper elaboration on the available information (cf. Petty & Cacioppo, 1986). Obviously, this kind of research is highly relevant for a better understanding of how people respond to persuasion in politics and marketing.

Klaus Grunert: Psychological aspects of strategic management. Economic psychology is still a field of research where we find disagreement about its extension and internal structure. Some economic psychologists or behavioral economists see the main goal of their endeavors in linking psychological and economic theory in order to better understand how individuals are affected by and exert influence on changes in the general (national and international) economic environment. Part of these changes result from economic policy guided somehow by economic theory in pursuing political goals. There are others who want to make use of interdisciplinary (economic and psychological) research for improving the efficiency of firms in production and marketing. Grunert's chapter on the psychological aspects of strategic management represents insofar a link between these two orientations as it clarifies how management in business organizations has first to figure out what kind of changes in the larger economic environment are to be expected, and second how competitive advantages can be detected and transformed into planning and implementing efficient strategies. Although the distinction between top-down and bottom-up marketing may not be as essential as the author stresses, it points to an important shift toward a more comprehensive assessment of the changes on the market, in particular of the changing preferences of the customers and the changing strategies of the competitors. Whether national or organizational welfare is the primary goal, the economic and psychological theories relevant to promoting such a goal may not differ very much. However, the different schools of thought in strategic management to which the author refers pose the question of how scientific knowledge and practical experience can be optimally combined. This is, of course, not only a problem of organizational management and economic policy, but of any field of practice. Studying the interplay between intuition (rooted in everyday experience) and reflexion (based on theoretical models and scientific methodology) may be a particularly rewarding task for economic psychologists in the field of strategic management.

Karl-Erik Wärneryd: Economic psychology and telecommunications research. An example of some overlap between marketing of specific products and the economic psychologists' concern for the behavioral aspects of macro-economic processes is given by Wärneryd's economic-psychological look at telecommunication research. On the one hand, companies producing and selling new telecommunication equipment are interested in the preconditions of adopting a new technology by firms and households. On the other hand, technological development and implementation in the field of communication affects the economy as a whole and the organization of work in the companies and in the public administration to a very high degree, and knowledge about the social structures and social forces behind the adoption of new means of communication is significant to any attempt to analyze the behavioral and economic implications of technological change. Therefore,

studying telecommunication may be not less central to economic psychology than studying saving and spending, deciding for work or leisure, coping with unemployment, and reacting to taxes or transfer payments.

Wärneryd's chapter on telecommunication does not intend to give an overview of empirical research on telecommunication. It rather reflects on principles of such a research, on different perspectives, on differences between the psychological and the economic approach, and it illustrates some of the problems by a study on the implementation process of TELEFAX in Sweden performed by Wärneryd and his associates in 1978 and 1988. The chapter also suggests, although it does not elaborate on it in more detail, that institutions and legal regulations play a major role in the way innovations become effective. As yet, economic psychology, focusing on people's preferences, perceptions, expectations, and behavioral intentions, often seems to neglect the influence of institutions and social structures in general.

Werner Güth: Distributive justice. A behavioral theory and empirical evidence. A central problem, both in economics and economic psychology, is the allocation of scarce resources. Often economics in general is understood as the science of making the best use of resources whose demand exceeds supply. Similarly, economic psychology is and has been interested in studying intrapersonal and social conflicts caused by such scarcities.

Whereas in market economies markets are the main institution deciding about the allocation of scarce resources, the basic idea of socialism is that a central planning institution should determine the final allocation or at least some of its basic aspects. No existing economy, however, relies either only on markets or only on a central plan, i.e. even in market economies people cooperate instead of trading via anonymous markets, and also socialist economies have to rely on markets subsidiary.

The allocation of scarce resources when several individuals cooperate voluntarily is usually analyzed as a problem of distributive justice. Such problems exist in market economies, e.g. when sellers form a cartel and have to decide about their quotas, as well as in socialist economies, e.g. in a Kibbutz when deciding who should do which work. Especially distribution conflicts in relatively small groups of interacting individuals have received a lot of attention, both in economics as well as in (economic)psychology.

The first chapter by Werner Güth, can be described as an attempt to develop a behavioral theory of distributive justice based on the psychological concept of equity. According to equity theory distribution conflicts come up by a production relationship according to which the interacting individuals create the group's success or total reward by their individual contributions. The simple idea of equity theory is then that every individual should be rewarded proportionally to his or her contribution, i.e. every contributed unit should be treated equally.

The idea of equal treatment has, of course, received a lot of attention also in economics. Actually most bargaining solutions (see, for instance, Nash, 1953, or Kalai & Smorodinsky, 1975) rely on ideas of equal treatment. As usually in neoclassical economic theory, rewards and contributions are, however, measured by their utility effects. Now from a psychological point of view the concept of utilities can serve at most as a point of reference or of conceptual departure when analyzing human decision behavior. Furthermore, even if utilities did exist, there would be no way to verify them interpersonally. Thus there would be no way to control whether equity norms for utility based rewards and contributions are fulfilled or not.

This explains why Güth develops general conditions which an economic variable has to satisfy in order to serve as a measure of reward or contribution. Of course, such a variable has to represent a scarce resource. The condition of interpersonal observability and measurability, for instance, rules out utility based variables in case utilities would exist. Other conditions like relevance meaning that contributions should reflect effort and rewards the degree of enjoying the group's success are far more vague.

Güth readily admits that he does not provide a general algorithm by which one can determine the allocation results for all possible distribution conflicts. Since there are infinitely many such situations, varying both in their economic and social structure, it would be naive to expect that one can define the allocation results for all these quantitatively and qualitatively different distribution conflicts based on a few conditions. As a neoclassical economist is unable to specify the utilities of all interacting individuals in all economic situations and therefore also unable to predict the economic allocation, a behavioral economist, too, will hardly ever be able to predict the economic results for all possible situations.

Another step to develop equity theory further is the consideration of situations with competing standards to measure rewards and/or contributions. To have something specific in mind consider, for instance, a situation where the individual contribution could be the working time or the number of pieces produced. Güth argues that in such situations there is often a natural hierarchy of standards in the sense that a superior standard to measure rewards or contributions requires stricter observability and measurement conditions than a more basic one. To determine, for instance, the number of pieces produced one does not only have to know working time but also the productivity per unit of time.

Güth's main hypotheses claim that a superior standard substitutes a more basic one if its stricter requirements can be guaranteed at reasonable costs and if it is expected to yield an essentially different allocation, that a superior standard, however, will only be substituted by a more basic one if both imply nearly the same result and if the basic standard is more cost effective, and that in case of minor results one usually will rely on more basic standards. Güth demonstrates the predictive power of the behavioral theory of distributive justice by explaining some well-

known experimental results and also by applying it to some real-life distribution conflicts as, for instance, cost allocation in condominiums.

Friedrich Schneider: Public choice - Economic theory of politics: A survey in selected areas. Since economic behavior accounts for a considerable part of human behavior, it is natural to analyze economic behavior from a psychological perspective like all kinds of human behavior. Of course, one might ask how economics could develop at all as a scientific discipline without being based on psychology. One answer is that now and then economics did rely on basic psychological ideas. More important is, however, that in economics one traditionally has assumed individual rationality which, from a psychological perspective, is an extreme idealization since human decision makers can be at most boundedly rational.

To give an example, consider again the game of chess. Here a minimal requirement for individual rationality is that both players choose a strategy, i.e. a complete behavioral plan relying on counterfactual considerations, since otherwise rational chess playing cannot be defined. Counterfactual considerations are entertained when evaluating choices which one can exclude given the present state of knowledge, e.g. about the own behavior or the own type of person. But a chess board has more board constellations than a human brain can capture, i.e. choosing non-trivial strategies is impossible. Clearly, complex problems in economics like management decisions in large enterprises which are active on various markets are of a similar complexity. So individual rationality is as unrealistic in economics as it is for chess playing.

In spite of its unrealistic trust in individual rationality the economic approach, i.e. the model of economic man or homo economics, has, however, been very successful and influential in all social sciences. Especially, in the theory of political decision making one often relies on the "economic approach". Actually, economists themselves started to explain political behavior which formally has been considered as being exogenously determined. This approach, which is selectively surveyed by Friedrich Schneider, is usually named "public choice (theory)". The main idea in public choice is to separate the incentives of political actors like democratically elected representatives and those of their clientele, e.g. their voters. Both preferences will usually differ. A member of parliament may, for instance, care more for his reelection than for keeping his promises on which voters' decisions have been based. Thus we face a principal-agent-problem, similar to the one in large firms (see the second paper by Güth and the respective introductory remarks).

Although the paper by Friedrich Schneider reviews only the economic approach to political decision making, i.e. describes the state of art in public choice theory, we consider it as important for economic psychology: If psychology wants to substitute the model of economic man by more realistic models of human decision making, it has to follow closely economics even if economists start to study phe-

nomena which traditionally have been analyzed by other scientific disciplines only, e.g. the political sciences.

An especially interesting aspect to which Schneider refers and to which economic psychology could definitely contribute a lot is the difference between representative democracies, where principal-agent-problems are more serious, and direct democracies with less serious principal-agent-problems, but, as is often argued, more selfish voting behavior. Often justifications for one of two forms of democracies are based on oversimplistic theories which probably will not withstand a more thorough psychological analysis. Why, for instance, should a voter not care for social security if asked to vote for it? To assume that voters will simply minimize their tax burden is much too simple. First of all, many voters will vote ethically, i. e. by neglecting their very personal circumstances. In more philosophical terms this could be described as if a voter tries to move behind the veil of ignorance (see Harsanyi, 1955; Rawls, 1972). Secondly, why shouldn't a voter develop the same kind of responsibility as an elected representative when faced with similar decision problems? The fact that Switzerland still relies on elements of direct democracy should provide simple opportunities for interesting field research, most preferable by economic psychologists who are familiar with both, the psychological and the economic approach.

Naturally, public choice theory stresses the importance of the economic situation, both for voting behavior as well as for policy (see Figure 1 of Schneider). Due to the statistically valid dependency of voting behavior and the actual economic situation governments are interested in having a boom just when an election is due. Some countries give the government even some freedom in timing an election what allows for an even better adjustment. Thus democratic elections can cause a politically induced business cycle. To overcome such unwarranted incentives one can, of course, spread an election over time, e.g. by sequential voting in the various districts. Also in direct democracies the risk of politically induced policy cycles is much weaker due to the more restricted discretionary power of the government.

It is an interesting question for economic psychology why voters only react to the actual economic situation instead of considering performance over the whole period of legislature. In case of extremely limited recall of democratic voters this would mean that democratically elected governments can have time restricted dictatorial power what is certainly undesirable. Another problem for economic psychology is why and to which extent voters hold governments responsible for their personal well-being. Could it be that we attribute success to ourselves, e.g. as successful entrepreneurs, whereas we hold the government responsible in case of a failure? Or do we simply vote for the government in case of a boom since we do not want to "change a winning team"? In our view, this illustrates that public choice confronts economic psychology with many new exciting research questions.

Schneider discusses how other political actors, e.g. the Federal Reserve Bank or trade unions and their counterparts, can be incorporated when trying to model the economic and political situation of democratic market economies. Of course, also international economic and political relations are important and can be considered, too. In his conclusions Schneider argues for direct democracy (since it weakens the principal-agent problems) and for more local policy decision making since it allows a more efficient control/monitoring of public expenditures and thereby a grater willingness to pay taxes. Psychologically it seems very convincing that one is much more willing to accept a given tax burden if one can see that it is used efficiently for something useful, e.g. for an improvement of the local economic infrastructure, and if we can react politically if taxes are used inefficiently or for some rather debatable goods, e.g. for political representatives' luxurious office equipment.

Friedrich Schneider: Measuring the size and development of the shadow economy. Can the causes be found and the obstacles be overcome? Regardless whether an economy is a market economy or a socialist economy, there are usually frequent economic activities which circumvent the legal regulations, especially those implying costs as, for instance, tax rules. The part of the economy which consists of all these illegal economic activities is called the shadow economy.

Radical defenders of market economies sometimes welcome the shadow economy and would not mind at all if its importance increases. They would justify this by complaining about the complexity of the legal rules in modern economies which nobody can overlook and therefore not obey. For the Federal Republic of Germany it is said, for instance, that more than 20.000 legal rules apply to the act of hiring a worker. If this is true, learning all these rules requires more effort than an employer will usually want to invest. To know all these rules and to understand them is hardly possible, even for legal experts.

It is, however, very questionable whether illegal economic activities are more efficient than legal ones although even more moderate proposes of market economies consider the legal institutions as overrestrictive and maybe even inconsistent. To demonstrate this consider an illegal delivery, e.g. building a house by black labor, whose quality turns out only in the long run. If the quality finally turns out to be miserable, the customer has no right at all to ask for an improvement, a repair, or a financial compensation whatever may be appropriate. Even if the house collapses, he would be held responsible. Like on a lemon market (see Akerlof, 1970) one might therefore expect only worst quality deliveries. This demonstrates that the shadow economy is by definition not more productive than the legal one although the latter may not be at all a "sunny economy".

The main intention of Schneider's empirical investigation of the shadow economy is to estimate the relative size of the shadow economy as well as its development in time, especially whether it is increasing as frequently conjectured. One

reason why this may be relevant is, for instance, that the government might be interested in tax revenue. If the shadow economy increases with increasing tax rates, the government may face a tax revenue curve which is shaped like the revenue curve of a monopolistic seller. Especially, one can imagine that the same tax revenue could be achieved with lower tax rates.

By the very definition of the shadow economy it should be obvious that there is no easy way to estimate the size of it as well as its development. Schneider surveys the methods which a social scientist as a detective spying out the shadow economy might use. Schneider himself favors the model approach which allows for multiple causes as well as for multiple effects of the shadow economy and whose empirical estimation is based on the statistical theory of unobserved variables.

The empirical results indicate, for instance, for Austria that the shadow economy has increased from 1.59 % of official GNP (gross national product) in 1975 to 4.70 % of official GNP in 1990 which, beyond doubts, is a radical increase and which certainly requires dramatic policy changes if one wants to stop or at least to slow down this development.

It is interesting to compare Schneiders's approach to the research tradition of illegal economic activities in economic psychology. Basically Schneiders's theoretical approach is macro-economic, and his empirical procedure is an econometric one. This usually means that one relies on data sources, provided by the statistical offices or similar institutions, and that one applies - at least in the case of Friedrich Schneider - highly sophisticated statistical estimation techniques. In summary, one usually accepts the data base and tries to make optimal use of it.

In economic psychology one usually restricts attention to a more specific type of economic activities, e.g. the one of illegal tax evasion (see, for instance, Elfers, 1990). Unlike the typical economic or econometric approach a major concern is the collection of new data as needed by the theoretical problem. Methods for collecting such data can be questionnaires, interviews, or experiments (Lea, Tarpay & Webley, 1987).

Whereas the willingness to invest a lot of effort in collecting data is certainly desirable, the very selective choice of topics is more debatable. After all, evading taxes illegally is only one specific aspect of the shadow economy which comprises all kinds of economic crime. Can, for instance, the results of illegal tax evasion be generalized to other economic activities of the shadow economy? If yes, why don't economic psychologists try to develop a general theory of criminal economic behavior instead?

In our view, one needs, on the one hand, the more macro-economic and macro-econometric research tradition that uses in time, by statistical evidence which they can hardly reject, official data sources in order to warn policy makers. On the other hand, the more specific research methods are also important, especially if they provide new evidence for trends which otherwise couldn't be identified. We hope

that Schneider's article will help to keep economic psychologists, who are interested in the shadow economy or at least in parts of it, informed about the economic and also the econometric approach.

Arthur Schram & Frans van Winden: Why people vote. A critical question in public choice theory, discussed by Schneider and answered in a special way by Arthur Schram and Frans van Winden, is why people vote at all. Since in a large scale election it is highly unlikely that a single vote matters, there seems to be no obvious incentive to overcome the cost of voting. Schram and van Winden rely on an important aspect of a voting body, namely its partitioning into social groups. Within such a social group they distinguish between "producers of social pressure" to vote and "consumers of social pressure". Clearly, economic psychology can offer some ideas on how to answer the intricate social psychological problem "why consumers attach utility to giving in to pressure" (Schram and van Winden p. 215) which Schram and van Winden do not address. An economic psychology approach will not need to rely on utility maximizing consumers since yielding to pressure is a rather natural reaction of somebody who needs and enjoys social relationships.

A basic assumption of Schram and van Winden is that members of the same subgroup have similar interests, e.g. like workers who are interested in fighting unemployment. The example, considered by Schram and van Winden, are group specific tax rates. If more members of a subgroup vote, the group specific tax rate becomes more favorable. Although this is not an unreasonable assumption, one nevertheless would like to see a justification how and why tax rates should react this way. Is it that policy makers are also subjected to social pressure or is it simply proportionality? It seems interesting to approach this question by using the methodology of economic psychology.

In a technically more demanding way Schram and van Winden prove that there always exists an optimal group participation in an election as well as a vector of participation rates of all subgroups which is an equilibrium. That means that every participation rate is optimal for the respective subgroup if the others realize their equilibrium group participation (for a more elaborate discussion of the game theoretic equilibrium concept see Güth's second article in this volume). They try to analyze the intragroup relations by distinguishing producers of social pressure, who are characterized by a superior "(opinion-)leadership ability" which is definitely a psychological category, and consumers of social pressure. Economic psychology is without doubt well equipped to measure an individual's ability to induce others to choices they otherwise would not make. After all this is one of the crucial topics in consumer psychology, e.g. when firms try to create a desire for an unknown product which does not satisfy a basic need.

From a psychological perspective it is rather questionable that producers of social pressure merely want to close the gap between actual and optimal group par-

icipation. Leadership usually brings about many social advantages. This is rather obvious for animal societies (see, for instance, van Lawick-Goodall, 1974) but also in human societies political leaders have better access to resources. So there might be production of social pressure even if the group participation in voting does not indicate a need for it.

In the same way one can easily think of more convincing justifications why consumers yield to social pressure. Schram and van Winden simply assume "that individuals derive positive utility from giving in to social pressure". One may, for instance, test for leadership ability before asking leaders to persuade others to fulfill a certain task, e.g. a civic duty. Such experiments obviously provide a basis for exploring why consumers resist or yield to social pressure.

Nevertheless the study by Schram and van Winden seems to be a pioneering step in the direction of exploring the social relationships within a voting body, once by partitioning the voting body more thoroughly into more coherent subgroups, and once by distinguishing in every such subgroup producers and consumers of social pressure.

Due to the complex structure of an election including the producers' decisions how much social pressure to exert, and the consumers choices whether to yield or not, one can hardly perform any comparative statistics for the model developed by Schram and van Winden, e.g. by exploring how increasing costs of producing social pressure or of voting in one or all subgroups influence the election result. The authors are, however, able to prove that there always exists a temporary as well as a dynamic equilibrium. Whereas a temporary equilibrium considers only one period, a dynamic equilibrium requires the equilibrium property for a sequence of voting periods. Even for dynamic equilibrium Schram and van Winden, however, rely on myopic behavior, i.e. no actor strategically considers the impact of his present behavior on future elections. Nevertheless expectations are required to be rational in a dynamic equilibrium, i.e. one correctly anticipates what happens in future elections, one just doesn't think that these future events are determined by the own present behavior.

The authors also indicate how their model may be adapted to winner-takes-all elections where the winning party gets all the power. They also provide a numerical example to demonstrate the applicability of their model. Since many parameters of the model are of a psychological nature, a real application of the model will definitely have to rely on estimation techniques of economic psychology. This shows that a closer interaction of public choice theory and economic psychology is needed.

Werner Güth: How to avoid intrapersonal strategic conflicts in game theory? The final paper, by Werner Güth, discusses the problem of intrapersonal and interpersonal strategic conflicts. Whereas interpersonal strategic conflicts are social decision conflicts where several individuals with different interests and more or

less decision autonomy interact strategically, intrapersonal strategic conflicts result if one or more individuals have to make several decisions and if the (rational) future choice behavior is bad in view of earlier decision stages. An example of such an intrapersonal decision problem is, for instance, consumption with the risk of becoming addicted: It may be optimal for a consumer to consume a certain "drug" once. However, after consumption he may not be able to quit consuming. Anticipating addiction he therefore will not consume at all, i.e. realize a second best alternative. While discussing this basic conceptual problem of game theory, which has become the prominent methodology of modern economic theory, Güth introduces the main modeling techniques (game forms), solution concepts as well as some of the most debated applications of game theory (e.g. durable monopoly markets and the Coase-conjecture, repeated games with and without incomplete information). Although he discusses the behavioral relevance of the game theoretic results, mainly by referring to experimental observations, the main motivation of the paper is to introduce the essential ideas of game theory by focusing on one of its main conceptual problems which surprisingly has not been studied systematically before.

Why is game theory important for economic psychology, and why especially intrapersonal strategic conflicts? First of all, since game theory is widely used in economic theory, some basic knowledge of game theory is highly desirable also for scholars of economic psychology. Furthermore, many paradigms studied in (economic) psychology, e.g. social dilemmas like the prisoner's dilemma or the chain store paradox, public goods provision, repeated games, bargaining models, belong to the folklore of game theory and are usually described with the help of game theoretic terminology which Güth introduces. Of course, game theory as well as neoclassical theory cannot really explain how people behave in economic game situations. Güth himself admits this when justifying his behavioral theory of distributive justice.

In our view, it is one of the main future tasks of economic psychology and experimental economics to develop a behavioral theory of game playing which is based on psychological concepts whereas normative game theory is based on intuitively convincing but highly unrealistic rationality requirements. In order to develop such a descriptive theory of game playing some basic knowledge of game theory is certainly needed. Furthermore, game theoretic predictions often serve as a point of reference when describing actually observed decisions (see, for instance, Roth, 1994, for a more general survey).

The problem of intrapersonal strategic conflicts will be one of the crucial paradigms to demonstrate the differences between game theory and the behavioral theory of game playing. Game theory has been developed purely to solve interpersonal strategic conflicts, i.e. social decision problems. It is therefore neither prepared nor able to deal with intrapersonal strategic conflicts. Güth argues that game theory therefore should transform intrapersonal strategic conflicts into interper-

sonal ones. This can be done by relying on local players who only decide about one single move.

For a behavioral theory of game playing such an extreme conceptual point of view is definitely wrong. It is well-known and supported by many (experimental) results, e.g. the status quo-effect (see, for instance, Samuelson and Zeckhauser, 1982, as well as Tietz, 1992), that human decision makers do not always choose the locally optimal move but feel obliged by previous decisions which they, due to an ego-defensive attitude, try to justify. Thus to split up a person into several independent decision makers, as implied by the notion of local players, would yield wrong predictions. Unlike a rational player or more generally a homo economicus, a human decision maker is characterized by strategic inertia whereas the notion of a local player denies such dependencies on previous choices. The probably most important evidence for strategic inertia is that unlike the folklore wisdom in economics sunk costs matter.

The game situations which Güth discusses more thoroughly are durable monopolies, i.e. monopoly markets where the only seller can offer his product repeatedly. The, at first sight, counterintuitive Coase-conjecture (Coase, 1972) claims for such markets that market prices become more and more competitive when the number of possible sales periods tends to infinity. There are field examples, e.g. periodic sales of seasonal products whose price cuts are anticipated as well as the anticipation of cheaper paperbacks which reduces the sales amounts of the earlier and more expensive hard cover books, which prove that the Coase-conjecture is also behaviorally important. It is, however, very questionable whether the way people reason about their behavior is similar to the game theoretic procedure of backward induction (see Güth & Ritzberger, 1992).

The experimental results of Güth, Ockenfels, and Ritzberger (1992) for durable monopolies with 2 or 3 possible sales periods indicate that human decision makers have great difficulties to compare the present and future effects of certain pricing decisions. Even students, who learned the game theoretic solution before, did not behave in the way predicted by game theory. Also the related results of Rapoport, Erev & Zwick (1992) for a closely related situation with no explicitly stated time horizon were not always in line with the intuition of the Coase-conjecture.

The game theoretic concept of forward induction can be described as an equilibrium refinement (for an overview of such refinement concepts see van Damme, 1991). A game theoretic equilibrium is a strategy vector from which no individual player wants to deviate. Forward induction selects equilibria according to which earlier moves are seen as signals of later behavior what clearly is in conflict with the notion of local players. Such a selection, of course, assumes subgames with multiple equilibria. In our view, forward induction is an important behavioral concept since human decision makers usually judge others by their previous behavior. It should, however, be defined much more generally than in game theory. Gardner,

Güth, and Ockenfels (1991), for instance, show experimentally that forward induction type of behavior can already be observed in nonstrategic contexts, i.e. in the so-called one person-games.

Other applications of game theory, considered by Güth, are repeated games and principal agent models. The latter study the strategic situations within firms and more generally in economic organizations that can therefore be described as the normative analogues of problems studied in organizational psychology. With respect to the principal agent relationships there seems to be a wide gap between research in economics which is highly normative and heavily relies on game theoretic methodology and the more descriptive type of work in organizational psychology. We, however, expect that principal agent models will become one of the favorite paradigms in experimental economics and this, hopefully, will help to close this gap.

Repeated games are game situations where the same individuals play a base game repeatedly. The most prominent examples are repeated prisoner's dilemma games. For a repeated prisoner's dilemma game, theory predicts that players do not cooperate if the game is repeated finitely often but that nearly every type of result is possible if it is repeated infinitely often.

Repeated games have been studied extensively, both by experimental psychologists (e.g. in the context of social dilemma research) and by experimental economists. What is regularly observed is that experienced participants will usually start by cooperating regardless whether the repetition number is finite or not. Some researchers claim to explore the infinite horizon case although this is impossible (see, for instance, Weg, Rapoport & Felsenthal, 1990 as well as Rapoport Erev, and Zwick, 1992). They usually just refrain from imposing an upper bound for the number of repetitions explicitly. In case of finite horizons participants usually defect from cooperation before the end, i.e. there is a termination-effect which proves that participants are aware of the game theoretic logic's which unravel cooperation completely.

Thus game theory predicts no cooperation for situations where cooperation until shortly before the end is a stable and reliable phenomenon: Since the game cannot be repeated infinitely often, there exists a last round where non-cooperation is the only equilibrium. Thus also in the second to last round one will not cooperate since this could only be justified by its effect on future cooperation. Repeating this argument thus yields general non-cooperation. In game theory this has inspired the so-called "gang of 4" or "crazy perturbation"-approach (Kreps, Milgrom, Roberts & Wilson, 1982) according to which all players expect an opponent who cannot defect at all from cooperation and whom they try to mimic in order to induce cooperation.

In our view, reputation effects are behaviorally very important; after all what scientists care for is a good scientific reputation. But again the game theoretic way of studying reputation effects has little to do with the way it is actually considered

by human decision makers. Game theory, for instance, requires highly complex and difficult considerations of backward induction based on Bayes-theorem, which is known to be behaviorally wrong.

This again demonstrates the urgent need to develop a behavioral theory of game playing, similar to the behavioral theory of distributive justice, outlined in Güth's other contribution to this volume. One should try to resolve the inconsistency of game theory and actual game playing behavior by using psychological concepts instead of "repairing" the game structure in an ad hoc-way (see, for instance, McKelvey & Palfrey, 1992).

For this Güth's article can provide at most some inspiration. Of course, also when outlining a behavioral concept game theoretic terminology will be very useful although strategically equivalent games, in general, may not be behaviorally equivalent (see, for instance, Nydegger & Owen, 1974, Roth & Malouf, 1979). This is the main reason for including Güth's article: To prepare the stage for developing a descriptive theory of game playing for which normative game theory hopefully will provide some guidance and inspiration in addition to serving as a point of reference and departure.

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Chapter 2

Psychology + Economics = Economic Psychology?

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Summary

In the early nineteenth century, economics was the study of pleasure and pain and psychology the study of the philosophical problems of mind and body relationships. When psychological research started dealing with pleasure and pain, physiological aspects dominated. Economists focused on societal problems and thought that all humans acted and reacted in the same, rational way whereas psychologists more and more stressed individual differences and laws referring to individual behavior. Economic psychology while being today mostly an extension of psychology to the study of economic behavior, can learn from economics and incorporate more of economic thinking and economic variables into the research. It would then become more truly interdisciplinary.

Key words

Economic psychology, history of economic psychology, applied psychology, economic behavior

Economists and psychologists

Who is an economist?

An economist is according to the dictionary someone who is a specialist in economics or someone who is economical (Webster's, 1977). What type of training does it take to become an economist? The obvious answer is: training in eco-

nomics. I shall give a pertinent long quotation from a British economist, G. Shackle, who is very well known among economists for his creative thinking (see Frowen, 1990). In 1955, he wrote: "to be a complete economist a man need only be a mathematician ..." That was, however, not the whole truth, so he added a few things: "a philosopher." Even this was not the end of the requirements, in the third place there follows: "a psychologist" which I note with great pleasure. After that he added a number of specialties: "an anthropologist, a historian, a geographer, and student of politics, a master of prose exposition, a man of the world with experience of practical business and finance, an understanding of the problems of administration, and a good knowledge of four or five foreign languages. All this in addition, of course, to familiarity with the economic literature itself."

I tend to agree with most of Shackle's assessment of the need for specialized knowledge, in particular when he concludes: "This list should, I think, dispose at once of the idea that there are, or even have been, any complete economists and we can proceed to the practical question of what arrangements are likely to provide us with men who feel not wholly confounded when an important economic decision confronts them" (Shackle, 1955, p.241).

The gist of this thinking and an important implication is that if you are an economist, you can only deal with certain aspects of economic behavior. It illustrates the idea that one has to use interdisciplinary approaches in the study of economic problems. Shackle himself had a team with representatives from different disciplines, among them psychology.

Who is a psychologist?

A psychologist is defined as a person who is trained to perform psychological analysis, therapy, or research (Webster's, 1977). This definition hardly corresponds to the popular notion that everybody is able to practice psychology and that some people are good at it while others are poor in their exercise of psychology. In everyday thinking a psychologist is not necessarily thought of as good at using psychology. Psychological knowledge from research always competes with psychological knowledge based on everyday experience, intuition, and speculation. Psychology is associated with the study of single individuals. Economic problems at the aggregate level often have psychological dimensions, but are usually not conceived as the business of psychologists. Everyday psychology is often seen as more congenial than scientific psychology for handling psychological dimensions in societal problems. Judging from the mass media focus of interest and knowledge, clinical psychology is practically the only psychological area of real interest to the general public. People who have some knowledge about psychology as a field of study tend to think of psychology as being split between schools and ap-

proaches. To some extent this may be true, but scientific psychology can still contribute to better understanding of many problems in society. Cognitive psychology which to the general public is relatively unknown is one of the most important developing areas in scientific psychology and has attracted the interest of researchers in many other sciences where human behavior is concerned.

The objectives of the paper

The main objective of the paper is to present the field of economic psychology against a historical background of similarities and differences between psychology and economics. Psychology and economics have something in common. Both of them study human actors albeit under somewhat different conditions and with major differences in the focus on problems. Historically the two sciences owe much to philosophy and some of these historical roots are common at least in the sense that some philosophers successfully combined accomplishments in both fields. Economic psychology is mainly seen as a new branch of psychology which could and should learn more from economics. While economic psychology is then an extension of psychology to the study of economic behavior, behavioral economics is an extension of economics to include psychological concepts or even a demand for new psychological underpinnings for economic theory.

The common historical roots of psychology and economics

Mandeville and Smith

Psychology and economics have common historical roots in philosophy and early discourses on man and society. The Dutch physician Bernard Mandeville who emigrated to England had in "The Fable of the Bees" and in two volumes of comments to this long poem propounded that societies could grow and prosper only if all people pursued their self-interest. One of his most shocking ideas was that in a society where frugality was not necessary, people should waste money on luxuries: Private vice was public benefit.

Mandeville seems to have been a forerunner both to economics and psychology (Hayek, 1978). He believed that personalities were stable phenomena and that changes in behavior were thus caused primarily by environmental factors. He distinguished between individual behavior and aggregate behavior. This is evident al-

ready from the subtitle of his poem: Private vice, public benefit. In a society that was not forced to frugality, there should be no frugality.

If people who could afford it stopped wasting money, the effects on society would be disastrous. All those who were gainfully employed through the spending of the wasters, would lose their jobs. There would be overproduction, falling prices on property, and underconsumption. Keynes (1936) cited Mandeville when he noted the possible ill effects of saving on the aggregate level.

Adam Smith is seen as the founder of classical economics. He is worth mentioning also in the history of psychology since long before he published his "Wealth of Nations", he wrote a book on psychology with the title "The Theory of Moral Sentiments." Earlier economic thinking had focused on property and exchange of property, but with Smith it switched to an interest in human behavior. Human agents were introduced into political economy. The theory became known as "classical economic theory." Like Mandeville, Smith maintained that people should act in accordance with their preferences and pursue their self-interest. With a large number of independent decision makers equilibrium would be obtained and the economy seemed to be governed by an invisible hand. Over a hundred years later, Smith was accused by the French social psychologist Gabriel Tarde (1902) of having forgotten what he knew about human psychology when he formulated his economic theory. Tarde who wrote the first treatise on economic psychology thought that economic theory should have been based on relations between people ("l'interpsychologie") rather than on self-interest. People were according to him affected by what other people thought and did. This, of course, agrees very well with the findings of modern social psychology and consumer behavior study.

Nineteenth century developments in economics and psychology

Names like Jeremy Bentham, James Mill, and the latter's son John Stuart Mill appear both in the history of psychology and economics (Boring, 1950; Murphy, 1951; Blaug, 1985). John Stuart Mill's contribution to political economy which was first published in 1848 was for many years the leading textbook in England and highly influential on economic thinking. Mill developed psychological associationism which prepared the way for psychological laboratory experiments investigating the old problem of mind-body relationships.

A big step towards establishing economics as a science that was independent of philosophy and psychology was taken by Jevons when he published his study on political economy in 1871. Whereas Smith and Mill had contributed to the development of both psychology and economics, although in different contexts, Jevons's work was exclusively aimed at developing political economy as a science on its own. He notes in the preface to his work that some people had been

thinking that economics was so complete a science that it could not be improved. He was referring to Mill's much-admired work on political economy. Still, Jevons said he ventured to think that he could add something to political economy by introducing mathematics and ideas from physics.

Jevons treated political economy as a "Calculus of Pleasure and Pain". He suggested that the science should emulate physics and start with some fundamental principle: "The Theory of Economy thus treated presents a close analogy to the science of Statistical Mechanics, and the Laws of Equilibrium of a lever as determined by the principle of virtual velocities. The nature of Wealth and Value is explained by the consideration of indefinitely small amounts of pleasure and pain, just as the theory of statics is made to rest upon the equality of indefinitely small amounts of energy" (Jevons, 1911/1871, Preface). He cites Mill's assertion that "an obvious psychological law" should be the foundation for economics. A suitable starting point was the fact "...a greater gain is preferred to a smaller one..." From that Jevons went on "... we may then reason downwards, and predict the phenomena which will be produced in society by such a law."

The fundamental idea behind economic rationality is simply that you can decide whether you prefer something to something else. It may be noted that we all think we can and we all know that it is sometimes extremely difficult for fickleness and lack of information. The second part of the quotation proposes the use of deductive reasoning in economics. This is something that the economists have practiced ever since.

According to Jevons, the results of the deductive reasoning should be compared with reality as represented by statistical data. "That every person will choose the greater apparent good; that human wants are more or less quickly satiated; that prolonged labor becomes more and more painful, are a few of the simple inductions on which we can proceed to reason deductively with great confidence. From these axioms we can deduce the laws of supply and demand, the laws of that difficult conception, value, and all the intricate results of commerce, so far as data are available. The final agreement of our inferences with à posteriori observations ratifies our method. But unfortunately this verification is often the least satisfactory part of the process, because, as J.S. Mill has fully explained, the circumstances of a nation are infinitely complicated, and we seldom get two or more instances which are comparable." (Jevons, 1911/1871, p.18). Jevons goes on to say that he is not sure whether it is right to call the reasoning deductive since it usually starts with some observation of reality.

Economics became an empirical science with strict requirements for theory development on the basis of mathematics and dealt with aggregate data. Marshall (1947/1890) worked out a microeconomic theory which was presented both in words and mathematics. Later developments in economics have involved even more use of mathematics and deductive reasoning. A special branch, econometrics which was inspired by Jevons, deals with advanced data analysis.

While thinkers in economics or political economy as the science was mostly called had as a goal to understand economic problems at all levels in society, psychologists belonging to the new science of psychology took up philosophical problems relating to mind and body as their chief study. The main emphasis was on how physical stimuli influenced experience. Kant had declared that psychology could never become a science like physics since it could not be made experimental and it could not use mathematics (Boring, 1950, p. 254; Danziger, 1990, p. 19). His successor Herbart started using mathematics and later during the century theory and experimental methodology, primarily borrowed from physiology, developed in such a way that psychological experiments became possible.

Psychology became an experimental science with the laboratory experiment as its chief tool of investigation. Psychological problems at the societal level were seen as everybody's business. No specially aimed, systematic research was expected to facilitate the dealing with them. Around the turn of the century, psychological researchers started to use correlational approaches to serve as a complement to controlled experiments and also substitute for those. The first uses were probably in intelligence testing and personality measurement. Associationism came from John Stuart Mill's and Alexander Bain's thinking in such a form that experimental psychology became feasible, employing methods from physiological research. It continued as psychophysics, but also developed into such areas in experimental psychology as learning and memory, motivation, and communication. Psychophysics is still preoccupied with problems of how sensations, i.e. sensory experience, are related to external stimulus dimensions. Psychometrics is a quantitatively oriented branch of psychology, mostly using correlational methods.

Economists and psychology, psychologists and economics

Classical and neoclassical economists and psychology

Classical economists were preoccupied with supply and demand and essentially considered prices (value of goods) to be determined by the costs of the production factors employed. Smith introduced the human actor into economics. John Stuart Mill is said to have rewritten classical economics and focused attention on subjective value as a basis for price. Actually he has been criticized for not having a real theory of value (Blaug, 1985). In 1871, two books were published in which theories of value were developed. One was authored by Jevons as already mentioned and was based on marginal utility, expressed in mathematical terms. The other was verbal and written by the Austrian Carl Menger. Both used the subjective

value of a commodity to the individual as the starting point for their discussions of price formation.

Menger did not explicitly cite any psychologists, but was rather anxious to clarify which type of psychology was proper to the psychologists and which type to the economists. He declared that those economists who were grappling with the causal contexts of goods and causal laws connected therewith, were mistaken about the task of economics. He meant that such tasks that related to the nature of objects were a matter for the natural sciences including psychology and that economists should deal with the meaning of goods to humans. "We [the economists], on the contrary, have to deal with goods as means for human purposes, explore their context in the goal consciousness of the commercing men (their teleological context) and establish their laws" (Menger, 1871, p.21; my translation from the German original).

The basis for Menger's concept of marginal utility was the need hierarchy which is pictured in Table 2.1.

Table 2.1
Menger's Hierarchy of Needs

Needs ordered after urgency									
I	II	III	IV	V	VI	VII	VIII	IX	X
10	9	8	7	6	5	4	3	2	1
9	8	7	6	5	4	3	2	1	0
8	7	6	5	4	3	2	1	0	
7	6	5	4	3	2	1	0		
6	5	4	3	2	1	0			
5	4	3	2	1	0				
4	3	2	1	0					
3	2	1	0						
2	1	0							
1	0								

The basic idea is that need groups can be ordered in terms of how demanding they are and that there are levels of satisfaction. The most demanding need group is the need for nourishment (I). When some satisfaction is given, the urgency of this need group diminishes and another need group is attended to. Menger suggests that need group V could be the need for tobacco which is attended to when the four higher need groups have each received a certain degree of satisfaction.

A need theory which is similar to that of Menger was much later conceived by Maslow (1954). While Maslow's need hierarchy has received a lot of, according to

some, undeserved attention, especially in social psychology, it has not inspired the same type of quantitative precision as the theory of marginally diminishing utility.

With the introduction of value as a basis for price, new psychological foundations were created for economic theory. This was done with little help from the contemporary psychologists (see e.g. Coats, 1988). It is true that Jevons (1911/1871) cited the first British psychologist Alexander Bain in many places in his book. He used the expression: "As Mr. Bain says.." when he discussed pleasure and pain as a foundation of utility theory. Mostly he referred to Bain's work on emotions and will.

A non-mathematical approach to economics was presented by the Austrian economist Böhm-Bawerk (1912/1888) who was critical of Jevons's use of psychology and his attempts at quantification of subjective concepts. Böhm-Bawerk dealt more thoroughly with the motives for capital accumulation and for the rate of interest than other contemporary economists. He focused on the role of impatience and distinguished three factors that could lead to failing capital accumulation. One factor was imperfect information which Böhm-Bawerk expressed in terms similar to what is now called limited cognitive capacity. A second factor involved problems of will, which now would be called motivation or self-control. He added that professional psychologists might not make that distinction and treat the two reasons as one. He did not say why they would do this.

A third factor influencing the willingness to consider future needs is the degree of uncertainty about the future and the brevity of life. "A utility of 100 with a probability of 50 percent that we do not experience it, we certainly estimate differently than a present utility of 100, but rather like one of 50, and I am convinced that every one of us who is promised a birthday present of 100,000 florins for the 100th birthday, would be willing to exchange this large, but somewhat uncertain gift for a much smaller fraction of it in present goods." (Böhm-Bawerk, 1912/1888:448; the present author's translation)

Like many other economists of his time Böhm-Bawerk saw economics as based on psychological underpinnings. He thought those had to be understood by economists to enable the latter to develop economic theory. Here is an example of his fundamental ideas about economic behavior.

"Humans strive for happiness. This is the most common, although also vaguest expression for a multitude of strivings, all of which have the aim of promoting such events and circumstances that are as pleasant as possible to our feelings and emotions and avert those that are unpleasant. If one wants to change the words, one can say instead of "Striving for Happiness" also "Striving for Self-Composure and Self-Development", or "Striving for as much Life Enhancement as Possible, or just as well, also, "Striving for as complete Need Satisfaction as Possible." (1912/1888:6; the present author's translation from the German original).

It is interesting to note that Böhm-Bawerk was apologetic because he depended so much on psychological thinking in developing his theory of capital and interest. He lamented the fact that the psychologists had not yet attacked the psychological problems that were important to him. He made the observation that psychologists were looking for more general behavioral goals and that the finer details of economic behavior might escape the psychologists while the economists had become more aware of such things and thus could rely on their own observations. Böhm-Bawerk (in the 3rd edition of his book on capital theory) wrote a long, psychological afterword to the theory of value where he discussed the role of psychology in economics. He said that psychological thinking was necessary to develop the theory of value of goods. Mostly he had to rely on everyday observation since professional psychologists had not devoted any attention to the problems. He discussed the problems connected with crossing borders and stated his reasons for doing so, among them the fact that some economists had introduced dilettantish psychology into economics. One of those thus referred to was Jevons. The main argument against the latter was apparently that he had said that subjective feelings could not be measured, but must be replaced by objective measures that could be treated with statistics and mathematics. Böhm-Bawerk consequently attempted to show that the amount of feeling ("Fühlungsgrösse") could be estimated subjectively.

The founding father of neoclassical economics, Alfred Marshall, devoted considerable interest to psychological problems although he rarely used the word psychology or its derivatives. He titled Book III (there are six) in his *Principles of Economics* "On Wants and Their Satisfaction." He said: "Reckoning is made for the greater force measured by a shilling in the case of a poor man than a rich: but economics seeks generally for broad results that are little affected by individual peculiarities."--"Habit itself is largely based on deliberate choice."--"Economic motives are not exclusively selfish. The desire for money does not exclude other influences; and may itself arise from noble motives." (1947/1890, p. 14-28 in Contents)

Modern economists and psychology

With the developments of the 1930's psychology was more or less banished from economics. Robbins (1935) declared that rationality was a question of logic, i.e. consistency of choice, rather than psychology. If economic theory were to depend on the psychological science, economics would have to be rewritten every five years or so. Psychological science did not produce any stable results, but was troubled by fads and fashions. While Keynes (1936) discussed a number of psychological concepts, he did not make any reference to scientific psychology and what

is more important, his macroeconomic reasoning did away with all psychological concepts.

Many economists in the 1950's and maybe also today may have had a feeling that they knew enough to understand and guide the fate of a whole national economy and the world economy. Keynes's main work "The General Theory of Employment, Interest, and Money" which appeared in 1936 during the deep World Depression had apparently come as a revelation to many economists. Scitovsky (1986) says in his Preface: "Then Keynes's General Theory appeared and seemed to have everything we were looking for. Its effect was overwhelming." "...We felt at the time that Keynes had resolved all the really important economic issues and were quite ready to devote our careers to filling in gaps, resolving obscurities, and explaining, simplifying, popularizing and gaining converts to Keynes's macroeconomics." (Scitovsky, 1986, p. viii)

Macroeconomics was thought to be the theory that was needed for guiding economic policy measures, in particular the combat against unemployment. During the thirties and the following decades economists serving as advisors to government relied on Keynes's macroeconomic theory to "fine tune" national economies. After Keynes (1936) there was for some years a feeling among economists that they understood the working of the economy so well that they even could prescribe very precise measures to improve or control economies. After many failures, there is nowadays among an increasing number of economists the feeling that the knowledge and understanding may be inadequate and that interventions may have unexpected consequences. An alternative theory advocates so-called supply side economics which favors measures that create supply rather than demand.

There is apparently room for improving economic theory and maybe even for learning something from other sciences. Scitovsky (1976, 1986) who was an early and firm believer in Keynes has, for example, turned to psychology for help. There are essentially three stances for economists with respect to psychology (cf. Coats, 1988):

1. total rejection
2. adaptation of psychological concepts so that they become compatible with the rationality postulate
3. generative use of psychological theories.

Mainstream economists outrightly reject the idea of being aided by scientific psychology. Psychological phenomena are seen as epiphenomena, i.e., they are caused by other phenomena that should be studied in the first place. This implies that subjective data are unreliable and uninteresting and should be replaced by objective indicators, so-called proxies. Data from interviews with people are viewed with great scepticism and accepted only if they have turned out to have predictive value for example in an econometric model. As a psychologist, I sometimes wonder why economists readily accept data from income tax returns while they reject data from well-conducted interviews about a household's financial situation.

The life-cycle hypothesis of household saving can serve as an illustration of the nonuse of psychology. According to the hypothesis, the individual aims at having zero savings at his or her death. Economic rationality presumes that there is no real bequest motive that can disturb these calculations. The fact that many people leave a fortune for inheritance is explained away in mainly two ways. In the first place, it is asserted by e.g. Modigliani (1986) that such cases are few and unimportant. Secondly, people are not good at predicting their own death. Thus the precautionary motive which drives them not to risk being without money at any time explains that people leave money behind. For a psychologist it would seem simpler to recognize the fact that a preference scale may comprise the desire to bequeath something to beloved ones or to charity. Studies of wealthy people have indicated that the bequest motive is a very strong motive for the management of wealth (Barlow, Harvey & Morgan, 1966).

A second, considerably smaller group of economists are working on psychological concepts to make them compatible with economic thinking, in particular with the rationality postulate. A good example is the use of dissonance theory (Akerlof and Dickens, 1982). It is assumed that people can control their beliefs about events and states of the world and have stable preferences for their beliefs. This way dissonance becomes compatible with rationality. Thaler (1990), Thaler and Shefrin (1981), and Shefrin and Thaler (1988) have been working on making the concept of self-control tractable to economic thinking. The idea is to start with conflict between two forces one of which represents the demand for immediate satisfaction of needs, the other one is the long-term prospect of satisfaction in all future. Both agents, the first of whom is called "the doer" and the second "the planner", are assumed to act rationally, given their different preference scales. A rational person who thinks of the needs of the future finds ways of setting rules so that the planner can dominate to a certain extent.

A third group, consisting of a few economists, want to rewrite economic theory and replace the rationality postulate with something that is better anchored in psychological research. This involves generative use of psychology (Gergen, 1982): psychological research results are generalized to new areas and to contexts that are far from the original settings of the studies. Scitovsky (1976) based his attack at the rationality concept on psychological arousal theory. He pictures the human being as pushed by needs for comfort, stimulation and pleasure. These needs are satisfied through ways of living and of purchasing goods like food, clothes, automobiles etc.

Alhadeff (1982) made another attempt at rewriting economic theory from a psychological starting point. He expressed his admiration for a behavioral law from Skinner's psychology: "Intermittent reinforcement raises the probability of responding above the value generated when all responses are reinforced" (Alhadeff, 1982, p. 3). This is contrary to economic rationality which asserts that people prefer more to less rewards. It is also counterintuitive that behavior is better

learned when, say, only every fourth behavioral act is rewarded than when all acts are rewarded.

In accordance with Skinner (1971) Alhadeff rejects the idea that people select stimuli in their environment, evaluate those after their needs, and then act. Instead, the direction of the controlling relation is reversed: a person does not act upon the world, the world acts upon him/her (cf. Skinner, 1971, p. 206). Alhadeff builds a number of models that show how equilibrium can be reached. The main theme is how the consumer allocates income on purchase of goods, including financial services and assets. Purchasing is seen as a conflict between desire and sacrifice.

Skinner's thinking has met with appreciation by other economists. In behavioral economics in particular there has been an exchange of ideas between economists and Skinnerian psychologists (see for example Hursh and Bauman, 1987, for a review). Skinner emphasizes what happens in the environment and disregards factors within the individual. This has some similarity with the economist's thinking. As Simon (1990) points out, behavior can be forecast (more or less accurately), from environmental changes, by economists without contact with the human being. Skinnerians look for behavioral laws in which environmental changes and rewards play the important role.

Psychologists and economics

Mainstream psychologists usually know very little about economics and are not at all interested in studying or even discussing economic problems (except their own). Some even seem to think that economics is the science of promoting selfishness. Psychologists who have some knowledge of economic theory maintain a very critical attitude towards the rationality postulate. The postulate is seen as an unrealistic assumption that does not hold as a description of human behavior, in particular if the maximization of utility is restricted only to involve financial outcomes. The critique amounts to a claim that psychological research can give a better foundation for economic theory than the rationality postulate.

Behind the critique, there seems to be a misunderstanding implying that economists think that the theory is applicable to individual economic behavior. The assumption is rather used "as if": "Suppose that individuals on average and over time behave in accordance with the rationality postulate... Then the consequences at the aggregate level will be...". Predictions can be good, but explanations may be deficient using the assumption of rational behavior in the economic sense.

Böhm-Bawerk's discourse on psychology is said to have had some influence on the German act psychologists who later became known as Gestalt psychologists (Endres, 1988). Arrow (1963) hints that Freud may have been influenced by the

Austrian contemporary economists when he formulated his economic principle of allocation of mental energy.

In recent years there are examples of influence from economic thinking on psychological research. Research on decision making and game theory is almost a common undertaking between economists and psychologists. Subjective utility theory which was developed by economists and statisticians has inspired a lot of psychological research and led to the development of psychological prospect theory (Kahneman and Tversky, 1979) as a major competitor. Another example of a developing common field of interest is the collaboration of Skinnerian psychologists with economists (Lea, 1978; Hursh and Bauman, 1987). In studies of learning, mostly using animals as subjects, psychologists have adopted economic concepts like supply and demand curves, price elasticity, substitutability of goods, and equilibria.

Prospect theory is interesting from many points of view. It is well known both among psychologists and economists. It was first fully published in one of the leading economic journals which is ordinarily hardly read by psychologists. The basic assumption involves that people tend to be risk seeking to avoid losses and risk averse when they focus on gains. If offered a choice between winning something with certainty and something with only probability, they prefer the certain alternative even though the expected value of the probability alternative may be higher. If the choice concerns a certain loss and a loss with probability, the common preference is for the probability alternative despite the fact that the expected loss is greater. Kahneman and Tversky talk about "loss aversion" rather than "risk aversion" which is a standard concept in economic terminology. A person is assumed to frame alternatives before making a judgment and decision about the alternatives. There is now ample evidence that judgments will differ depending on whether a problem situation is framed as a loss or as a gain situation. This, of course, disagrees with the invariance assumption of economic theory which states that preferences do not vary according to how the alternatives are presented.

Prospect theory has found adherents and opponents among economists (Arrow, 1982; Smith, 1991). The authors have become involved in discussions with economists and while retaining their basic ideas developed the theory further and judging from their more recent writings they have been forced to learn somewhat new ways of thinking from economics (Tversky and Kahneman, 1986). They have had to defend their theory and many experiments testing (and mostly corroborating) the theory have been carried out by economists (Thaler, 1992).

The concept of bounded rationality which was proposed by Herbert Simon has been widely adopted in applied psychological fields like organizational psychology and consumer psychology. Simon who has received the Nobel prize in economics and the highest U.S.A. scientific award for his work in psychology recently summarized what he called "invariants of human behavior". He said about the economic rationality postulate: "Accepting this assumption enables economists to

predict a great deal of behavior (correctly or incorrectly) without ever making empirical studies of human actors." (Simon, 1990, p.6)

Simon (1990) suggests that a few cognitive principles such as recognition processes, heuristic search and serial pattern recognition, which together constitute something like intelligence, could be used instead of optimality as unifying principle. In such a case psychologists would presumably like economists now be able to predict a great deal of behavior without having any contact with human actors in a specific situation.

Business administration and psychology

In the business world there is since long cooperation between economics and psychology. Perhaps it should rather be stated as utilization of both sciences. Business administration which is a rather inadequate umbrella name for disciplines as diverse as accounting and finance, organization, and marketing has relied both on economics and on psychology in the development of theory and for the guidance of practical affairs. In the first two areas, organization theory and marketing there has been a clear dominance for theories and methods derived from psychology and social psychology whereas economic theory has had less to contribute. Accounting and finance long stayed as highly practical fields in which there was little quest for theory except generalizations from practical work experience.

Finance or, as it is now often called, financial economics has during the last one or two decades become a field in which the most sophisticated economic theory is used. There are some attempts also to develop behavioral finance, based on developments in cognitive psychology, especially risk judgments and decisions under risk (Thaler, 1992). Behavioral accounting focuses on how people use information from accounting and auditing reports. The research in this new field has been heavily influenced by cue utilization and human information processing research in cognitive psychology (for a review of experimental research, see Swieringa and Weick, 1982).

A comparison between psychology and economics

The way of thinking is quite different in the two disciplines of psychology and economics. Some characteristics for each of the two are given in the following overview (Table 2.2). These are not all the differences there are. They are the main differences as I see the situation. There are also similarities. Both sciences work with empirical data and use statistical methods for comparing hypotheses derived from theory with observations of reality. As noted earlier there are certain areas,

like decision making and risk taking, that are of mutual interest and where there is cooperation. A more detailed comparison is found in Wärneryd (1986).

Table 2.2
Some Characteristics of Economics and Psychology

<i>Economics</i>	<i>Psychology</i>
Founded on a few fundamental assumptions, by Becker (1976) called "The Economic Approach to Human Behavior": utility maximizing behavior, stable preferences, market equilibria	Mostly inductive, with low-level empirical theory. Concentration on explanation of individual behavior
Deductions based on these assumptions	Often strivings for descriptive detail
Mathematical language and specified models	Experimental and statistical methods, scaling methods
Objective data	Observational and subjective data, also on emotions and feelings
Interest in aggregate phenomena (macro)	Interest in general behavioral laws and individual differences
Assumptions about individual behavior serve "as if"	Assumptions about individual behavior must be realistic (agree with psychological knowledge)
Psychological concepts are interpreted in economic terms so as to be compatible with the rationality concept	Typically ignores context, structural, and system variables

While economics is something of a unitary science, with its basic postulate of rationality, psychology is divided and split up between many branches, both schools and domains of study. Experimental psychology as it came forward towards the end of the nineteenth century had a rather clearly defined paradigm which psychology somehow has lost with its diversification.

Courses in experimental psychology with laboratory experimentation and in testing theory were earlier mandatory for psychology students, but there seems to be more freedom of choice today. Students in economics all over the world are

commonly exposed to the rather fixed paradigm of economics which gives them a similar training and a common language.

Economic psychology

Economic psychology studies economic behavior at different levels in an economy: the individual/household level, the population group or market segment level, the mass market level, and the societal, macro level. Economic behavior involves choices and decisions implying the use of scarce resources such as money, time, physical and mental energy in order to satisfy needs. Factors affecting economic behavior as well as components of economic behavior are studied.

Choices are not necessarily made after deliberation. Choice by failing to make a decision is quite common. It can be called choice by default. Most consumer purchasing is routine or habitual behavior. Katona (1953) emphasized that the alternative to rational decision making was in practice not irrational behavior, but rather routine behavior.

The study of habit formation which usually has been pigeonholed under learning has been an important area of study in psychology. To what extent is economic psychology, and to what extent is it economics? Mostly research in economic psychology has been characterized by psychology as a starting point and dependence on psychology for theories and methods. The research has not been distinguished by its use of elaborate theoretical reasoning or specified models. It has produced results that are close to observations and have a low level of abstraction. Katona (1975) favored such research as an alternative to the highly abstract thinking in economics. His idea was to build economic psychological theory on simple generalizations from empirical studies of reality, mostly based on questioning the consumers and business decision makers.

The future, like the earlier, development of economic psychology depends in the first place on developments in basic psychological research. It is noteworthy that in an applied field like consumer behavior study the development has been parallel to interest streams in basic psychological research. Theories and methods have quickly found their way from basic research to the applied field of consumer behavior. Just look at some issues of the *Journal of Consumer Research* for evidence of this! Advances in social psychology have without delay been tried out in the consumer area. In recent years, the progress in cognitive psychology has been of great importance to economic psychology. Prospect theory (Kahneman and Tversky, 1979) has already been mentioned. Human information processing is a shared interest between cognitive psychologists and consumer psychologists.

Many studies in economic psychology employ theories and methods from social psychology, for example, in research on attitudes to taxes. While the main inspiration in the study of consumer behavior was for some years taken from human

information processing studies, there is now a tendency again to consider mood, feelings and emotions and other motivational concepts. The topic of mood and mood changes in purchasing processes is a new branch of this study.

An increased quest for better explanations in economics will mean that more interest among economists will be devoted to psychology. The movement of economics towards psychology has been called "behavioral economics". Like economic psychology behavioral economics seeks to improve the study of economic behavior by using psychological theories and methods. If one wants to make a distinction between the two, economic psychology can be said to be an extension of psychology to the study of economic behavior under some influence of economics. Behavioral economics is an extension of economics to include psychological theories and methods, but still keep the essence of economics.

It has become more and more obvious that economic theory cannot be used as the single basis for making good predictions of many future economic phenomena. Saving theory, as represented by the dominating life-cycle hypothesis, fails to explain much of saving and gives no clues for stimulating saving. Increases in unemployment are accompanying augmented inflation and stagnation. Inflation can apparently occur at the same time as stagnation, leading to the new term of "stagflation". In debates, prominent economists talk more and more about psychological factors that influence developments in inexplicable ways that cannot easily be explained in terms of traditional economic theory.

Expectations play an important role in many macroeconomic contexts. Rational expectations theory (Carter and Maddock, 1984) which is based on the idea that people in general form expectations in the same rational way as the best economists and econometricians has its shortcomings when applied to reality. For one thing, it is unclear how information is actually handled by the receivers. The concept of "credibility" has slipped into the discussion. If the information from a government policy maker is not credible, the treatment of the information will be different than if it is credible. Psychologists may know something about credibility and be able to measure both expectations and credibility which could be of some help to economic thinking.

One of the great difficulties is for psychologists to understand what economists expect from psychology when they are at all inclined to pay attention to psychology. John Stuart Mill provided them with "an obvious psychological law." This law was simple and general and the exploration of its consequences led far and is after 150 years still keeping economists busy. Keynes (1936, p.96) talked of a psychological law: The fundamental psychological law, upon which we are entitled to depend with great confidence both a priori from our knowledge of human nature and from the detailed facts of experience, is that men are disposed, as a rule and on the average, to increase their consumption as their income increases, but not by as much as the increase in their income." This is common-sense psychology and again a general law which holds on the average and over time according to

Keynes. Katona (1975) has a number of objections and questions its validity against what his empirical research tells.

In economic psychology we have taken the problems from economics in the sense that we have focused on aspects of economic behavior in various contexts: macroeconomic problems like taxation, inflation, saving, and unemployment; microeconomic like consumer behavior, decision making in business, and entrepreneurship. The thinking we have applied has mostly been direct loans from psychology. We may have learned too little from economics. Lea, Tarpy, and Webley (1987) stress the importance for economic psychology of studying context variables as well as individual variables. In another context I have presented my own, similar ideas (Wärneryd, 1989), and pleaded for more macropsychology.

It is desirable that economic psychologists and also other psychologists devote interest to discussions of psychological problems at the societal level. We need models that help us discuss problems without always asking for new data when we are asked, for example, by journalists to explain or predict certain economic phenomena. Examples of such questions are: What will happen to savings after a tax cut? What caused the crisis in the stock market and when will it end? How do consumers react when a war is threatening?

I do not pretend that economic psychologists have or can provide good answers to all questions of this type. In many instances no other science can give satisfactory answers. Psychology may then contribute to the elucidation and perhaps also solution of the problems by using and extending extant psychological knowledge, i.e. use it in a generative way (Gergen, 1982). The proper study of psychology and some knowledge of economics may be a good background for a psychologist so as to be able to advance the discussions of economic behavior in such situations. The proper study of economics and some knowledge about psychology may be the right foundation for an economist to find the questions tractable.

Conclusions

Dealing with psychology and economics, one runs: "...the double jeopardy of falling into serious error or deficiency in two fields rather than one" (Alhadeff, 1982). Researchers in economic psychology undoubtedly face this problem. Trying to understand and apply thinking from another science is always a danger if it implies critique of one's own specialty. There is a temptation to stay within the accepted paradigms.

Developments in economics may include the rise of a new type of psychology that is adapted to the (perceived) needs of economists. As shown earlier in this paper, economists have before created their own psychology when what they got from their colleagues in psychology was not found useful. Schumpeter (1954, p. 27) concluded on the basis of his review of economics and psychology: "Economists have never allowed their analysis to be influenced by the professional

psychologists of their time, but have always framed for themselves such assumptions about psychical processes as they thought it desirable to make."

It is also conceivable that psychologists may find economic behavior problems to be so intriguing that they want to establish their own economics framework. After all, economic problems are felt to be exigent both at the societal and at the individual level. Little is as yet known about the role of economic problems for being hit by neuroses and maybe even more serious afflictions.

The title of this paper has a question mark since I am not sure that I can or, for that matter, want to remove all doubts about the formula that economic psychology is the summation of psychology and economics. In fact, as economic psychology appears today it is not a real mixture of psychology and economics applying the best thinking of each of the two. It is dominated by psychology seemingly because of the strong empirical orientation in psychology, its new capacity of providing interesting findings on human behavior.

Psychological methods with some sophistication like the controlled laboratory experiment, multidimensional scaling methods, and survey techniques have proved their ability to provide useful data about human economic behavior. Experimental economics utilizes both hypotheses and methods from psychology (cf. Hey, 1992). Data based on survey results are entered in econometric analyses. Psychologists, too, are becoming interested in context variables (cf. Collins and Gunnar, 1990). Economic psychology will profit from attempts to apply more economic, especially macro-oriented thinking in the study of economic behavior.

Over the last decade, a lot of progress has been made in the direction of considering context variables in psychology. There is no doubt in my mind that economic psychology will prosper in the future as it integrates more of economic thinking into psychology.

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Chapter 3

The Evolutionary Biology of Economic Behavior

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Summary

This chapter asks why economic behavior has evolved in the human species. It starts by considering what definition of economic behavior would be adequate in a biological context, and concludes that neither scarcity nor rationality will provide one. Next, the question of whether animals other than humans show economic behavior is considered; it is concluded that in a limited sense they do, but that they do not have an economy in the sense of an organized system of division of labor and exchange relationships. Then the question of why this kind of behavior has emerged in humans alone is considered. It is concluded that human intelligence is not the answer, and that though human language might be, its evolution might equally be a consequence of the prior emergence of an economy. Rather, the origins of the economy seem to lie in the fact that humans are hunters who, being primates, possess hands and are in addition highly social. This argument leads to the surprising conclusion that economic behavior does not derive from biological selfishness, but from the less usual situation of natural co-operation.

Key words

Biology, division of labor, evolution, language, trade, money, co-operation

Introduction

The purpose of this essay is to pose what, so far as I am concerned, is an entirely new question. Why did economic behavior evolve in humans? Probably others, especially economic anthropologists, have asked this question before; evolutionary

game theorists have certainly touched on related issues (e.g. Selten, 1991). But so far as I know it has not been brought into economic psychology.

In asking that question I am taking for granted the answers to one or two other questions. First, I am taking for granted that the behavior of any animal species can in some sense be accounted for in terms of Darwin's theory of evolution by natural selection, and that the human animal is no exception. It is of course possible to argue that the huge variety of species of animals that we see around us have their origin from some source other than nature selection; but there is no alternative hypothesis that comes anywhere near convincing either me or the majority of biologists. There is continuing discussion over some of the details, but it is reasonable to take it for granted that the Darwinian analysis of where we came from and why is broadly correct. Secondly, I am going to take for granted that the sort of neo-Darwinist approach that goes by the name of sociobiology has at least some useful insights in it. This is substantially more controversial, and indeed I am not going to adopt a sociobiological approach hook, line and sinker, but I shall lean on sociobiological arguments to some extent. Finally, and least controversial of all, I am going to take it for granted that humans living in modern, "Western" economies do show economic behavior.

On the other hand, there are some things that I do not want to take for granted. First, we should not assume without discussion that it is only humans living in modern Western economies who show economic behavior. So I am not going to assume, without discussion at least, that economic behavior is a particularly modern phenomenon within the context of human history and prehistory, or even that it is a uniquely human phenomenon. In fact, we cannot make much progress with the question of the evolution of economic behavior unless we look a little more closely at what economic behavior is, and to what extent it is in fact a phenomenon that we only find in our own culture (or cultures like ours) and in our own species. In one way, asking about the evolution of economic behavior is, precisely, a vehicle for getting us to think more closely about what we mean when we say that we humans in our culture behave economically, and to think through what the implications of that are, at every level—not just the psychological and economic levels, but also the social and the biological.

It is tempting, of course, to play on words, and refer to that long standing joke within economics, the description of "economic man" in mock-biological terminology as *Homo economicus*. But this essay is not about the evolution of *Homo economicus*. The point about *Homo economicus* is that he (or she) is a fictional animal, who actually obeys the axioms of economic theory. A final thing that I am going to take for granted is that real, live flesh and blood humans do not obey the axioms of economic theory. They do not maximize, in any simple sense, and the only way we can say they are rational is by adopting the kind of formalist approach to rationality that is described in chapter 4 of the present book. Considerations about maximization and rationality will inevitably come into this essay, but

it is not about the evolution of rational behavior. It is about the evolution of economic behavior, which is something else. In order to get a grip on this question, we must now ask: What is economic behavior anyway?

The definition of economic behavior

The obvious place to look for a definition of economic behavior is in an economics textbook. They offer a variety, most of them unsatisfactory for our purposes. No doubt they are entirely suitable for the purposes for which they were intended, but things look rather different when we extend the context to cover the full range of human and maybe even non-human societies. Rather than surveying the whole field of possibilities, I want to pick up two common definitions that are thoroughly unsatisfactory when we put them into a biological context. Definitions, of course, are not absolute, because they enshrine theories (Popper, 1966); if readers disagree with the definition I propose here, that signals that we hold different theories about economic behavior.

Scarcity. First, can we say that economic behavior is behavior in the face of scarcity? Economics often is defined as the science of allocation of scarce resources, and it is argued that things only become an economic problem when there is a scarcity. Air, for example, only becomes an economic commodity in a submarine or space station. That may well be true, but it is not a sufficient definition of economic behavior. All animal species (and plant species too) face scarcity in one form or another, they all adjust to scarcity, and evolution drives the ways in which they make that adjustment. Because causality is nearly always circular at the evolutionary level, we could put this the other way around, and say that the way plants and animals adjust to scarcity drives their evolution. This, though, is the subject matter of ecology, and although ecology and economics are closely related, they are not the same thing. If we were to say that every animal that reacts to the scarcity of its food, or of mates, or of nesting sites in its environment, thereby behaves economically, we would have a drastically extended concept of the economic, and at the same time a very impoverished one. Economic behavior is more than that. Scarcity is a good candidate as a necessary condition for a behavior to be called economic, but it is not sufficient.

Rationality. Another plausible definition, which would also be radically unsatisfactory for our present purposes, would be to say that economic behavior is rational behavior. This is unsatisfactory for several reasons. Most fundamentally, the meaning of rationality is itself unclear, as I argue in the next chapter. To use rationality as a definition of economic behavior, we must first be more precise, and we must begin by distinguishing the two senses of rationality: rationality as a mechanism for taking decisions (Rational Foresight), and rationality as a descrip-

tion of a kind of decision people take (Maximization). It turns out that neither of those will do as a definition of economic behavior.

Rational foresight. Rational Foresight will not do because human beings only have very limited foresight, and a great deal of what must be economic behavior by definition goes on without any foresight at all. We do not sit down and think about the consequences of going or not going to work today, we just go; and if we tried to sit down and think it out, we would not be able to take everything that is relevant into account. On the other hand, there are things that we do plan very carefully (a love affair, perhaps) which we would not want to call economic behavior at all. Rational Foresight, therefore, is neither a necessary nor a sufficient criterion for economic behavior.

Maximization. But in any case, foresight is not usually what we have in mind if we say that economic behavior is rational behavior. Normally, we are referring to rationality in the sense of maximization. That need not be a very strong assumption; all we are saying is that whenever we are talking about economic behavior, and only then, we can use a maximization assumption of some sort. This is unsatisfactory for the same reason that defining economic behavior in terms of scarcity is unsatisfactory: much of modern evolutionary biology uses maximization models, which we as economic psychologists think of as rationality models, to describe the behaviors of animals of other species, and even of plants.

It is true that some of that behavior might plausibly be called economic. The next section of this essay reflects on three examples.

Maximization in animal behavior

Optimal foraging theory. Maximization models are particularly widely used in behavioral ecology, which studies the behavioral mechanisms by which animals fit into their ecosystems or "ecological niches". That "fitting in" involves a three-level system of interactions; the individual animal has to be seen in relation to its physical environment, its biological environment (that is to say, the other animal and plant species that exist in its environment) and its social environment (the other animals of its own species among which it lives). There is a large body of ecological theory that deals with the behavior of animals seeking food in their environment, and the optimization or maximization models it uses are collectively referred to as "optimal foraging theory". As an example, let us look at the feeding behavior of the spotted flycatcher, an attractive little bird found over much of northern Europe. In warm conditions, it feeds by sitting on a post or other prominent perch and waiting till a reasonably large flying insect comes by, then darting off and catching it. Davies (1977) studied flycatchers in a large garden and the adjoining churchyard in the countryside near Oxford, and described how they solved two problems associated with this "sit-and-wait" foraging strategy: (a) if a fly

comes by, is it worth the bird's while to fly after it? and (b) if not many flies have come by lately, is it worth moving to another perch? It is easy to find, mathematically, the optimal solution to these two problems, and Davies showed that the birds' behavior was consistent with the optimal strategy both for fly selection and perch selection (which are examples of general problems in foraging theory called the "diet selection" and "patch selection" problems). Specifically, the birds always took large flies, but only took smaller flies if there were not many large flies around, and the time they stayed at a perch was just long enough to maximize their overall success for the foraging period.

Part of the charm of behavioral ecology is that you can so often do it in your back garden in this way. But regardless whether the species involved is something exotic or something common or garden like a flycatcher, the scientific strategy is the same: find the most efficient kind of prey-seeking behavior, and investigate whether the animals actually follow it. In practice a few behave exactly as predicted, a few behave nothing like you predict, and the vast majority show behavior that is qualitatively like what optimal foraging theory predicts but differs from prediction in some details. What is not in doubt is the utility of the maximization approach, not just in formulating hypotheses to test, but also in producing reasonably accurate (and sometimes surprising) descriptions of behavior. So it is not just that maximization is used in behavioral ecology: it is also appropriate.

The theory of schedules of reinforcement. A second way in which maximization models have been used in the analysis of animal behavior is in the analysis of the effects of Skinnerian schedules of reinforcement. Every psychologist learns about the work of B. F. Skinner, arguably the last of the great pioneering theorists in psychology, who continued as a staunch advocate of radical behaviorism until his death in 1990. Although Skinner in fact ranged over a very wide variety of topics, he is probably best known for his early research on the behavior of rats and pigeons under food reward, and in particular for the various arrangements he invented or systematized for giving occasional reward to animals (Skinner, 1938; Ferster & Skinner, 1957). The responses concerned were called operants, and correspond to what in lay terms we call voluntary responses--Skinner's paradigm cases were a rat pressing at a lever, or a pigeon pecking at a switch. What Skinner discovered was that different rules for choosing some of the responses to be rewarded generate different patterns of behavior. He called such rules "schedules of reinforcement". If you make more than one response available, there is a huge range of possible schedules, and the behavior they generate can be quite complicated; but it is still highly reliable. There has been a lively and controversial literature that has attempted to explain why each kind of schedule has its distinctive effect on behavior. One of the most successful strategies has been to use some sort of maximization model, in which we assume that the rat or pigeon will maximize the rate at which it obtains rewards from the schedule. This idea does not command universal

assent (e.g. Herrnstein & Vaughan, 1980; Vaughan, 1981), but it has been used to great effect (e.g. Staddon, 1983, Chapters 7 & 8).

Simulations of economic behavior. A third way in which maximization models can be used for looking at the behavior of non-human animals is in the analysis of microeconomic experiments. It is possible to set up experiments that specifically test the mathematical axioms of economic theories using rats or pigeons, and to test whether they behave in ways that, if they were shown by humans, would unhesitatingly be described as maximizing utility. There is quite a large literature on this technique. Since I reviewed it for economic psychologists a decade ago (Lea, 1981), it has grown considerably both in size and in sophistication: some of the key researchers, such as John Kagel and Ray Battalio were economists by their original training, and they have applied the most modern forms of microeconomic theory to the behavior of rats and pigeons.

The problem with maximization. In all the three cases described above, maximization models have been used in exactly the same way as they would be used in economics. Yet most readers will not feel satisfied to describe them as cases of economic behavior, certainly not as clear-cut cases. If humans only did the sorts of things that we have described here, if we only went scouring along the hedges for blackberries, if we only hammered away on levers to get pellets of food, if we only lived in Skinner boxes for 24 hours at a time, choosing between lemonade and root beer (which is what the rats in experiments described by Kagel, Battalio, Green & Rachlin, 1980, did), would we really describe ourselves as having an economy? If that was all our economic behavior consisted of, I doubt whether we would even have a distinct term for it.

Nor do the problems with maximization as a definition of economic behavior end there. I chose those three examples because it might be plausible to describe them as economic behavior. But maximization models are also used in much less behavioral ecological problems, for example in describing how plants "decide" to develop as male or female (Charnov, 1984). Can we really call this economic behavior?

Economic behavior and the economy

Clearly animals do maximize. Of itself, that does not mean that maximization cannot be used as a definition of economic behavior; maybe animals other than humans do show economic behavior. Yet the examples we have considered seem to fall far short of what we normally study in economics. One way of grasping this difference would be to say that perhaps animals do show economic behavior, in a way: they do maximize, they do adjust to scarcity, and those are both parts of what we mean by economic behavior even if they are not satisfactory as definitions. But unlike humans, they do not have an economy.

What do we mean by having an economy as distinct from showing economic behavior? What is an economy? There are two obvious essentials that were missing from the animal behavior examples I used above. First, an economy is *social*. When we think about the experiments of Kagel, Battalio and their colleagues, it feels uncomfortable to talk about the rats as having an economy, because all they ever do is interact with an impersonal apparatus. In a sense, of course, they are interacting with Kagel and Battalio, but they do not know that, and there is no way they can come to know it; as far as they are concerned they are responding to a machine. The arrangement is asocial. When we talk about an economy in the human world, on the other hand, we are talking about something in which many people are engaged.

The second essential fact about an economy is that it is a *system*. That needs a little more explaining. Imagine an urban park on a summer's day, with a good-sized ornamental lake in it. If we go for a walk round the lake, we can be fairly sure of seeing some grandmothers throwing bread to the ducks on the lake, who will probably be squabbling over it. Is this economic behavior? The bread is a scarce resource, and the ducks are showing social behavior, even if of an aggressive kind. But do we want to call it economic behavior? Probably not; and one of the reasons is that the bread supply, though limited, comes from a cornucopia. The ducks do nothing to cause the bread to happen; it falls as it were from heaven. The fact that they squabble over it, or share it peaceably, has no further implications for whether more bread appears. But when we talk about an economy, we mean something that has a whole network of interactions; furthermore, that network has to constitute a closed system, at least approximately. The most basic description of economic structure divides the economy into households and firms, and immediately adds two sets of exchanges between them. On the one hand, households give firms labor and receive money in return; on the other, households give firms the money back and receive in return goods. Both exchange systems are thus cycles, in which money is rotated through the economy. Although circular, the economy is not a completely closed system in the ecological sense, because there are also inputs to firms from, for example, what we call primary commodities: minerals, agricultural products, and natural energy. The example of natural energy shows that not even the ecology of the planet is a perfectly closed system, because without the sun we could not keep it going. Systems, in fact, tend to be closed in some respects and not in others. A real economy is a *relatively* closed system; the grandmother-duck system, by contrast, is barely closed at all. It operates in a single direction, and that is why we do not want to call it an economy.

To summarize, therefore, one way of defining an economy is that it is a system of individuals interacting over scarce goods. That definition contains an implication that is very important from the point of view both of economic psychology in general, and for the specific question of the evolution of economic behavior.

Once you have such a system, you almost automatically get differentiation within it. Because discrete exchanges are involved, differentiation by time is logically inevitable, but so is differentiation by person. We say that, "Households sell labor to firms": that implies that households and firms are different kinds of economic agent. At some moments in your life you are part of a firm, and at other moments in your life you are part of a household. Within each individual's life, therefore, there is a division of labor: each of us contains a worker part of ourselves and a consumer part of ourselves, and we may also have an entrepreneur part, too. Of course these are not literally divisions of our selves; it would be better to think of them as roles we play. Division of labor within the person is thus inevitable, and in practice, of course, the most striking feature of a modern economy, as distinct from a primitive one, is the increasing extent to which there is also division of labor between persons. It is no accident that it appears at the very beginning of the founding classic of modern economics (Smith, 1776/1908). For example, we, the writers and readers of these essays, are most of us academics: we are specialized in academic work, either at the teaching level or the learning level, and none of us would be much good at many of the other jobs that have to be done in the world, like being secretaries or telephone operators or engine drivers or airline pilots. And there are other people who are good at those things who would not make such terribly good students or professors.

We are beginning to flesh out our definition of an economy. It emerges as a social system, involving many individuals reacting to scarcity by playing different roles. Now we can pose a little more clearly the question about why this complex of behaviors has evolved in humans.

Are there non-human economies?

First, though, we must make sure that the question is appropriate. Is it in fact the case that economic behavior, in the sense of behavior within a structured economy, *did* evolve in humans? Or is it a common trait of some larger group of animals, possibly of all animals? In that case we would not want to ask why it has evolved in humans; we want to ask why it evolved in animals at large.

It is certainly true that interacting systems involving division of labor are found in other animal species. To take again a common or garden example, most of the species of small birds that nest in your garden or in parks in towns and so on are what an ecologist would call pair-bonders. Male and female mate at the beginning of the season. They remain together and cooperate in rearing the young. But they do so normally in a way that involves some division of labor. Normally the female bird will specialize more in sitting on the eggs to incubate them. The male bird will specialize more in collecting food and bringing it back. Both contribute to the rearing of the young, but they contribute in specialized ways. The male

normally contributes less, by the way, but that is another story. An extreme example of such division of labor in pair-bonding birds, though not one many of us can observe in our gardens, arises in a tropical bird, the hornbill, whose behavior I have analyzed in economic terms elsewhere (Lea, 1980). Hornbills nest in holes in trees. In some species, at least, a mated pair build a nest together, the female lays her eggs and settles down to incubate them, whereupon the male goes away and gets a lot of mud and bricks her up in the nest so she cannot get out. We then have a total specialization, and obviously if the female cannot get out and if the male fails to feed her, the nesting is going to fail, which is a serious loss to both in terms of Darwinian fitness. Both are therefore heavily committed to the operation.

A second example of sexual division of labor fits less well with our gender stereotypes, at least in some respects. It concerns the behavior of lions (Bertram, 1978; Schaller, 1972; Caraco & Wolf, 1975). Lions are very unusual among the cat family in being social. They live in groups (prides) of perhaps a dozen. The hunting is done largely by the lionesses; male lions are relatively ineffective hunters, probably because they are too big, heavy and showy. Lionesses hunt in a cooperative way, but when they have cubs it is dangerous to leave them to go hunting. What happens is that one lioness will look after a large number of cubs, forming what is called a crèche, while the remainder go off and hunt food which all will share. They will not share equally; those present at the kill are likely to get more than the "baby-sitter", while the male lions, which may not have contributed at all to the hunt, will take a large share, but that is also another story.

Of course the most famous example of animal division of labor, and one which economists have often referred to, is the behavior of honey bees. Nowadays ecologists see bees as typical examples of what they call eusocial insects, which include many species of Hymenoptera (bees, wasps, and ants), and the unrelated group of termites. There is even one species of eusocial mammal, an African rodent called the naked mole rat, an animal whose appearance is as repellent as its behavior is fascinating (Jarvis, 1981). It is very well known that bees form large, organized societies in which different individuals have different roles. Some look after the larvae, some fetch food, some defend the hive from intruders and one, the queen, just lays eggs. Finally there are the males, the drones, who just inseminate females and shortly die. Though an essential role, this does not fit in very well with our concept of the beehive as place of busy, organized activity.

Trade, money and the definition of the economy

Clearly, social interaction over scarcity with division of labor is not a uniquely human trait. If we were to ask the question of why that evolved, we would have to look more widely than at our own species. That might well be instructive to do

(for example there is an extensive literature on the evolution of eusociality, e.g. Wilson, 1971). But perhaps there is also a narrower definition of what an economy is that would exclude beehives and lion crèches and immured hornbills. What seems to be lacking in these non-human cases is *trade*, or, in its extreme version, money.

A definition of an economy that depended too strongly on trade would be too restrictive, because there are many human societies which do not trade, or not to any measurable extent. In particular, the earliest human societies are thought to have resembled what are usually called hunter-gatherer societies. In a classic hunter-gatherer society the men hunt and the women and children gather nuts and roots and berries, and the majority of the society's food consists of vegetable matter, with only about a third being fresh meat (Lee, 1968). As they could do without the meat but not without the rest, we really ought to call them gatherer-hunters; it would not be unfair to say that the males are more decorative than useful. There is division of labor in such societies, but their ecological adjustment is such that they have virtually no property (Sahlins, 1974, chapter 1), and correspondingly no trade worth talking about.

Certainly the idea of trade is too restrictive if we want to make economic behavior coterminous with the human species. Furthermore money, the tool which makes trade really simple and easy, is a very late invention in human societies (Grierson, 1978). Many societies that are much more complicated than hunter-gatherer societies - both pastoral societies and settled agricultural societies - have no money, or had no money until Europeans came in touch with them. The possession of money would be much too restrictive as a definition of economic behavior.

Economics is a human concept. It would not be surprising if, like most concepts, it does not actually have a precise definition that covers the way that humans collectively use it; its usage is fuzzy, imprecise or polymorphous (Ryle, 1951; Wittgenstein, 1953). But we have now reached a good point to draw the fuzzy line that divides the economic from the non-economic. Somewhere between division of labor, which is perhaps too general a criterion, and the presence of trade, which is perhaps too restrictive, it becomes convincing to speak of behavior as being truly economic. Although this is a fuzzy line, it is not an unusable one; in particular, it is almost certain that economic behavior in this sense is unique to humans.

It is not so clear that all human societies would show economic behavior in this sense; hunter-gatherers are probably sitting somewhere on the fuzzy boundary that separates the economic from the non-economic. But why have humans got economic behavior, in this sense, when no other species has? There are plenty of other species of animals in the world. Nobody knows how many, but even if we restricted ourselves to living vertebrates, there are tens of thousands: around 5,000

species of mammals, 8,000 of birds, not to mention reptiles, amphibians and fish. Why have humans and no other species developed economic behavior?

The evolutionary requirements for economic behavior

Intelligence. There is one very obvious answer, which is almost certainly wrong. Surely human economic behavior results from human intelligence? But this will not do at all. It is not easy to find sensible ways of comparing intelligence between species. But, with one or two exceptions (e.g. Macphail, 1982, Chapter 11), everyone agrees which are the most intelligent non-human animals: chimpanzees certainly, probably dolphins, arguably dogs, maybe crows. None of these show any sign of economic behavior in the sense we have discussed, whereas the species that do come close to having real economic behavior, bees, ants, termites and mole rats are markedly, in fact deplorably, lacking in signs of great intelligence. There seems to be no correlation across species between intelligence and approximation to human economic behavior. Furthermore, it is quite easy to teach social exchange, the basic requirement for trade, to other intelligent animals. Chimpanzees and other species of primate readily learn to exchange arbitrary tokens, things that are physically very like money, for goods that they want (e.g. Wolfe, 1936; Cowles, 1937; Markowitz, 1975). They have the intellectual capacity to operate an exchange system, but it never happens in their natural social life. It is not for want of intelligence that animals like chimpanzees have failed to evolve economic behavior.

Language. A more plausible possibility is that special kind of intelligence which we have and that no other species has, at least not without human training. Humans have language; other species do not. We can certainly see that it would be difficult to strike a bargain without language. At first glance, it is hard to see how you could conduct trade if you did not have language to conduct it in. However, that is not quite as clear as it looks, because the history of exploration of previously unknown lands shows that people who had no common language were able to carry on trade to some extent. Perhaps the trading was not very efficient, and of course it could be argued that although the parties to it could not speak to each other, they could each use their own language to formulate to themselves what was going on. The modern history of tourism also suggests that lack of a common language is not a complete bar to trade. There is no doubt that the desire to trade is a powerful spur to foreign language learning, but that does not make language a necessary condition for the emergence of trade.

It is at least as plausible, in fact, that the opposite is true. Suppose that our ancestors were already for some other reason engaged in trying to trade with each other before there were real humans in the world: that might be one of the reasons why they evolved this unique and peculiar capacity for language. Precisely because

it would help a lot if you were trying to bargain, it would be adaptive for a human, or a prehominid, to have language, in a way in which it is not very important for a chimpanzee.

Division of labor. Surprisingly, therefore, intellectual factors are not very convincing as an explanation, or at least they do not seem to provide sufficient conditions for the emergence of an economy. They might, however, be a necessary condition. To see this, let us turn to a completely different possibility. Does the fact that a species has a society in which labor is extensively divided, predispose its members to start engaging in trade with one another? I would argue that it does. The most elementary economic argument about the benefits of trade is that, if different individuals are specialized in different skills, then the social products will be enhanced if what A produces and what B produces can be shared between them: if some of what A produces can be given to B and some of what B produces can be given to A. In some of the species we discussed above, bees for example, division of labor exists without trade, but there you do not have intelligence.

There is another difference between the division of labor that occurs in humans (and, indeed, our probable ancestors), and the other examples of division of labor which we can find in the animal kingdom. We are unusual in that we combine clear division of labor with a carnivorous, or partially carnivorous, way of life; lions are the only other example that is obvious. The peculiarity of carnivore is that a carnivore can get a tremendous amount of food very quickly. This is why domestic cats sleep for 13 hours a day: they can afford to, indeed it is the best thing they can do (Meddis, 1975). Even picking up their food in penny packets such as mice, they can get all they need to eat, and do all the other things they need to do in 11 hours. Humans are only partially carnivorous, so we can only afford to sleep for eight hours a day. Lions, like domestic cats, hunt for very little of their time, and spend a great deal of time sleeping; they still have time to be "among the world's sexiest creatures" (Barash, 1982, p. 243). Division of labor *plus* intelligence *plus* a carnivorous mode of life begins to look like a plausible set of preconditions for the evolution of a complex economy.

Tools. However, we can readily add other possibilities. One characteristic of humans is that we are toolmakers, and have been from before the time when we were recognizably human. The human evolutionary line started making tools long before the explosive expansion of the brain that distinguishes *Homo sapiens* from other hominids and prehomnids. And indeed our living relatives are perfectly capable of toolmaking, and do it spontaneously. Jane Goodall's description of toolmaking in chimpanzees (Van Lawick-Goodall, 1974) is well known; less well known, but perhaps even more instructive, is the experiment of an anthropologist (Wright, 1972) on an orang utan in the Bristol zoo. Wright spent a considerable time crouched in a cage at the zoo next to the orang utan, making chipped flint implements. The orang was provided with a pile of flint, and after a week or two it started making implements too. So it was perfectly capable of learning by imi-

tation, without any other encouragement, the simplest human technologies. And if an orang utan could do it, certainly an Australopithecine or other prehomnoid should be able to (and this, indeed, was the point Wright was trying to establish). Tool-making may well be linked both to hunting and to division of labor. Social hunters tend to hunt things that are bigger than themselves, and that creates a pressure towards tool using, especially in an animal like a human which is not naturally heavily armed. Tool-using tends to imply tool-making, and if the tools are of any complexity, some individuals are likely to be better at making them than others. The scene is set for a new kind of division of labor.

Parenting. Another characteristic human specialty is that we are parents. Humans, more than any other species, specialize in extended parental care. The period of immaturity in the human young is enormously long compared with that of comparable species. Even chimpanzees are effectively independent by the time they are about 8 years old, which is by no means true of human infants. We have already seen how parenting can drive division of labor in birds. But in the long period of dependence of the human child, you find another form of exchange driven, because it is the most probable explanation of the evolution of the totally bizarre and abnormal human interest in sex (Symons, 1979). Human sexuality is about the oddest thing about humans: alone among primates, our sexual interest in one another is not constrained by a fixed estrous period, by pregnancy, or by any other cause of infertility. In ethnological terms, human sexual behavior is largely emancipated from its original reproductive function. Our best guess as to the selective pressure that drove this is the need for women to trade: to obtain protection and provision from men in exchange for granting them sexual enjoyment.

Play. Yet another human characteristic, linked to our carnivore, our toolmaking and to our parenting, is that we are playful. Carnivores have a way of being playful, which is one reason why cats, dogs, and even ferrets are more satisfactory pets than rabbits or budgerigars. But the special thing about human playfulness is that we go in for a lot of object play; although not unknown in other species (Fagen, 1981), it is much less widely distributed than social and whole-body play. Object play probably results from the fact that, being primates, we have hands rather than paws. It is an arguable antecedent for money, as has been explained elsewhere (Lea & Midgley, 1989). But it is also relevant to trade and bargaining. Once you have things in your hands that you are not going to use or consume immediately, giving and taking them from one another becomes a more plausible kind of behavior.

Wisdom. Finally, we could perhaps remind ourselves of the exact meaning of the biological name for the human species: *Homo sapiens*. Literally, this means the wise man or woman (and more probably it will be a woman, because of greater female longevity). Wisdom is not the same as intelligence: though neither is easy to define, the distinction is fairly straightforward. Wisdom is about accumulated knowledge and experience, whereas intelligence refers more to the capacity to process it quickly. The point about knowledge and experience, about

wisdom therefore, is that the older you get, the more of them you are likely to have: the more you know of the history of your group, the more you know of what has happened in the past, the more you can use it as a guide for what will happen in the future. At least, this was the situation until the emergence of such very modern technologies as writing. Traditional human societies, quite rightly, tend to value the old. This is not quite true of hunter-gatherers, who tend to leave their old behind and let them die when they can no longer move easily with the band; but that is because they cannot do anything else given their highly mobile way of life (Sahlins 1974, chapter 1). But once a society gets beyond the hunter-gatherer stage, it becomes very important to preserve the old: it is to the pastoralist Hebrews that we owe the commandment, "Honour your father and your mother, that you may live long in the land the Lord your God is giving you" (Exodus 20 12). Respect for the old enforces a new kind of division of labor. The old are the repository of the culture, and if the rest of society want access to that knowledge, that wisdom, then those who are still young enough to hunt and gather or to till the field or look after the flocks, must provide food and other resources to the old in exchange for their wisdom. A new kind of trading is required, involving a new resource class (Foa & Foa, 1980): goods are traded not for other goods or for services but for information. Here lies a less obvious reason why language may be important for the evolution of the economy, because it is language that gives us a culture worth preserving and paying for.

Synthesis. What I am suggesting here is not very earth-shattering. Its merit, if it has one, is that it picks up some things that are important and easily neglected, while suggesting that some other things that seem important may not be. The view I am putting forward is that the origins of economic behavior in humans lie in the fact that we are hunters, because that encourages division of labor; that we are primates, because for this reasons we have hands, which in turn encourages play and tools; and that we are highly social, which on the one hand is partly a consequence of our long parenting period, and on the other makes it possible for us to use the wisdom of the old. It is not unusual for primates to be social (in fact it is the norm for Old World monkeys), but it is unusual for hunters to be social, and it is very unusual for primates to be hunters. In the human species an unusual constellation of properties come together. My view therefore is that true *Homo economicus* is not rational man, but he is (or she is) a tool-using, social, hunting primate.

Implications

Everyone can enjoy evolutionary arguments, and the only equipment you need to engage in them is an armchair. Does the argument put forward in this essay have any implications? Oddly enough, it does.

The first implication is a negative one. We are not so clever to have an economy. Having an economy is not anything to be proud of, at the biological level: the economy is just the human version of the ecology, and every animal has an ecology. Humans have a peculiar way of adjusting to scarcity, but it is not massively peculiar. I had to work quite hard to identify something about economic behavior that really was uniquely human.

Secondly, and also negatively: when we do identify something that is distinctively human about the human economy, it does not seem to require tremendous intelligence. Intelligence may well have had a part to play, but of all the things that could have contributed to the development of an economy in humans, it seems to me to be the least important. It is more likely that having an economy made us more intelligent, in the sense that having an economy may have given a selective advantage to more intelligent individuals, and particularly to those possessing the rudiments of language.

What about positive implications? The key one is that sociality is crucial. It is the social aspects of our behavior, the fact that we lived in a particular sort of society whose members reacted on each other in particular sorts of ways long before we were fully human, long before we had anything that we would recognise as an economy - it is the sociality of our species which was crucial in driving the development of an economy. But social animals are only going to develop an economy if they can cooperate and share. They need not cooperate fairly or share fairly, and everything we know about co-operation and sharing shows that both are usually somewhat unfair. We have examined several kinds of behavior that look somewhat like economic behavior, and are plausible evolutionary precursors of it. All require some degree of cooperation and sharing. So I argue that it is only because these things are in human nature that we were able to evolve an economy. If they ever ceased to be in human nature, we would not be able to sustain an economy.

That leads to a surprising general implication. What I am arguing is that the evolutionary roots of economic behavior are in cooperation, not in competition. That is not to say that there is no such thing as economic competition, nor that such competition is unnatural: humans are self-centered and self-interested as well as social and co-operative. Nor should we lose sight of the fact that individual altruism may result from genetic selfishness; that is the core insight of sociobiology. My conclusion is that selfishness and self-interest alone would not have led to the evolution of economic behavior; it is only because we are able to co-operate that we are able to compete in the distinctive way that we call economic. The evolutionary argument shows that a science of economics, or an economic psychology, which tried to build everything on self-centered, self-interested individual behavior would be a dismal science indeed. In fact, it would be a biological nonsense: perverse and unable to make progress.

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Chapter 4

Rationality: The Formalist View

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Summary

This chapter puts forward the formalist view of rationality, in contrast to the more usual substantivist view. The idea of rationality has meant different things to psychologists and economists, and this has led to confusion. The formalist view argues that the root assumption of neoclassical economics, that people are rational utility maximizers, has no empirical content. This means that it is not possible to show that it is true or false. Any systematic behavior can be represented as the outcome of a maximization process. Economic psychology, therefore, should not consist of a conflict between economic (rational) and psychological (arational) accounts of behavior. Rather, the rationality assumption is a language or framework within which particular theories about economic behavior can be expressed. Psychological theories and data, like economic data, can contribute to the construction of such lower level theories, and to empirical tests of them. Unfortunately, although this position is more sympathetic to economic thought than psychologists often are, it has so far not been widely accepted by economists. The chapter concludes by discussing some advantages and disadvantages of both substantivist and formalist positions.

Key words

Rationality, utility maximization, paradigm, theory construction

Introduction

The story is told of a young curate who, having recently been ordained, went and sought his bishop's permission to ask him a very serious question. How long, he

wanted to know, should he wait before repeating the same sermon before the same congregation? The bishop looked at him thoughtfully, and replied, "In your case, young man, I think that two weeks should be sufficient".

Everyone who is interested in economic psychology has read numerous articles about the question of rationality, and this essay will inevitably involve an element of repetition. But equally, no book about economic psychology can ignore the question. Unless you clear your mind about the rationality question at the beginning of this book, you run the risk of spending some very unproductive time as a reader. This chapter is not a guide to the voluminous literature on rationality, for which MacFadyen (1986) or Hargreaves Meap, Hollis, Lyons, Sugden and Weale (1992) will be a much better introduction. Rather it is an attempt to state a personal position. It is not quite the conventional one, so if you are suffering from a sense of *déjà lu*, be warned that there may be some surprises in store.

We must all decide where we stand on this question if we are to be effective as economic psychologists. Rationality is either the most important question in economic psychology or the least important question, and it does make a difference which of these two positions we take. In this essay, I shall be arguing that it is the least important question, and I would like to persuade readers to spend less time, as you read this book, asking whether this or that kind of economic behavior is rational. But if, at the end of the chapter, you do not agree with me, I hope you will at least know why not.

Rationality in economics and in psychology

What, then, is the rationality question, why do I think that it is not important, and why might you disagree? In the first place, the rationality question is, as Wärneryd argues in Chapter 1, the key to the structural difference between psychology and economics as academic disciplines. Wärneryd reminds us that economists have been investigating rationality ever since John Stuart Mill, and some historians might date its influence from even earlier. Of course there are and have always been economists who do not want to focus on the rationality issue, or who find that their fields of study are not very much illuminated by the assumption of utility maximization; but they are a minority and sometimes, when the sense of economic orthodoxy has been strong, almost a persecuted minority. I will return to such non-rationality driven economics later in this chapter.

For the majority of economists, the assumption of rationality sets the programme for their thinking and research. It is not so much that they investigate rationality, as that they investigate the consequences of assuming that people make rational choices. "Rational choice" can mean something very simple, and very reasonable; for example, an essential axiom of rational choice (e.g. Simmons, 1974, p.6) is, in everyday terms, that people will reliably prefer more of a good thing to

less of a good thing. Some of the other axioms are not so obvious, and the out-workings of the axiom set are of course very far from simple; they constitute the whole of economics, or at least the whole of economics as it is conceived by members of the neo-classical school.

The essential difference between psychology and economics is that psychology lacks any such dominant programme or guiding assumption. It is very common for economists who have become interested by economic psychology to turn to a friendly psychologist and say, "Well, I can tell you what the economic theory of such-and-such a phenomenon is; now, could you tell me what the psychological theory of it is, and then perhaps we could compare them?" It is excruciatingly embarrassing for a psychologist to be faced with such a question. The only possible reply is, "Er... well, which psychological theory would you like? I have roughly 150 in stock and they all have something to be said for them". At this point our friendly economist is apt to retire, baffled, and lose interest in interdisciplinary co-operation.

But if the structural difference between economics and psychology is baffling for an economist, to a psychologist it poses a standing temptation. The axioms of rational choice look like assertions about human behavior and human cognition, and if they are interpreted in this way, they are plainly untrue at the psychological level. The temptation, therefore, is to turn to economists and point out that they have founded their science on false assumptions. "Economic man", the fictional being who obeys, precisely, the axioms of rational choice, is not just a fiction but a completely implausible fiction, and the only thing you could generate from this fiction is a sick and distorted economics which fails to describe human economic behavior. What economics needs, according to this tempting line of argument, is a new, empirically established model of human choice; on this foundation, a proper science of economics can be securely established.

I describe this as a temptation because it leads economic psychology astray. There are at least three reasons why this is so. The first two can be argued fairly briefly; the main business of this chapter is to argue the third.

The first may sound trivial, but it is not. To assert that the rationality assumption is false is simply tactless, and therefore no way to approach a fruitful interaction with someone who has gone through many years of being trained how to wield it. Science is a human activity and scientists have human feelings; the dynamics of social groups work just as effectively between academic disciplines as between any other human groups. Of course, diplomacy is not everything and sometimes it is unnecessary to be undiplomatic; sometimes, indeed, it gets you listened to. But if psychologists are interested in working with economists on any problem *other* than the appropriateness of the rationality assumption, attacking rationality is not the way to go about it.

In any case, there is a much more serious objection. Psychology does not in fact have any alternative model of human choice to offer the economist. We do have a

number of possible alternatives, but none of them has the simplicity (and therefore the power and generality) of the rationality assumption, while being remotely as plausible as rationality. There are ideas in psychology that are very simple and powerful. Skinner's reinforcement principle is a good example, and it has been applied to economic behavior (e.g. Alhadeff, 1982). But it is not nearly as plausible as an underlying description for all human behavior as economic rationality; most economists would say that if that is the alternative they would rather stick with the principle they already have. Conversely, there are ideas about choice that are highly plausible, for example Kahneman and Tversky's (1979) prospect theory, of which we shall hear much more elsewhere in this book; but they lack the generality and power of rational choice.

Finally, as Wärneryd suggests in his chapter, the programme of replacing economic rationality with something else misunderstands what economists are doing with the rationality assumption; it is a misunderstanding of the function of the rationality assumption within economics. Most economists would agree with Friedman (1953) that it simply does not matter whether rationality is an accurate description of the behavior of individuals, so long as with its aid they can make predictions about the behavior of the whole economy, and a reasonable proportion of those predictions turn out to be right. A psychologist faced with the all-pervading rationality assumption is tempted to argue that economists are unlike true scientists because when the data disagree with their theories, they modify the data instead of the theory. But this is too sarcastic about economists (and perhaps too idealistic about natural science: cf. Kuhn, 1970). Economics has always been constrained by data. In its earliest days, perhaps down to Marshall (1890/1949), most of the data used were facts of common experience; to an increasing extent, they are the results of statistical surveys and even of experiments. Empirical data do modify economic theory. But what they do not modify is the use of the rationality assumption. I shall argue at length below that this is because rationality is not a theory at all, but rather serves as a framework within which detailed theories are constructed.

Two confusions about rationality

Economic psychology, therefore, must not become a process of "testing the rationality assumption". That is the trap; what can we do to avoid it? The first step is to clear up two confusions.

Mechanistic rationality vs. descriptive rationality. The first is the confusion between rationality as mechanism and rationality as description. It has been discussed at length by Simon (1978). The word rationality is a singularly unfortunate one, because it has developed two perfectly legitimate meanings. Of the two, the one that lies at the root, and is etymologically most correct meaning, no longer

corresponds to what we most often mean when we use the term to describe economic behavior.

In everyday speech, when we describe behavior as rational we tend to mean that somebody has thought out what he or she is doing. When we describe people as rational individuals, we mean that they are the sort of people who think out the consequences of their actions before they perform them. To anticipate the theme of Chapter 6 a little, it is this capacity for rational foresight that is probably the biggest effective difference between human cognition and animal cognition. As human animals, animals but human nonetheless, our possession of language enables us to model the consequences of our actions before we perform them, and to choose between actions according to their consequences. Economics acquired the concept of rationality through philosophers of the 17th to 19th centuries who were using it to characterize human thought in this way. The philosophical notion of man as rational led to the economic idea that people use rational foresight to make economic choices.

However, it is no longer this sense of rationality that we are using when we say that in economics we assume that behavior is rational. Modern economists make no assumption whatsoever about the mechanism by which behaviors are chosen, and therefore when Wärneryd correctly says that the great majority of economic choices are made as a matter of habit or routine, that has no implications, one way or the other, for whether those choices are rational in the modern sense. In modern economics, "rationality" is a descriptive, not a mechanistic term; or, in other words, economics is interested in the *effects* of behavior, not the *processes* by which it is brought about. We describe economic behavior as rational if it is the behavior that will maximize utility in the given situation. It is linked to the idea of rational foresight, since (descriptively) rational behavior is what would be produced by a perfectly (mechanistically) rational decision maker. But real decision makers may employ no end of rational foresight and end up making choices which are very bad for them; and on the other hand, they may respond completely by habit (or they may even be rats or pigeons, and therefore presumably have no capacity for rational foresight at all: cf. Lea, 1978) and still make choices which successfully maximize their utility.

It is crucially important to recognize what kind of rationality we are talking about if we are going to criticize rationality. Many psychological critiques of the assumption of rationality are actually talking about the processes by which choices are made rather than the effects those choices produce, and process is quite irrelevant to the question of whether or not those choices maximize utility.

Rationality, maximizing and optimizing. The second confusion we have to clear up is merely a verbal one. In different decision-making contexts, we sometimes talk about "maximizing", sometimes about "optimizing", and sometimes about "rationality". Different words are popular in different sciences that are linked with decision-making, and they do have different connotations. In ecology, for example,

sometimes a distinction is drawn between maximizing and optimizing: some ecologists talk about maximizing where the objective function (the quantity that is to be maximized) is defined on a single variable, and optimizing where there must be a compromise between many desirable outcomes. This distinction is hardly worth making in economics, because we are always faced with a multitude of desirable objects; utility is always a function of $x_1, x_2, x_3, \dots, x_n$. So for our purposes, as economic psychologists, these different words are just different ways of talking about the same basic proposition: that we can predict what behavior will be by finding what behavior maximizes some utility function.

Substantivist and formalist approaches to rationality

We turn now to the way psychologists often misunderstand the role of the rationality assumption in economics. To illustrate this, let me refer to a small piece of research that some colleagues and I carried out at Exeter some years ago, and contrast different things that one might say about it. This needs an aside to explain the origins and purposes of the research. We were interested at the time in the psychology of money, and the ways in which the standard economics textbook's description of the functions of money might be an inadequate description of how people respond to money at the individual level. In particular, we had come across some of the literature in economic anthropology about forms of money that occurred in cultures before they came into contact with what we may now call the common world culture--the culture that originated in Western Europe. One of the characteristics of these forms of money is that they often have special purposes. For example, on Rossel island in the Pacific, there was one form of money which must be used by women and another form of money that must be used by men (Einzig, 1966, p.62). This arrangement clearly inhibits economic transactions between men and women, so it is regarded as a "primitive" and inefficient form of money compared with our "modern" cash system.

To read the literature on "primitive money" is rather to get the impression that economists, and to a lesser extent economic anthropologists, take a Whig view of history: they see the history of money as a steady progress towards the advanced and perfect state of the modern monetary system. We were interested in ways in which modern money might reflect some of the so-called limitations of primitive money. Are there kinds of economic transaction which you cannot carry out with modern money?

One plausible candidate is the use of money as a gift. Things may not be the same in all countries, in fact, we know they are not (Hussein, 1985); but in England, it would be considered socially highly unacceptable for a young adult (a student let us say) to give his or her mother money for a birthday present. And I do mean highly unacceptable. This piece of research led to the only time when I have

been seriously threatened with being beaten up while conducting a psychological experiment. We needed to know the answer to the following question: if you found yourself in the situation where the only thing to do was to give your mother money for her birthday, how much money would you give? We were contrasting this with how much money people would spend if they went into a shop and bought a gift, the idea being that if you were making a present of money you would have to give more money, to compensate for its social unacceptability. People are generally very tolerant of the apparently idiotic questions psychologists ask them, but for a subset of respondents, this question was altogether too much; they refused to name a sum, and became angry when we tried to insist.

There are clearly limitations on what you can use English money for in English society. These limitations have been explored further in later research (e.g. Pieters & Robben, 1990; Burgoyne & Routh, 1991). How do we analyze them? From one point of view this social rule is highly irrational. If you want to give your mother the greatest utility then clearly you should give her money for her present, because then she can go out and buy whatever present will most please her. We pointed this out in our report of this piece of research (Webley, Lea & Portalska, 1983), although we carefully did not say we thought it was the most important implication of our findings. Nonetheless we got, if not storms, at least heavy showers of annoyed economists pointing out to us that they could draw up a utility maximizing explanation of this behavior (for a published example, see Cameron, 1989, though this was not the main thrust of Cameron's article). Such an account simply has to start from the premise that the purpose of the gift was not to give your mother maximum utility in terms of maximum goods, but rather to maximize her utility by showing how much you care about her; and that to do this you needed to give a gift which showed you had spent your time walking round the shops looking for the present she would like most, and furthermore that you care about her sufficiently to know her tastes and choose an appropriate gift. Such a carefully chosen present would give her more utility than a large sum of money with which she could go out and buy any commodity she wanted.

Our answer to our economist friends was that we did not disagree: we too thought (in fact we knew from other parts of the investigation) that this was a very relevant factor, though not the only one--for example, the giving of money conveys signals about status that it is inappropriate to send to your mother--but that is beside the point.

I want to contrast two ways of responding to our little discovery (if it can be called a discovery, since we were sure what we would find before we carried out our research). You could say that we need to reject the utility maximizing principle, because in this case it has failed; this is what economists like Cameron thought we were saying (though in fact we were not). Alternatively, you can say that utility is still maximized, but the arguments of the utility function are not what we might originally have guessed: the value of a gift to your mother is not a

function only of the value of the commodities that are involved in the gift, but also of the time spent by the donor and the knowledge of the recipient's tastes the gift reveals. It is easy to see why time enters into the cost to the giver (cf. Becker, 1965). It is less obvious, without additional, social psychological assumptions, why it should enter into the benefit to the receiver; but once you make those assumptions, you can proceed with a perfectly orthodox economic analysis of the situation. Of course, doing so involves changing the economic theory of giving (in so far as there was one beforehand), so as to take account of the data we had obtained. But that, surely, is what scientists should do.

What attitude to the assumption of rationality does this example reveal? It implies what I am going to call a *formalist* approach as distinct from a *substantivist* approach, and in the rest of this chapter I am going to argue for the merits of the formalist approach. The substantivist approach says that the assumption of rationality genuinely has empirical content; it is a substantive proposition about human behavior; it asserts something which may or may not be true, and whose truth or falsity we can investigate by empirical means, whether they take the form of psychological experiment, econometric survey, or reflection on the facts of everyday experience. According to substantivism, it is meaningful to inquire whether rationality is true, and we can hope to get an answer, yes or no. The formalist approach on the other hand says that we are not interested in whether rationality is true or not, because this is a question we could never settle. Rather, rationality is a language we adopt in order to be able to formulate a theory. It is simply the framework within which economists construct substantive theories about particular things; my little theory about how you choose gifts for your mother is an example. The change from one set of arguments for the utility function to another represents a substantive shift of economic theory within the formal framework of the utility maximizing approach.

The economists' response to the formalist position

This formalist approach was presented, though not by that name, in the textbook of economic psychology which two colleagues and I wrote (Lea, Tarpy & Webley, 1987, Chapter 5). It was, indeed the key argument that lay behind much of the development of that inordinately long book, which took us much of the last decade to write. Having rejected the substantivist approach to rationality, we inevitably rejected with it the idea that the programme for economic psychology is to investigate by psychological means the truth or falsity of the rationality assumption. Instead, we proposed a different paradigm for carrying out research in economic psychology. We felt quite pleased with ourselves when we had done that, because we felt we had beaten out an approach which offered much more prospect for fruitful collaboration with economists. As we were all psychologists, we very much

wanted to collaborate with colleagues from economics on problems where we felt both economic theory and psychological theory had interesting things to say. The formalist approach to rationality implies a neat division of labor between people trained in economics and people trained in psychology in generating theories in economic psychology. Psychologists, approaching a new economic situation with their characteristic empirical cast of mind, can use psychological data or theory to suggest what variables are likely to influence the behavior concerned. Economists, formulating their ideas in terms of utility maximization, can take those variables as arguments that will enter into a utility function, and find out how people would maximize utility in these circumstances. We can then all go away and collect the evidence and see whether we are all correct. There is no question of the psychologists' variables being incompatible with a utility maximization approach, because any function of any variables can be represented as the outcome of maximization. Equally, however, the results of the empirical test are not a foregone conclusion; if behavior turns out to be incompatible with the utility maximization theory, we must reconsider how utility was specified. The way to productive interdisciplinary research is thus open.

Much to our disappointment this proffered olive branch was promptly trampled in the dust by economists, including some whom we greatly respected. Economists in general are not prepared to accept the notion that utility maximization is purely a formal proposition. We had not, of course, invented the formalist position (it is eloquently presented, for example, by Boland, 1979, 1981). There are some mathematicians or mathematical social scientists who have argued that rational utility maximization is so much a formal proposition that any consistent pattern of behavior whatsoever can be represented as the maximand of some utility function (e.g. Rachlin, 1980) and it is merely a question of convenience whether this is the best way to write the descriptive equation for a particular set of behavioral circumstances. But perhaps coming in a book that explicitly addressed the relation of economics to psychology gave the formalist idea prominence.

And most economists fiercely rejected it. Here are two examples from reviews of our book:

Doubtless, economic psychology will develop along the lines proposed in the book, but it will be clear that I am not optimistic that much progress will result... I do not believe that they are right to argue that the focus of experimentation should move away from being rationality questions (which it is naive to interpret as largely tautologous)...

--Binmore (1988).

...there is one major point in the economist's view of the world that should be mentioned. Some of us have a general theory of behavior which is applicable to businessmen and investors, but also to criminals, procreators, participants in riots, politicians and bureaucrats. They all optimize their own behavior and, in doing so, optimize the outside Walrasian world... To many neoclassical observers this is the very core of economics. I am not saying that psychologists

should accept the notion that the world we happen to live in is a neoclassical one; I hope they shall criticize that notion. But therefore it should be recognized first.
--Pen (1988).

As an aside, I have to say that Pen's implicit hope for a critical dialogue between economists and psychologists over the rationality question is, in my view, too optimistic: my experience has always been that it is easier to persuade psychologists than economists that such a dialogue between the two disciplines would be interesting.

Economists like Pen, therefore, take a firm and positive substantivist position: they see rationality as an assumption that has content and is, as a matter of fact, true. Other economists, even more interestingly, took a negative substantivist position: they said that they did not want us to sidestep the rationality issue, because what is wrong with modern economics is that it is transfixed with the issue of rationality; they wanted to recruit psychologists to help them in overthrowing it. This seems to be the objection to our approach that comes from Earl (e.g. 1990), who is one of the economists who has taken most interest, and the most intelligent interest, in psychological theory. For such economists, rationality is an assumption that has content and is, as a matter of fact, false. And finally there are economists who see rationality as neither right nor wrong but a dangerous irrelevance as the starting point for economic theory, which ought instead to be founded in empirical sociology. It is certainly true that, over the past two or three decades, the so-called neoclassical approach (which makes the strongest use of rationality of any economic theory) has advanced enormously in popularity and prestige. Up like a rocket can mean down like a stick, and it may well be that in another decade or so we could be looking at an economics that was much less dominated by rationality assumptions. There are already signs of this happening, whether we look at the political arena or within the academic world; the popularity of Etzioni's (1988) book *The moral dimension* is a sign of the growth of a very different economics, which is beginning to be widely known as socio-economics (although most mainstream economists are at least as skeptical about this as about economic psychology).

Thus, if you adopt a formalist position about rationality you are criticized by some economists like Pen who believe that rationality is right but want to test that belief, and by other economists such as Earl who think that the rationality assumption is all wrong and that they need psychological theory to find an alternative. As for the socio-economists, a rough summary of their position would be that the neoclassical approach is the very devil, and you are not on their team unless you are joining in trying to cast it out with all its works--which undoubtedly include the assumption of rationality. So for different reasons, very different economists would take a broadly substantivist position as against the formalist position I am advocating here. We therefore need to look at what is on their side.

Why might it be true that people behave rationally, and therefore, if those things were wrong, why might it be false?

Arguments in favor of substantivism

I am going to propose some reasons and then knock them down. Of course this is a slightly artificial exercise, and to see justice done you must really go to a substantivist theorist, but I shall do what I can to strike a balance between partiality and impartiality.

Rational foresight. We can take it for granted, I hope, that people do indeed have rational foresight. Could it be that the existence of this mechanism for decision making really does ensure that most, even all, decisions are more or less utility maximizing? This would be a very odd argument indeed, for two reasons. First, we have had now over thirty years of serious, indeed Nobel prize winning economists like Herbert Simon pointing out that from a mechanistic point of view rationality is bounded, and furthermore it is bounded for reasons that a utility maximization theorist has to recognize, such as the cost and time of mental computation. There is an economics of information (e.g. Stigler, 1961; Heiner, 1983) and an economics of time (e.g. Simon, 1978) involved which imply that the decisions taken in many circumstances cannot be utility maximizing ones. One can get round that to some extent, but at a cost: the analysis gets more and more contorted. Remember that one of the advantages of rational choice as a founding assumption for economic is its simplicity, and if it loses too much simplicity it loses its attraction.

An even more killing objection to rational foresight as a mechanism has been put by Thaler (e.g. 1992). Why is it that a theoretical economist, who has just spent a year and a half working out what is the normative behavior in a complex situation, then assumes that all the people who are in that complex situation have been behaving like that for the last three thousand years? If the analysis is so difficult, if it requires great efforts of economic theory to discover the utility maximizing behavior, it is unreasonable to suppose that real people in the real world already knew it by process of rational foresight.

Evolution. Why else might choices be rational? A popular idea is to appeal to the Darwinian theory of evolution by natural selection; natural selection, it is argued, guarantees that animals will behave in the most advantageous possible way--and people are animals. As an evolutionary theorist, in a modest way, I have to tell you that, while the second half of this proposition is undeniable, the first half is untrue. Evolutionary theory guarantees only that animals will behave in the worst viable way; while it will certainly get rid of unviable forms of behavior it cannot be relied on to produce the best behavior (Lea, 1984, p. 7). Evolution is a

historical process, and you cannot begin to construct a proper evolutionary theory of any animal's behavior without knowing its history, that is, without knowing where it started from. Its starting position will constrain the end states it can reach, so that an ahistorical maximization assumption is almost bound to make wrong predictions.

There is an even more basic objection to biological evolution as a reason for human behavior being optimal: so far as our economic behavior goes, none of us are making our decisions in an environment to which we could conceivably have evolved. Perhaps at a social level, it makes sense to analyze human behavior in terms of biological evolution (e.g. Wilson, 1978); like all other mammals, we still have to interact with parents and sexual partners and offspring. Our economic institutions, however, are nothing like those into which *Homo sapiens* evolved. Biological evolution is simply too slow to ensure maximizing economic behavior.

Cultural evolution. This same objection applies also to some descendants of evolutionary theory that could reasonably be said to be more relevant to economics than the original Darwinian, biological theory. It has been strongly argued that there are analogue processes involved in cultural evolution, and in particular that selective processes are important in cultural as in genetic evolution (Dawkins, 1976, chapter 11; Lumsden & Wilson, 1983). Might not selection at this level produce a reliable tendency to rational choice? But cultural selection like genetic selection is a history-dependent process; it cannot be used to generate predictions abstracted from the individual's cultural history.

Economic competition. Cultural evolution is not the only social-science analogue of genetic evolution. One analogue stands a much better chance of inducing optimal behavior, at least in a limited number of economic agents, and that is economic competition. Many authors, notably Hirshleifer (1977), have sought to make a comparison between the process of competition between firms and the process of biological evolution. The analogy is a plausible one, and it is particularly interesting because in certain competitive environments the worst viable behavior (which is what evolutionary processes will normally guarantee) is also the best possible behavior, because nothing except the best possible behavior will guarantee your survival. There are cases like that in biological evolution too, but in a truly competitive market, it is the norm: the most efficient firm will drive all others to the wall unless their behavior conforms to the optimum it has found. Thus there is a plausible possibility that competition will ensure utility maximizing behavior by the selective mechanism of eliminating firms that do not maximize utility. The problem with this argument is that it tends to imply there can be no further improvement, yet we see industrial and organizational innovation going on all around us. It is also limited. It is all very well to say that firms are utility maximizers, or that large firms will be utility maximizers while small

firms and individual firms will not; indeed, one of the founders of modern economic psychology, Reynaud (e.g. 1981) did argue very much along those lines. The trouble with the argument is that it gives us no help in explaining the behavior of consumers; and most economic psychologists are more interested in consumers than firms, because consumers, being individuals, relate more easily to psychological theory--though it would be a very unfortunate limitation if economic psychology did not also look at behavior in and between firms.

Educated decision making. If you go and talk to an audience of statisticians about real human decision making, and how it deviates markedly from the normative principles they have established, you will sometimes get the following response: we accept that is how people tend to behave, but our task is not to describe the fact or even to bemoan the fact; our task is to educate people to behave optimally. Thus if our mythical theoretical economist has, after his or her year and a half of effort, managed to find a new normative solution, we need to go out and tell people about it, so they can start to behave more rationally. On this idea, therefore, even if human choice is not at present rational, it will become increasingly rational in the future. In particular, if we discover irrationalities, we can tell people about them, so that they can avoid them.

This is a very important point of view, because logically it is a consequence of a general principle that distinguishes social science from the natural sciences, including biological science. More strictly, we should say that it distinguishes the human sciences (which include human biology) from physical science and from other biological sciences. The general principle is that people are capable of reacting to our theories about them. Thus, if we point out that people typically do not behave optimally, that may modify their behavior in a direction of optimality--or of course it may modify the behavior of firms, who may try to exploit this non optimality. In either case, it has the potential to change the situation that we thought we had described. This general principle sets a limit on prediction in human science that some researchers have seen as so fundamental that it should be compared with the Heisenberg uncertainty principle in physics. Tietz (1992) has recently spelled out its importance in the specific context of apparent irrationalities in economic choice.

Logically, therefore, there certainly is a possibility that the process of education will bring about utility maximizing behavior in fields where we do not have it yet, at least in the future. While the logic of the uncertainty principle is unassailable, and it can be very important in some branches of economic psychology, I doubt its practical importance in this case. Perhaps I am unduly pessimistic, but having been engaged in the trade and craft of education for a quarter of a century, I see no sign that it can have that powerful an influence on peoples' behavior--though this does not mean there will be no change in behavior. Some irrationalities may disappear if they become widely known.

Arguments for formalism

The above have all been arguments for positive substantivism--the view that rationality is (or could become) a true proposition about behavior. Some of the arguments I have advanced against them veer towards negative substantivism; if rationality is demonstrably false, it is demonstrated to be a substantive position. In my view, however, all the above arguments leave us in a state of uncertainty, which gives some support to my formalist position, but of a rather roundabout kind. What then of the positive arguments for formalism?

Mathematical formalism. The most fundamental is the simple mathematical argument that any consistent behavior can, if necessary, be described by means of a function; and that function can in turn be described as the maximand of some other function. This is, in slightly more mathematical language, the argument of Rachlin (1980). The question of rationality then becomes one of convenience rather than substance: is it mathematically convenient or heuristically useful to move via the maximand stage?

The historical argument for formalism. However, the argument I find most persuasive is much less sophisticated. It is historical rather than mathematical. The first book I know of with the title of Economic Psychology was written by Gabriel de Tarde, a French sociologist and social psychologist, and published ninety years ago (Tarde, 1902-3). We have had therefore nearly a hundred years of economic psychology. For most of this time, the dominant approach has been to test, or at least reflect on, the rationality of economic choices using psychological theory and data. My personal interpretation of our collective history is that this concentration on the rationality paradigm has not yet led to a rapidly advancing interdisciplinary development of economic psychology. Rather, those areas where we have seen rapid development have been those where economists and psychologists, for reasons exterior to both disciplines, have had to pool their resources to study a common problem--a problem given to them by government or by public opinion. Katona's surveys of consumer confidence fit this pattern; so do, for example, studies of taxation, which have already filled the first and the most recent special issues of the *Journal of Economic Psychology* (see Lewis, 1982; Webley & Hensing, 1992); and so does what I hope may be the next great problem for economic psychologists, the study of the psychological consequences and contributions to the enormous economic and political changes now going on in Eastern Europe. Logically, too, we could consider such applied disciplines as marketing and business administration as branches of economic psychology, even if few of their practitioners would recognize the description. Their progress and success in the past century needs no demonstration.

The gains from formalism

The historical argument is a pragmatic one, and it needs to be complemented with pragmatic arguments that there are positive advantages in taking the formalist view.

Interdisciplinary co-operation. First, in spite of the immediate reaction from economists described above, I still think that the formalist view ought to facilitate collaboration between psychologists and economists. Clearly there are substantivists, among both psychologists and economists, who actually want to spend all their time talking about the truth of the utility maximization position; but I do not believe they form a majority in either discipline.

Asking substantive questions. This leads on to my second point. Rationality is not the only question in economic psychology; there are many others, and they are more interesting. Taking the formalist position should direct economic psychologists' attention to questions which we can answer, as distinct from the rationality question, which in nearly a century we have failed to answer, and which if the arguments of this chapter are accepted we logically can never answer, because behavior will always be consistent with the maximization of *some* function. Furthermore, many of the interesting questions in economic psychology are easily put in the form of asking what arguments should go into a utility function. A psychologist might more naturally put them in the form of asking what are the factors that affect the economic behavior of individuals, and what are the facts about the economy that affect the non-economic behavior of individuals, but nothing is lost by asking them in utility terms.

Tastes. A particular class of questions which economic psychologists should be addressing are questions about what economists call "tastes". Nearly all economists would say that whereas they can tell you how to maximize your utility given your tastes, they can say very little about what your tastes are and where they come from (cf. Stigler & Becker, 1977). But if we want a substantive economics, an economics which deals with facts and not only with theory, we must sooner or later find out what peoples' preferences actually are, and that means asking what their tastes actually are. Psychology can tackle this question in two ways, in terms of general theory and in terms of specific empirical tests.

The general theory of tastes is what a psychologist would call the theory of motivation, and it is one of the best developed areas of psychological research, reaching into every branch of psychology. Not surprisingly, therefore, psychology is rich in techniques for investigating specific "tastes", whether we call them preferences, values, motives, or goals. Integration of this material with economic theory and data greatly strengthens our ability to predict human behavior, as marketers and other consumer scientists have found. Nor is this a one-way traffic. In

investigating tastes by economic means is by no means an impossibility, indeed very often what you are really doing in practical econometrics is finding out about peoples' tastes, because tastes determine the demand functions that you compute. However, it is also valuable to have an independent measure of tastes against which you can test your econometric or other practical economic conclusions. Similarly psychologists need to learn how to use the data that comes from practical applied economics as evidence in discussions of human motivations. The econometrics of the alcohol and tobacco markets, for example, have quite a lot to say to the psychologist interested in addiction. This is just one example of the way the formalist position leads to a fruitful division of labor between psychologists and economists.

The diversity of psychology. The final gain from the formalist position is that it allows us to make positive use of the fragmented and incidental nature of psychology, which contrasts so obviously with the relatively monolithic nature of economic theory, instead of deploring it. Under formalism it is no longer a problem that psychology has many small-scale theories for different situations--that we have one psychology for family size, another for the difference between luxuries and necessities in the market, and yet another for giving and receiving gifts. If what psychologists are doing is specifying the arguments of utility functions, you do not need to refer to "the" psychological theory of this or that phenomenon in order to compare it with the economic theory, as you do if you organize economic psychology around testing rationality. Instead, you can refer to whatever data have been collected, from whatever theoretical viewpoint within psychology--which is the natural way to use psychology, a data-driven discipline. So, if we take the vexed example of the effects of direct taxation on the labor supply, what the economist needs from the psychologist is an estimate of the value people will set on leisure as distinct from consumption, or an indication of the circumstances under which they will set a high value on leisure rather than on consumption, so that we can tell how things will turn out if we increase taxes. Without this information, utility maximization can only say that people might work harder or less hard, longer hours or fewer hours, depending on the detailed circumstances. Psychology is a science of details, and correctly used that can become a strength and not a weakness.

The losses in formalism

There is, indeed, no such thing as a free lunch. What do we lose if we take up a formalist position? As we argued in our text book (Lea et al, 1987, Chapter 5), the rationality question has served as a Kuhnian paradigm for economic psychology. It has allowed us to do "normal science", because it gives us something we can always ask, as economic psychologists, about any new economic behavior

that falls our way. Is behavior in this new situation rational? It is like a handle we can always turn, and rely on getting out some kind of tune. Suppose, one peaceful afternoon, I am in the middle of writing, let us say, a chapter for a book which I had promised the editor months ago. Out of the blue, a journalist rings me up and says, for example, you're an economic psychologist, what do we know about the ownership of pets? Now so far as I know there is only one paper on the economic psychology of pet ownership, and that hasn't yet been published (Endenburg, Hart & Bouw, in press); there are a few published econometric studies (and probably a whole lot of unpublished ones done by the manufacturers of pet food), and there are a few psychological studies done by clinical psychologists concerned about the companionship effect of animals. So how can I answer the inquiring journalist? I can always say, well the interesting question about the ownership of pets is whether this behavior is rational or not, and of course economists say that it is and psychologists say that it isn't and... and by this time the journalist has lost interest and rung off, and I can get on with finishing my chapter. But an opportunity has been lost.

Psychologists should recognize a parallel here. The rationality question is rather like the nature/nurture question. Every psychology student learns, early in his or her career, that you can always ask, about any human behavior whatever, whether it is the product of our genetic inheritance or the result of our upbringing. It happens to be the most boring and least productive question you can ask, but if you do not have much imagination you can always ask it.

As a formalist, I suggest that this is precisely the situation of the rationality question in economic psychology. It is, admittedly, a question you can always ask. But if you do, the answer is rarely if ever informative.

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Chapter 5

Cognition and Economic Psychology

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Summary

This chapter introduces the main topics of cognitive psychology, attention, interpretation and integration, storage, retrieval, and problem-solving, and illustrates applications in economic psychology. The importance of two types of theories are specifically stressed: models of cognitive structure, i.e., the organization of knowledge in memory, and models of unconscious cognitive processes. Especially when combined, these types of theories have a vast potential for analyzing economic behavior.

Key words

Cognition, information processing, knowledge, unconscious processes

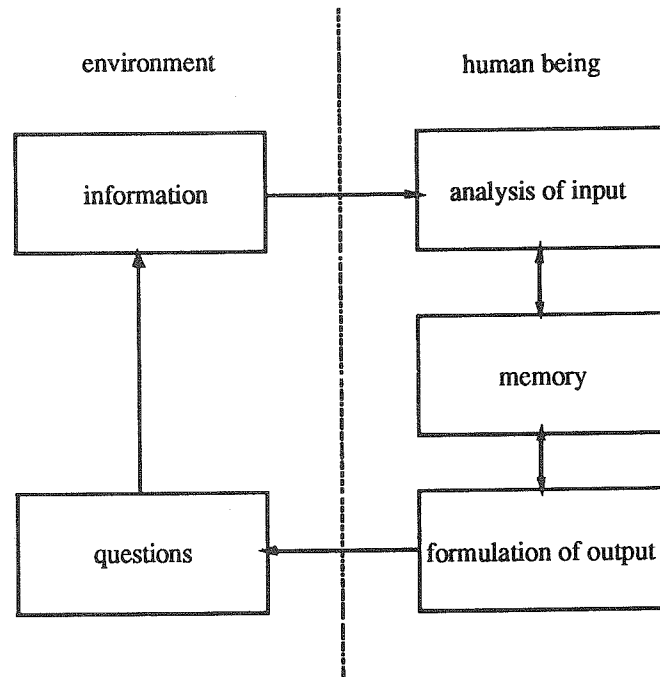
Why cognition?

Theories from cognitive psychology have been widely used in economic psychology. The analysis of consumer behavior, one of the most well-developed areas of economic psychology, has made heavy use of theories on individual decision-making. Research on inflation and unemployment has used theories on perception and mental organization. Work on entrepreneurship and managerial decision-making has used theories on cognitive biases. For further or more detailed examples, all basic textbooks in economic psychology may be consulted (Furnham & Lewis, 1986; Lea, Tarpy & Webley, 1987; van Raaij, van Veldhoven & Wärneryd, 1988; see also the contributions in K.G.Grunert & Ölander, 1989).

This is not astonishing. Economic behavior is concerned with the attainment of goals under constraints in a social environment. Cognitive psychology is concerned with how human beings analyze information obtained from the environment, how it is stored in memory, and how the stored information can be used to acquire and interpret new information and direct behavior towards the attainment of goals (figure 5.1). This basic view of the human being as an information processor who interacts with the environment in order to attain goals is congenial to the economic view of people (cf. Simon, 1990).

The cognitive view of human beings is, however, of fairly recent origin. Under the reign of behaviorism and neobehaviorism, human information processing was not a matter of concern. The *reaction* of the human to events in the environment was in focus. It was only in connection with the cognitive revolution in the sixties that the *active* role of human beings in obtaining information from the environment and orienting their behavior towards self-determined goals became the dominant theme.

Figure 5.1
The Cognitive View of Human Beings



Still more recently, however, the heavy emphasis on cognitive theories in economic psychology, and especially in the area of consumer behavior, has come under attack (e.g., Foxall, 1987; Nakamoto, 1987). It is argued that the cognitive

emphasis is at the expense of analyzing emotional processes, and that an information-processing view implies a rational human being, whereas psychology's contribution to explaining economic behavior should, on the contrary, refer to the non-rational aspects. It is also criticized that increasingly complicated cognitive models become increasingly remote from empirically testable aspects of human behavior.

I hope to be able to argue in this contribution that much of this criticism is founded on misunderstandings and/or a narrow view of cognitive psychology, where cognition is equated with conscious problem-solving behavior. I will try to show that cognitivism implies a broad view of humankind with a wealth of implications for economic behavior, only a few of which have received research attention to date.

In the following, I will highlight what I regard as basic aspects in the central cognitive processes of *attention*, *interpretation and integration*, *storage*, *retrieval*, and *problem-solving*. I will point out main theoretical insights and empirical results, and draw conclusions of relevance for economic psychology.

Attention

The study of attention is mainly the study of selectivity in perception. Due to the limitations of the human capacity for conscious information processing, only a small number of the cues somebody is exposed to are consciously perceived and possibly stored for later retrieval and use (Johnston & Dark, 1986). The question at which level of processing this selection of incoming stimuli occurs has been subject to considerable debate - does one simply screen out certain sources of stimuli, like certain sensory organs, does one screen out stimuli based on their general physical characteristics, or does some substantive processing take place before the selection is made?

From the many models of selective attention that have been presented during the last 20-30 years, the early models by Broadbent (1958) and Norman (1968) are particularly well-suited to illustrate the main issues. Broadbent (1958) suggested that stimuli are selected mostly on the basis of their physical characteristics, like pitch, intensity, spatial localization of sounds etc. This kind of selection can occur at a very early processing stage, where the substantive meaning of incoming stimuli has not yet been analyzed. Incoming stimuli are conceived as being organized in a number of information channels, which are characterized as having common physical characteristics - like information received from a particular person, or received by some particular sensory organ.

The Norman (1968) model on the other hand assumes that *all* stimuli are analyzed not only with respect to their physical characteristics, but to a degree which allows us to attach meaning to them. Incoming stimuli thus activate their concep-

tual counterparts in the long-term store. The amount of this externally caused activation, together with some internal source of activation which Norman calls *pertinence*, will determine which stimuli will be selected for attention and further processing. Norman thus assumes that selection takes place primarily because of the stimuli's *personal relevance*. This presupposes that all stimuli, in the pre-attentional phase, are in some way interpreted in order to be able to judge their relevance.

The major experimental paradigm which helped to decide between the Broadbent and Norman views on attention was the so-called cocktail party situation. At a cocktail party, one usually concentrates on the person one is conversing with, and usually screens out all the other conversations going on at the same time. This is completely compatible with the Broadbent view. But when something of particular personal salience is mentioned in other conversations - like one's own name - this will attract attention as well. The surrounding conversations are simultaneously scanned to an extent which makes it possible to detect salient words or topics. This is not explicable by the Broadbent model. *Shadowing tasks* have been the main experimental analogue to the cocktail party phenomenon: A subject receives two different messages to the right and the left ear, and is instructed to concentrate on one of them by shadowing it, i.e., repeating it in loud voice. Later memory tests show that part of the message which was played to the other ear nevertheless was understood, and that the likelihood of this to occur is related to the personal relevance of the information (Cherry; 1953; Lewis; 1970; Moray; 1959; Treisman; 1960).

This means that considerable unconscious information processing occurs during attention and perception. Stimuli to which one is exposed are, at least partly, semantically analyzed, i.e., linked to preexisting cognitive categories, before they possibly rise to consciousness. It also means that the process of selective attention acts like a kind of automatic relevance detector. Those stimuli, which, during the automatic semantic analysis, are linked to cognitive categories with high pertinence, i.e., cognitive categories which are currently relevant because they are associated with goals actually pursued, a problem in the process of being solved etc., will have the highest chance of being consciously perceived and stored.

This automatic relevance evaluation has considerable importance for market communications, like advertising (K.G. Grunert, 1991). Most advertisements are never consciously perceived. But many will be automatically evaluated with respect to their personal relevance. If an advertiser wants to increase the likelihood for perception of an advertisement, once the receiver has been exposed to it, then the main implication would be to include informational cues which will be deemed relevant. The problem (for the advertiser) is that many products are intrinsically uninteresting for consumers. In this case, relevance can be increased by adding relevant non-product related cues, like a celebrity endorser.

These conclusions are congruent with those of the so-called *Elaboration Likelihood Model*, which has gained some popularity in analyzing advertising effects (Petty & Cacioppo, 1983; Petty, Cacioppo, & Schumann, 1983; Schuman, Petty & Clemons, 1990). In this model, the effect of types of informational cues on attitude change is related to the degree of *involvement* consumers have with types of products - functional importance, emotional attachment, social visibility and perceived risk being the main determinants of involvement. The main prediction of the model, which has been substantiated in several empirical studies, is that "central cues", for example, functional product attributes, will be more effective for attitude change in connection with advertising high-involvement products, while "peripheral cues", which are not related to the product, like a celebrity endorser, will be more effective in connection with low-involvement products. These results are readily explained by the automatic relevance criterion operating in selective attention.

Other applications in economic behavior readily come to mind. How does an entrepreneur perceive economic opportunities in the environment? How does a broker scan the unbelievable flow of information in a stock exchange? How is the tax controller's attention directed to those parts of the tax declaration which hint at illegal tax evasion?

The mechanism of selection in attention is largely outside volitional control. Nevertheless, it quite obviously contributes to an information intake which appears very reasonable. It could even be called some kind of "cognitive rationality" (Gottschalk & K.G. Grunert, 1982). Given the limitations of the cognitive system, people use their limited cognitive resources in such a way that they concentrate on those informational elements in their surroundings with the highest subjective importance.

Interpretation and integration

If we accept that incoming stimuli are automatically analyzed with respect to their semantic content, how does this linkage between physical stimuli and semantic categories actually work? This is a very difficult and in many aspects also unresolved question. But one can get some insight by imagining a memory structure in the form of a hierarchical network. Memory nodes corresponding to abstract concepts, like *my name*, are hierarchically linked to other nodes corresponding to phonemes, which again may be linked to nodes corresponding to acoustic features (Elman & McClelland, 1985). Automatic stimulus recognition may then be explained by the activation of this sequence of associations, and the stronger these associations are, i.e., the more familiar the stimulus, the better will this recognition work.

The recognition of stimuli depends therefore always on previously stored information. Collins and Quillian (1972) have demonstrated this with a nice example: The sentence "The policeman raised his hand and the cars stopped" is immediately understandable by most adult human beings. Still, this understanding requires considerable previous knowledge. Imagine that the cars were parked on a hill and started rolling because of an earthquake. The sentence "The policeman raised his hand and the cars stopped", in this context, would at once provoke the question: how did the policeman manage that? The reason that we, in a normal context, do not ask this question is that we know how a car operates, that in order to move it needs a driver (under ordinary circumstances), and that drivers have learned in driving school to stop when a policeman raises his hand.

Only the most well-known stimuli can be "recognized". In other cases, the term "interpretation" describes more exactly the attempt to make sense out of what one perceives. Many stimuli will, during the perceptual process, be linked to several cognitive categories, without perfectly matching their semantic definition. Interpretation can be viewed as a process of *categorization*: It is attempted to sort objects or events perceived in the environment into the cognitive categories which organize previous experience. The less familiar the object or event, the more ambiguity is there with respect to the categorization. Taking again an example from market communication, a new low-calorie chocolate bar may be categorized either as a sweet or as a health food. Which categorization finally comes to dominate consumer perception probably will have huge commercial consequences, and advertising people would be well advised to study the categorization process in detail.

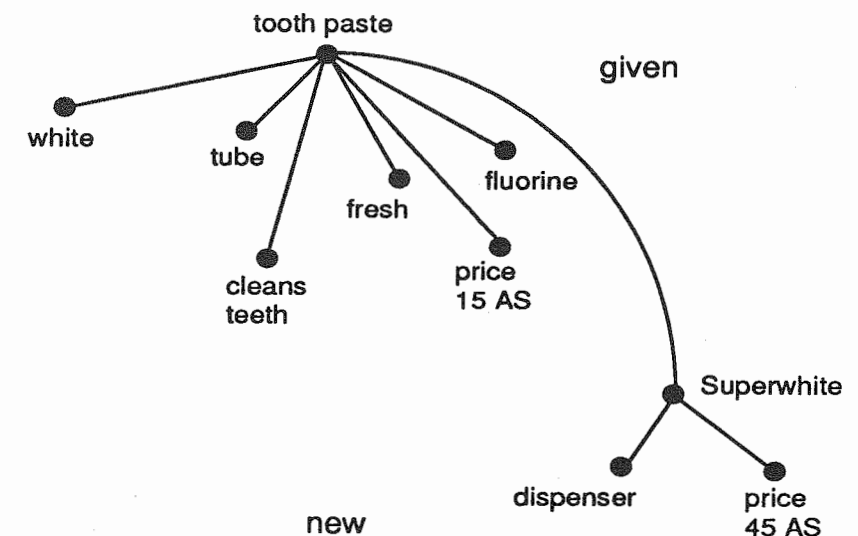
If a perceived object or event is new, it cannot completely match a pre-existing cognitive category. It will in some way deviate from what was previously known or experienced. Most models of the organization of knowledge in memory assume that the storage of new information follows the *given-new principle* (Clark & Haviland, 1974): Only the ways in which the perceived object or event deviates from the known cognitive category are stored. Figure 5.2 shows a fictitious example of how a consumer would store information on a new brand of tooth paste, which s/he may have seen on the shelf in the supermarket. Everything about the new brand which conforms to general knowledge about toothpastes - i.e., that it cleans teeth, contains fluorine etc. - will not be stored. The only thing to be stored about the new product is how it deviates from those already known; in this case, by its higher price and different packaging.

Storage by the given-new principle results in an organization of knowledge which follows the *economy of storage principle* (Chang, 1986; Collins & Quillian, 1969): knowledge is not stored with a subordinate cognitive category, if it can be inferred from a superordinate category. Thus, the fact that toothpaste cleans

teeth will be stored with the generic category *toothpaste*, and not with every brand of toothpaste one happens to know.

The given-new and economy-of-storage principles imply that learning will be incremental and culturally dependent. What is given and what is new will differ between various recipients of a given message, and these differences will be strongest when the recipients come from different groups which each have their collectively shared set of cognitive categories - i.e., from different cultures or sub-cultures. The question of cultural differences, a topic receiving widespread attention in business research, can therefore fruitfully be analyzed using concepts from cognitive psychology. Likewise, the cross-cultural validity of survey research instruments can be investigated by studying how the instrument relates to cognitive categories in the various respondent groups (S.C. Grunert, K.G. Grunert & Kristensen, 1992).

Figure 5.2
Example of Given-new Principle



Interpretation and integration processes have not been widely studied in economic psychology. In advertising research, the question whether an advertisement conveys the intended meaning is addressed, but with little use of concepts from cognitive psychology, like categorization. There has been some research on the categorization of prices as acceptable or unacceptable (e.g., Helgeson & Beatty, 1985; Kosenko & Rahtz, 1988; Lichtenstein, Bloch & Black, 1988).

Storage

Associative networks have already been presented in the preceding section as a possible model of the organization of knowledge in memory. The major models competing with associative networks are *distributed memory* models and *schemascript* models.

Associative network models assume that the organization of knowledge can be modeled as a set of nodes and links. The nodes represent cognitive categories and the links associations between these categories. The links can vary in strength, and, in some models, also in type. Most network models assume the economy-of-storage principle. The major models of this nature are the ones by Anderson (1976, 1983a), Norman and Rumelhart (1975), and Quillian (1968). Several applications to economic behavior have been attempted (K.G. Grunert, 1982a, 1990; Lunt, 1989; Lunt & Livingstone, 1991; Snelders, Hussein, Lea & Webley, 1991; Williamson & Wearing, 1991).

Figure 5.3

A Network Representation of Consumer Knowledge (adapted from K.G. Grunert, 1990)

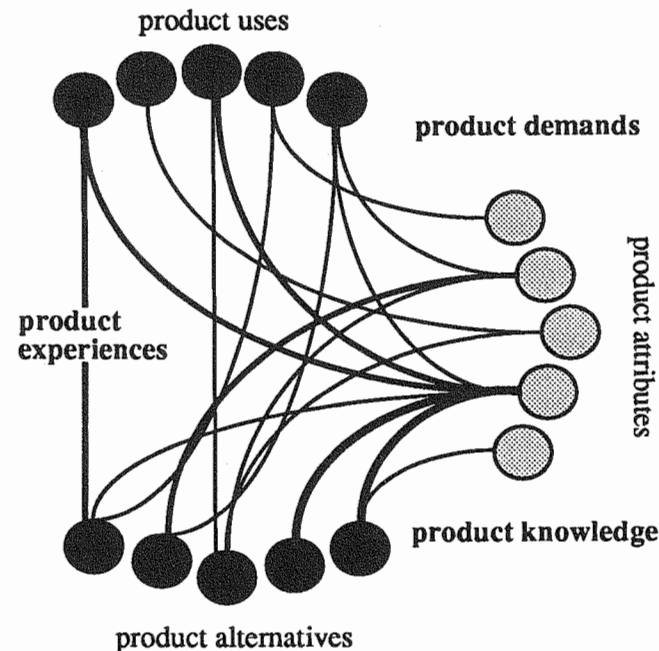


Figure 5.3 shows the general structure of a network representation of knowledge concerning consumer products (adapted from K.G. Grunert, 1990). Three kinds of cognitive categories are distinguished: product alternatives, product attributes, and product uses. Linking product uses to product attributes results in associations

mirroring consumer demands, i.e., attributes that are desired for certain uses. Linking product alternatives to product attributes results in associations describing how alternatives differ with regard to these attributes and may hence be called product knowledge. Finally, direct links between product uses and product alternatives are assumed to result from product experience. The main idea is that the cognitive structure is organized in such a way that it enables consumers to detect whether a product is useful for intended uses. In order to do this, attribute information is useful, if there is no own experience which has resulted in direct links between alternatives and uses. Based on how elaborate each of the three parts of the cognitive structure is, different types of consumers can be distinguished: consumers who lack both experience and knowledge (when a product is bought for the first time), consumers who have knowledge, but little experience (the car freak knowing everything about Porsche, but without means to buy one), and consumers who have experience, but little knowledge (which may be typical for many convenience products like toothpaste). Based on these distinctions, hypotheses about information acquisition and purchase behavior of these various types can be formulated.

Figure 5.4

Example of a Network Representation of the Causal Structure of Unemployment (from Lunt, 1989)

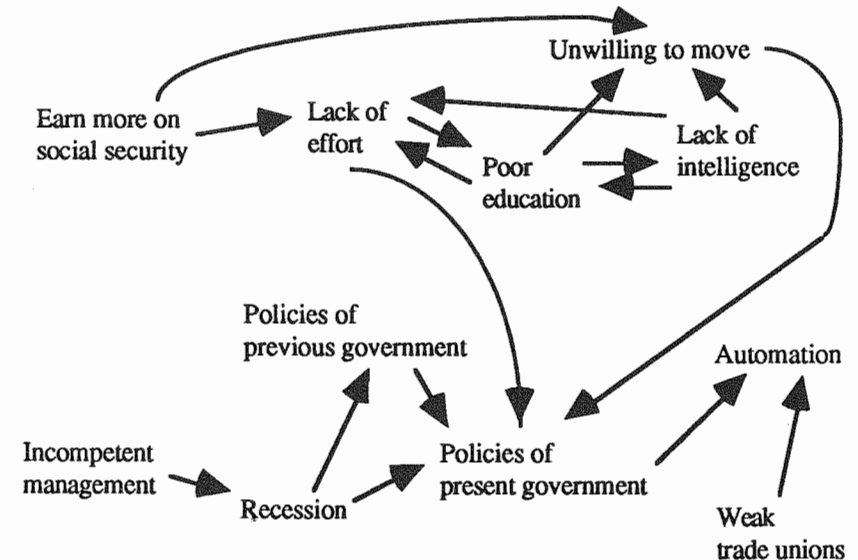


Figure 5.4 shows another example of an associative network about economically relevant knowledge. It is from an empirical study by Lunt (1989), who has measured the perceptions of the causal determinants of unemployment for people with different political affiliations. Figure 5.4 shows the cognitive map of

Conservative voters. As would be expected, the map of Labor voters looked quite differently (see Lunt, 1989, for the other maps).

Network models have been criticized because they cannot handle imagery, and because they are limited to semantic - as opposed to episodic, i.e. time-place-specific - information. Actually, this applies only to the earlier models. Episodic information can be presented as a linear sequence of nodes and links, organized by time. A node can easily be an image - or a feeling or emotion, for that matter (see, e.g., Bost, 1987; Bower, 1981; Srull, 1987).

The network models discussed so far should more precisely be called *positional* networks, because a piece of knowledge has a specific position in the network. Distributed memory models (e.g., Kohonen, Oja & Lehtio, 1981; McClelland & Rumelhart, 1985) are networks as well; the difference is that they are not positional, i.e., a node does not correspond to a single piece of information. Instead, all information is distributed across the whole network. New incoming information provides a pattern of activation, which spreads throughout the network and changes the strengths of the links of the network. Learning occurs hence by consecutive changes of link strengths. Knowledge is retrieved by applying part of the original activation pattern; after this has spread throughout the network, the resulting response activation pattern will resemble the one originally caused by the to-be-recalled event. Distributed memory models perform impressively in explaining recognition and recall phenomena, but have not yet been applied to economic behavior.

Schemas (alternatively also called *scripts* or *frames*, see Minsky, 1975; Rumelhart & Ortony, 1977; Schank & Abelson, 1977) are in certain respects a richer concept than networks, because a schema is not only a model of knowledge representation, but also a model of cognitive processing. A schema not only stores information, but, when incoming information instantiates the schema, it also leads to the formulation of expectations, proposes relevant questions to the environment, and suggests default values for missing information. There has been some use of the schema concept in connection with economic behavior (S.C. Grunert, 1993; Peter & Olson, 1990; Sujan & Bettman, 1989).

The three approaches obviously complement one another. Distributed memory models are probably much closer to the neurophysiological functioning of memory than the other models, but so far their explanatory power is restricted to very basic phenomena of storage and retrieval. Positional network models are more remote from the neurophysiological basics, but lend themselves more easily to modeling knowledge structures in an applied area like economic behavior. Schemas go beyond positional networks in that they not only represent *declarative* knowledge, i.e., knowledge about facts, events, and objects, but additionally integrate aspects of *procedural knowledge*, i.e., the storage of perceptual and motor skills, and include a dynamic element in that they combine cognitive structure and cognitive processes. This gain in breadth comes at a price, of course, in that

schema models have generally less precision than network models, and that the question of how Schemas develop has not been answered convincingly.

Since it is not possible to prove that one model of cognitive structure is more correct than another, unless one combines the structural model with a process model and hence incorporates it into a more complete model explaining the kind of human behavior one is interested in, the choice of a model of knowledge representation will have to depend on its usefulness in explaining and predicting sets of behavior. In economic psychology, distributed memory models will probably be less useful, because they predict behavior at a micro level which is relatively remote from economic behavior. Positional network and schema models can however be fruitfully applied in economic psychology. Positional network models will be appropriate whenever the main interest is on how factual knowledge about a class of events or objects is organized and how it develops over time. Two examples of this have been shown. Schema models will be appropriate when the emphasis is on the dynamics of man-environment interaction within a reasonable time span. Examples could be behavior in a shop or in a restaurant (Bower, Black & Turner, 1979), buyer-salesman interaction (Heroux, 1987; Leigh & McGraw, 1989), and consumer behavior in reaction to emotional distress (S.C. Grunert, 1993).

Retrieval

How is information retrieved from the cognitive structure? A positional network model of cognitive structure will be assumed in the following (schema models, as mentioned, have retrieval processes partly integrated into the structure model). For positional network models, the process of retrieval can conveniently be described as being based on *spreading activation*. Spreading activation theory was mainly developed by Collins and Loftus (1975) in connection with their work on semantic memory, and was later extensively used by Anderson and his coworkers as a basic mechanism to explain the nonconscious processes in the cognitive system (Anderson, 1983a, b; Anderson & Pirolli, 1984). Every node in a network, according to spreading activation theory, can have varying degrees of activation. Activation can result from external and internal stimulation, just as in the Norman model of selective attention described above. Thus, when an external stimulus is linked to a cognitive category, the memory node corresponding to that category will become activated. In addition, internal processing causes certain parts of the cognitive network to be more activated than others. This internal activation is determined largely by strategic processes, i.e., by conscious thinking, goal-setting, problem solving etc. From the activated nodes, activation will spread throughout the network, according to the following principles (Balota & Lorch, 1986; K.G. Grunert, 1990):

activation spreads in parallel, i.e., simultaneously from all activated nodes, and along all links emanating from a node;

in that process, activation will be lost, and the loss will be inversely proportional to the strength of the link between two nodes;

the resulting activation of some node will be the sum of all ingoing activation, no matter whether externally or internally caused;

when there are no new sources of activation, the activation pattern in the network will very rapidly approach some asymptotic pattern.

It is further assumed that the amount of activation received by any particular node in memory is linked to consciousness. Whenever the activation of a node surpasses a threshold, the corresponding cognitive concept will become conscious. This threshold may vary in a way which keeps the number of nodes which are conscious at any particular point in time at some optimum level (cf., e.g., Anderson, 1976; K.G. Grunert, 1982; Norman & Rumelhart, 1975; Shiffrin & Schneider, 1977).

Spreading activation thus determines which information from memory will be consciously remembered. In this sense it is a mechanism of retrieval. Since spreading activation *results* in consciousness, it must by necessity be an unconscious, *automatic* process. Since spreading activation is a parallel process, it is not subject to capacity limitations, in contrast to conscious (*strategic*) cognitive processes.

Spreading activation theory is an interesting example of an attempt to study unconscious cognitive processes. It must be noted that the implications of these unconscious processes for human behavior are enormous. All consciously controlled behavior, including economic behavior, is framed by the information which we retrieve from memory. Things not remembered cannot be considered. The voluntary decision to behave in one way and not in another is thus limited by the results of unconscious retrieval processes.

Retrieval can actually be more than a mere remembrance of facts. Spreading activation theory can explain unconscious information integration: it can be shown that the amount of activation finally received by the nodes corresponding to alternatives in a decision situation can be explained by a linear decision rule integrating the attribute information stored about the alternatives (K.G. Grunert, 1982b). In addition, when possible alternative courses of action are retrieved in a decision situation, the amount of activation received by each may, beyond consciousness, be used as a heuristic for the appropriateness of the alternatives in handling the situation (K.G. Grunert, 1989, 1990).

"Spontaneous" evaluation of objects perceived in the environment can also be explained by unconscious retrieval processes. As noted above, perception involves linking the perceived objects to cognitive categories and activating them. According to spreading activation theory, every perception will automatically initiate re-

trieval, since the activation will spread throughout the network. This may result in the retrieval of evaluations stored in connection with previously experienced similar events. This is of interest not only in connection with decision-making, but also with respect to explaining emotional arousal - a question which has received little attention in economic psychology to date (an exception is S.C. Grunert, 1993).

It is therefore unfortunate that economic psychology, when using cognitive theories, has mostly concentrated on conscious processing. Unconscious information processing, both in attention and retrieval, has a powerful impact on the courses of economic action humans perceive as possible, and on the way they are evaluated. There is an array of methods which can be used in studying it, like reaction times, naming latencies, sequence analysis, and similarity scaling (Friendly, 1979; K.G. Grunert, 1990; Snelders et al., 1991).

Problem solving

Problem solving has been the cognitive process which has received most attention in economic psychology, especially in the context of decision-making between alternatives. Numerous studies have dealt with the application of various choice heuristics, decision networks, constructive decision-making etc. (much of the earlier research of this kind in the consumer behavior literature is summarized in Bettman, 1979). In recent years, the attractiveness of this research topic seems to have decreased, however, probably in conjunction with the limited predictive success of many studies, and the general criticism of the cognitive approach mentioned in the first section.

As I hopefully have shown by now, equating a cognitive approach with the study of conscious information processing leads to neglecting the greatest potential of cognitive psychology. There are good arguments for why the analysis of conscious problem solving is actually the most difficult part of cognitive psychology with respect to prediction. Problem solving is subject to volitional control, and hence easily adapted to task circumstances. Over a set of tasks they will hence have limited stability, which makes prediction difficult.

That does not mean that problem solving cannot be analyzed. But the elements of problem solving most amenable to theorizing and successful prediction will not be those which are under the subject's volitional control. Instead, one can successfully analyze the *framing* of problem solving by retrieval processes, as discussed in the preceding section, and one can analyze the *automatized elements* in problem-solving processes. Routine in solving certain kinds of problems leads to the establishment of procedural knowledge, i.e., skills which guide the acquisition and perception of information, search procedures in memory, information integration procedures etc. These automatized elements cannot usually be verbalized - which

makes them more difficult to analyze - but will, on the other hand, be stable over a wide variety of situations, which makes them attractive objects of investigation. This is why the analysis of *expert* problem solving has been one of the most successful areas of problem solving research (e.g., the work by Newell & Simon, 1972): because they will have a set of automatized problem solving routines. Much research in consumer behavior, however, has been characterized by analyzing people *as if* they were experts - by putting people into a complex situation, usually involving a choice between several multi-attribute alternatives, which led people to establish ad-hoc problem-solving procedures, with little or no external validity.

There are of course many instances of expert problem solving in economic behavior - managers, brokers, politicians, salespeople, consumers, all are experts in certain types of problems. The automatized elements in their problem-solving behavior are, in the business literature, many times called *intuition*. This may be a misleading term, if intuition is interpreted in such a way that it means unexplainable or unanalyzable (see my contribution on *strategic management* in this volume). To the contrary, the study of intuition in problem solving may be one of the more promising areas of research.

Conclusion

In this contribution, I have attempted to show that a cognitive approach has promise for a wide variety of phenomena in economic psychology, and that a cognitive approach has a much broader scope than is often realized. I have, especially, tried to present the argument that the analysis of *unconscious cognitive processes* should receive much more emphasis than it has up to now. Unconscious cognitive processes determine what we perceive, determine our spontaneous reactions to new events, determine what we remember, and, to some degree, how we solve problems. More insight into the unconscious processing of information related to economic events should be a main issue of future research in economic psychology.

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Chapter 6

Psychological Aspects of Strategic Management

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Summary

This chapter introduces strategic management both as a business practice and as an area of research. Strategic management is characterized as a business orientation with a top-down approach to addressing markets, as opposed to the bottom-up approach proposed by traditional marketing orientations. Three schools of thought are distinguished in research on strategic management: the design school, the planning school, and the shared experiences school. None of them has made much use of psychological concepts, but it is attempted to show that each of them could profit considerably by making use of psychological theories and methods.

Key words

Competitive advantages, strategic management, strategic planning

What is strategic management?

Strategic management is not an area psychologists, even economic psychologists, have been very concerned with. However, I will argue in this chapter that they should be - because the topic is one which is perceived as important by both business academics and business practitioners, because it involves a lot of questions where psychologists can contribute to answer them, and, last but not least, in order to break the peculiar barrier which exists between business strategists and behavioral scientists - a topic I will return to shortly. In this chapter, I will first explain what strategic management is all about, and trace its roots both in academia and in real life. I will then distinguish some schools of thought within the area,

give examples of each, and point out possible contributions of psychology along the way.

A definition of strategic management

Elsewhere in this volume, Werner Güth defines a *strategy*, from the viewpoint of game theory, as a *complete behavioral plan for all forthcoming periods*. That definition could as well be applied to a business strategy: a plan, reaching far ahead into the future, that specifies feasible answers to all events that could possibly happen in the business environment. However, as Güth also points out, it is usually not possible to have such a complete behavioral plan, so for all practical purposes one has to have some more modest definitions. Even though definitions of strategic management, as with every popular concept, abound, I believe the following definition covers most of the aspects which are ascribed to the concept in the literature:

Strategic management is the planning and implementation of integrated actions in the pursuit of sustainable competitive advantage, based on a systematic analysis of the strengths and weaknesses of the business and threats and opportunities in the environment.

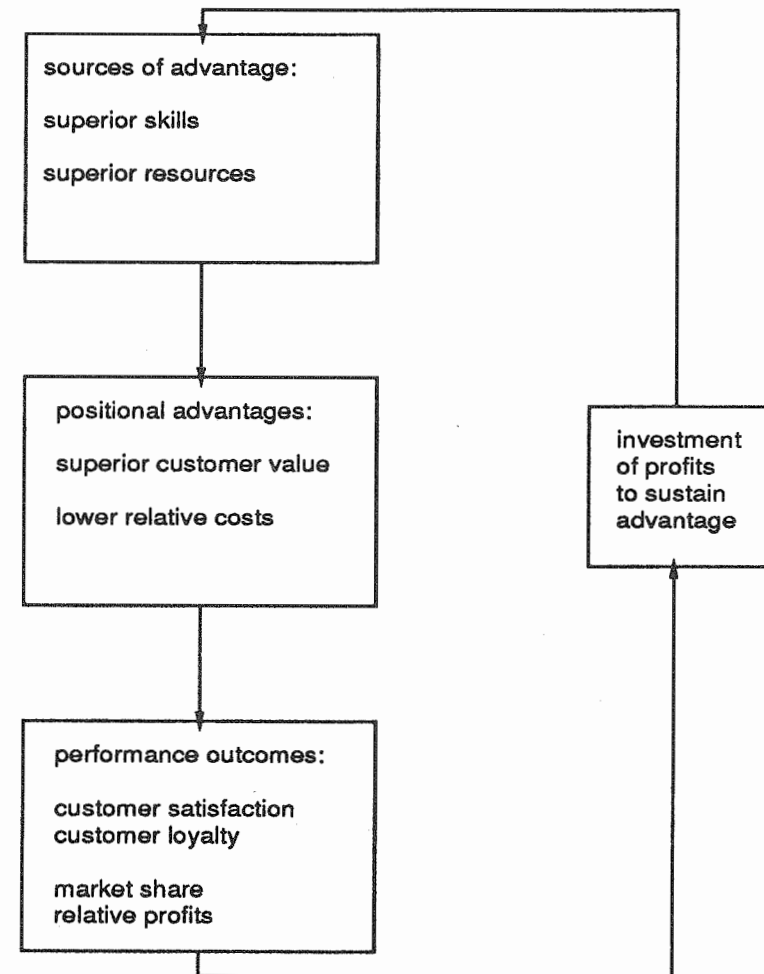
A systematic analysis of strengths and weaknesses of the business and threats and opportunities in the environment expresses the idea of *business ecology*, which is a crucial one in this area: the notion that the most successful businesses are the ones which have found the best match between internal characteristics and the environment. Achieving such a match will lead to a *competitive advantage* with respect to rival businesses. This is another very basic concept, referring to the observation that in every market there are usually a few companies which consistently perform better than the other companies in that market. The aim of strategic management is thus to achieve superior performance. *Integrated actions* refers to the idea that strategic management is *not function specific*. Business administration is generally very much divided into various business functions: marketing, finance, production etc. In strategic management, these various functional areas of the business are to be integrated in order to be able to pursue a sustainable competitive advantage. Finally, the definition refers to *planning and implementation*. Strategic management developed from the idea of business planning; the notion *strategic planning* may actually be more familiar to many than *strategic management*. The emphasis in strategic management on planning and implementation mirrors the abandonment of the earlier belief that competitive advantage is something which is achieved mainly by top management, preferably by the chief executive officer, and that the rest of the business then just had to carry out what was decided at the top. Now, implementation is regarded as at least equally important as planning in strategic management.

The concept of competitive advantage

I would like to briefly elaborate on the concept of competitive advantage, because it is so central to the topic to be addressed. Figure 6.1 shows a model, developed by Day and Wensley (1988), which summarizes much of the current thinking in the area.

Figure 6.1

The Day and Wensley (1988) Model of Attaining Competitive Advantage



Attaining competitive advantage as a business goal actually alludes to the fact that the nature of competition has changed within the last 100 years or so. In order

for business to be successful in the long run, so goes the main argument, it is usually not helpful to compete with other businesses head on - i.e., to compete on the same dimensions, such as price or basic product quality, especially not if the competing businesses have strengths with respect to these dimensions. Rather it would be preferable to find areas for competition where the other businesses are weaker. Thus, every business in the market should try to find some area where it has particular strengths, and not try to imitate its competitors with respect to their particular strengths. Day and Wensley call these possible sources of advantage *superior skills and resources*, where superior is always defined with respect to competitors. A superior skill is anything which has to do with the qualifications of personnel, and superior resources may be financial resources, the location of the company, technology, a well-established brand name etc. These *sources of advantage* can possibly be turned into *positional advantages*, and here the literature mainly distinguishes between two kinds: *superior customer value*, i.e., skills and resources are used to produce products or services which the customers perceive as having a value superior to that of the competing products, and/or *lower relative costs*, i.e., the business succeeds in having lower costs than the competing businesses. These positional advantages have then consequences for *business results*. They can result in higher *customer satisfaction* and *customer loyalty*, and this again will increase *market share* and *profit* for that business. The cash which is generated in this way can then be used to invest into either sustaining the skills and resources which gave rise to these advantages, or to invest in the development of new skills and resources, if it should turn out that the environment in which the company is operating is changing very rapidly, so that advantages which are valuable at this point in time may no longer be valuable in the future - for example, because of changes in customer life styles and tastes, because of new technologies which erase a cost advantage etc.

Strategic management, according to the definition given above, aims at helping businesses identify their sources of advantage, and turn them into positional advantages, and finally into good business results. The whole process depicted in figure 1 can be called the process of attaining competitive advantage.

The elements of the strategic management process

The literature is replete with morphologies of the elements of the strategic management process. At the very basic level, all these morphologies boil down to four elements:

Situation assessment: In what environment is the business operating, and what are possible sources of advantage?

Strategy generation: What possible business strategies, centered around the two general notions of creating perceived value and lowering costs, could the business pursue?

Strategy selection: choosing the strategy which seems most promising;

Implementation: designing the business and its external relationships so that the chosen strategy can be fulfilled.

To each of these elements it is immediately possible to attach examples of the possible contribution of psychology:

Situation assessment is obviously a question of how the environment is *perceived* by the decision-maker. Psychology could contribute considerably with its knowledge about the biases operating in perception and attribution, and could also contribute by developing *measurement methods* aimed at assessing business environments, which avoid some of these biases.

Strategy generation is in the first place a question of *creativity*, where psychology has a set of methods concerned with enhancing creativity. But at least as important would be the contribution of *behavioral theory* towards analyzing the *effects* of strategies, should they be carried out. As will be shown shortly, such analyses are often lacking from the discussion of business strategies now.

Strategy selection is an example of complex *decision-making* under uncertainty. This phenomenon and the various cognitive biases characterizing it have been widely researched in psychology. There have also been attempts to develop training programs for managers and other decision-makers based on these results (e.g., Russo & Schoemaker, 1989).

Implementation is, of course, related to *organizational psychology*. This is widely accepted, and will hence not be pursued further here.

Top-down and bottom-up view of addressing markets

There are thus quite a number of possible contributions of psychology to strategic management. Why has there been so little contact between these two areas of investigation?

A reason may be that psychology in business administration, and especially in the area of marketing, has been mostly concerned with that part of the interface between the business and its market which is usually called *tactics*. Tactics, in business as everywhere, refer to small, short-run problems, like designing an advertisement or choosing a sales outlet, in contrast to the strategic problems, which have to do with the general attitude the business wants to adopt with respect to its market offerings. Psychology has been associated very much with the former, not

with the latter - with how one should design a package, how one should develop a certain product, which chain of distribution to choose, etc., but not with how to decide on the business mission, whether to pursue a high-quality strategy, etc. Psychology has thus been used in fine-tuning the tactics of marketing to the needs and the behavior of the customers in the market.

Table 6.1

Top-down versus Bottom-up View of Markets (from Day, 1984)

ISSUE	TOP-DOWN VIEW	BOTTOM-UP VIEW
Definition of market	Markets are arenas of competition where corporate resources can be profitably employed	Markets are shifting patterns of customer requirements and needs that can be served in many ways
Orientation to market environment	Strengths and weaknesses relative to competition - cost position - ability to transfer experience - market coverage	Customer perceptions of competitive alternatives - match of product features and customer needs - positioning
Identification of market segments	Looks for cost discontinuities	Emphasizes similarity of buyer responses to market efforts
Identification of market niches to serve	Exploits new technologies, cost advantages, and competitors' weaknesses	Finds unsatisfied needs, unresolved problems, or changes in customer requirements and capabilities
Time frame	2 to 5 years	1 to 3 years

Starting from customer needs, and then aggregating into segments and markets is also called *bottom-up marketing*. It is the core of the classical marketing concept developed in the fifties and sixties. In contrast, the way of thinking in business strategy has mostly been the other way round. Strategic thinking, according to its main proponents, starts at the top, with questions about the mission of the company, a very basic and general idea about which markets to serve with which offering in which way. From this general idea one then works the other way and, at some point in time, maybe one also tries to analyze the implications for some

of the more concrete questions concerned with how to serve a particular market segment. The strategic approach towards addressing a market can hence be called *top-down marketing*. Table 6.1 (from Day, 1984) elaborates the distinction between the top-down and bottom-up views of markets in somewhat more detail.

In strategic management the emphasis has predominantly been on the aggregate instead of the individual, which has not led strategists naturally into the realms of psychology when they were looking for suitable theory and results. Instead, they have recently been turning towards *industrial organization theory*, which, for a long time, has been analyzing determinants of the intensity of competition in a market or industry, although from the viewpoint of national welfare and not from the individual business viewpoint. Michael Porter, now one of the main gurus in the area of business strategy, became famous mostly on the grounds that he went through the industrial organization literature and tried to adapt it to the purposes of strategic management (see Porter, 1980, 1985).

These two views on how a business can deal with its market, bottom-up marketing and top-down marketing, strategic marketing or behavioral marketing, have been very much isolated from each other in the academic community, and the non-use of psychology in the area of strategic management is just one aspect of this. This division is a very unfortunate state of affairs, because one would assume that both ways of thinking would supplement each other in our understanding of what makes businesses succeed in competition.

Why strategic management came into being

I would now like to address some of the reasons why strategic management has developed, both as an issue in business and as a topic in academic research. Ansoff (1987), one of the mentors of the field, argues that strategic management is something which has developed over time in large American enterprises, and was hence a real phenomenon which academia then took up mainly on a descriptive basis. According to Ansoff, the ways American businesses have pursued and organized their planning activities over the last sixty years can be boiled down to four types, which form a sequence over time. First, there was the *organic* planning type, which basically meant there was no planning. The business was largely unmanaged and simply consisted of day-to-day operations. Then there was the *reactive* type, which reacted to changes in the environment, but without a long-term vision of how it should react when the necessity arises. If one could talk about a basic business strategy here, it just emerged because of incremental changes. The third type he calls *ad hoc management*. This, too, is incremental, but one has an idea which kind of strategy one would like to follow by making subsequent incremental changes. Finally, there is the *systematic management* stage, where one delibe-

rately tries to develop a strategy on how the business is supposed to react to changes in the environment in the long run. Ansoff argues that some large American companies, notably General Electric and IBM, entered this stage in the sixties. Viewed in this way, the whole field of strategic management can be traced back to impulses or stimuli which were generated by the behavior of these few large American companies.

This view is not held unequivocally. It can be pointed out that the main thrust of Ansoff's work (e.g., Ansoff, 1965, 1979, 1984), and that of many of his colleagues, is *not* descriptive, but strictly normative - strategic management is the superior way of running a company, and Ansoff's four stages (which are not backed up by systematic empirical research) may just be an attempt to build an empirical underpinning for what is essentially a set of normative assumptions. From an empirical point of view, it has actually been argued that Ansoff's type four has been very rare, and that in the few instances where companies consciously tried to implement it, they have abandoned it again (Peters, 1984). Nevertheless, it is clear that the development of planning practices in large companies has given an important impetus to the development of strategic management as a research topic.

But the main underlying reason for the popularity of the topic in both business and academia seems to lie in some fundamental changes in Western markets over the past few decades. If one looks at the evolution of Western markets within the last 100 years or so, three basic orientations can be distinguished, which characterize the prevalent business attitude, and which were developed in response to the characteristics of the market (cf. Baker, 1983). In the first half of this century, many businesses were characterized by a *production orientation*. This means that businesses were in the first place concerned with producing goods in large quantities, with the effect that unit costs fell and prices came down, enabling the development of mass markets. At a time where incomes were mostly low and where there were a lot of unmet demands and needs, one can argue that a production orientation was actually the business attitude which was most likely to generate sales on the market place.

During the fifties and sixties, discretionary income rose considerably. People had more money to spend, and there were not as many unsatisfied customer needs. Sustained growth became more difficult for businesses, because delivering goods in increased quantities did no longer automatically meet unmet demands. This tightening of the market caused the *marketing orientation* to develop: Since the needs and wants of the buyers were no longer obvious, one should start by analyzing what the needs of the buyers in the market actually are. This brought us market research, the idea of market segmentation, and all the concepts which are related to modern marketing theory.

The next fundamental change more or less coincided with the first so-called oil crisis (cf. Day & Wensley, 1983). When the world economy started to slow down,

growth at the business level became more difficult. As long as there was global growth, understanding customers' wants may have been sufficient in order to participate in that growth. But once markets or the economy as a whole stagnate, this may not be enough: even if all competitors are equally good at identifying what the customers want or need, not everybody will be able to survive in that market. For this reason, a new orientation developed in the seventies, according to which looking at the customers and their needs alone is not sufficient. One should also look at competitors, because, if we take for granted that everybody analyzes customer needs, then the relative strengths and weaknesses of the companies will be the main determinant for survival or growth. We may call this the *strategic management orientation*, and from this point onwards there was the schisma between bottom-up (customer-centered) and top-down (competitor-centered) approaches to markets.

Thus, in a general sense, the strategic management orientation, just as the production and the marketing orientations, is a response to some general economic developments which have occurred in Western markets.

Schools of thought in research on strategic management

Looking at the academic analysis of business strategy, several schools of thought can be distinguished. Mintzberg (1990a), in a valuable review article, distinguishes no less than 10 schools, but for the present purposes a distinction between three may be sufficient (Grunert, 1993). I will distinguish between the design school, the planning school, and the shared experiences school.

The design school

The design school is closely related to the Harvard Business School, which, as the first business school, in 1910 developed a course in business policy. The Harvard Business School, as is well-known, was the main proponent of the case method, and their extreme views on how it should be used in teaching business are closely related to their views on analyzing business strategy. The design school, at least until very recently, flatly denied the usefulness of theorizing in the area of business strategy (see Christensen, Andrews, Bower, Hamermesh & Porter, 1987, and the critique by Mintzberg, 1990b). Business strategy can be taught, but not researched. It can be taught by presenting the students examples of concrete business situations, having them analyzed, and discussed in class. The idea is that, by looking at many cases, one develops an understanding, an intuition for business ecology. Every business is regarded as unique in all aspects, and every business there-

fore also has to find its own unique match with its environment. The ability to do this can be trained by the case method.

Thus, the design school is not a school of research in the strict sense. There is no theory development and no theory-based education, only the development of intuition by discussing as many cases as possible (cf. Mintzberg, 1990b).

From a psychological point of view, many things could be said about intuition. Herbert Simon (e.g., 1979, 1990) has strongly denounced the mysticism around intuition in his approach towards business strategy formation. Intuition, he argues, is not something mysterious, but an aspect of human behavior which can be explained. "Intuition" develops on the basis of a lot of experience with a certain subject matter, which results in certain stimulus configurations in the environment being automatically recognized and linked to appropriate reactions. Elsewhere in this volume, I discuss in more detail the notion of automatic, unconscious cognitive processes, which allow a person to subconsciously perceive a large number of stimuli and relate them to previous knowledge. How such automatic processes develop is a rather well-researched area, although unfortunately not in connection with the problem of business strategy formation. The existing research on development of automatic processing, both with respect to perceptual and motoric processes, indicates that learning is facilitated by having a phase of cognitive learning first, where the principles to be automatized are consciously processed.

If one accepts this view of intuition, then obviously the notion that intuition can be formed only by looking at practical cases without guidance by theory is plainly wrong. If we knew those aspects in the environment which successful business experts use to form a judgment and find an appropriate response, i.e., if we could develop a theory linking cues to responses, then explaining this theory to people would be a major step in training the kind of automatic processing desired. This view of intuition would hence lead to a research program in business strategy formation. The design school's conclusion, i.e., that there is no need for theory and no possibility for theorizing in that area, should hence be rejected, and recent developments at Harvard seem to indicate that one is backing away a little now from the previous extreme view.

This notion of intuition as something which can be explained by learned, unconscious cognitive processes is by no means new. Just note what another great strategist (even though not in business), Agatha Christie's *Hercule Poirot*, said in *The ABC murders*:

...there, at the very start, I made a grave error. I permitted my feeling - my very strong feeling about the letter - to remain a mere impression. I treated it as though it had been an intuition. In a well-balanced, reasoning mind there is no such thing as an intuition - an inspired guess! You can guess, of course - and a guess is either right or wrong. If it is right you call it an intuition. If it is wrong you usually do not speak of it again. But what often is called an intuition

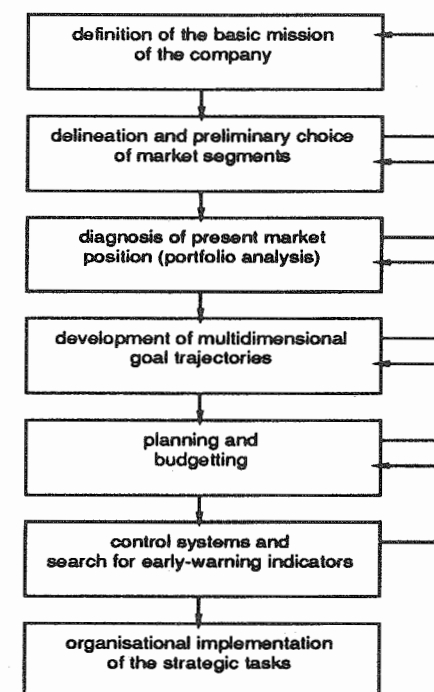
is really an impression based on logical deduction or experience. When an expert feels that there is something wrong about a picture or a piece of furniture or the signature on a cheque he is really basing that feeling on a host of small signs and details. He has no need to go into them minutely - his experience obviates that - the net result is the definite impression that something is wrong. But it is not a guess, it is an impression based on experience.

The planning school

The main message of the planning school is that we can develop planning instruments which help businesses in finding the right strategy. This school obviously has a normative thrust: one develops more or less complicated models on how a business should go about in trying to find the optimum strategy (the main mentor of this school is Ansoff, 1965, 1979, 1984).

Figure 6.2

An Example of a Simple Model of Strategic Management (from Köhler, 1981)



These models can all be regarded as elaborations of the basic morphology of the strategic management process. Figure 6.2 shows an example of one of the simpler

models, presented by Köhler (1981). The basic model consists of seven steps. First one should define the basic mission of the company. Then the basic market segments to be served are chosen, and the market position obtained to date is analyzed by means of a portfolio analysis (to be discussed shortly). Goal trajectories are then developed, measures are planned and budgeted, control systems and early warning indicators are sought, and finally the strategic tasks are implemented organizationally.

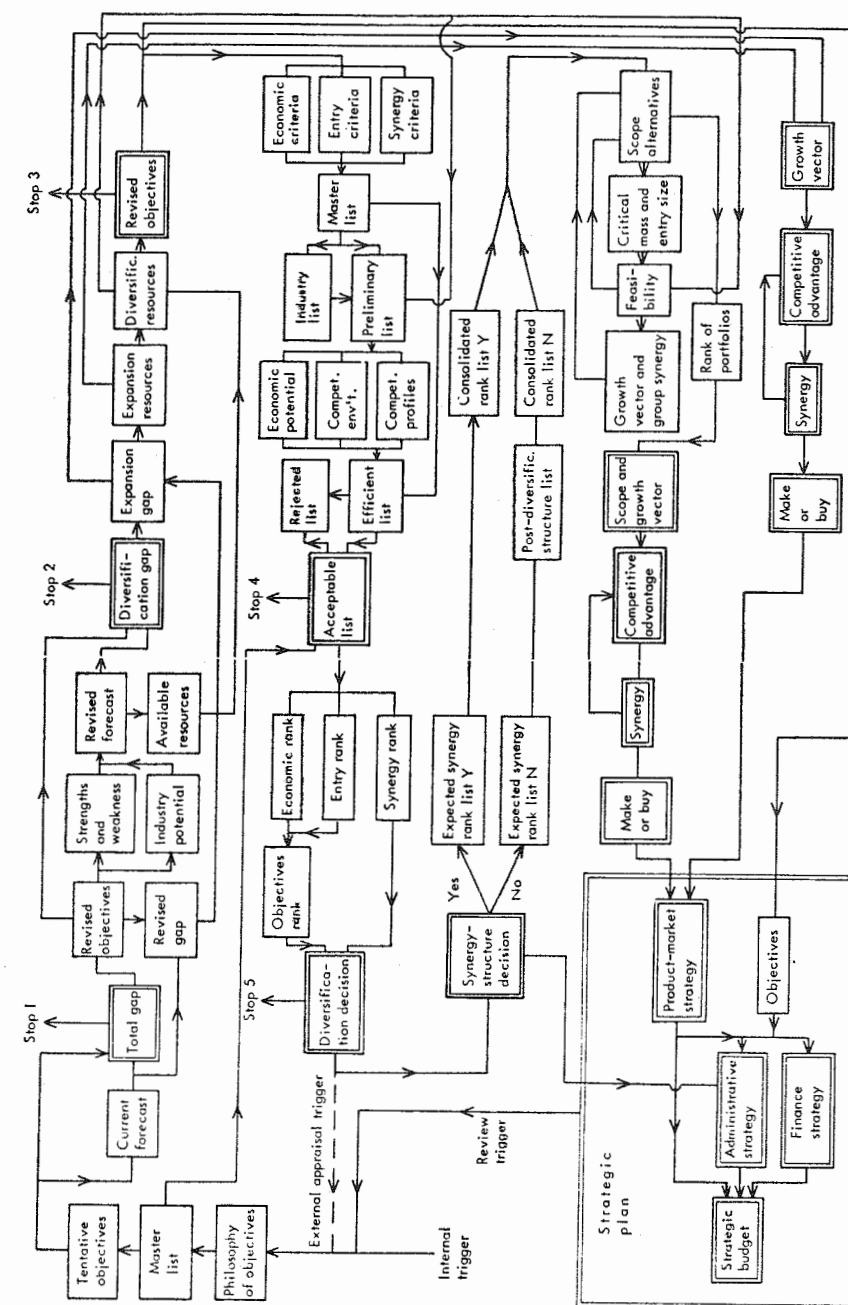
There are many of these models. They are also very similar. Many of them are much more complicated and contain fifty or a hundred different boxes prescribing the optimum planning process. Figure 6.3 shows an example of one of the more complicated models (from Ansoff, 1965). But the basic message is always the same: prescribing a certain series of steps one should go through in trying to find the optimum strategy.

I would like to mention a few of the instruments which have been developed to aid this planning process, and to discuss some of their psychological implications. With respect to *situation analysis*, one concept which has been quite popular is the concept of *key success factors*. The idea is that in every market there are a few factors, a few company characteristics which explain the bulk of the variance in business success (Carroll, 1982; Grunert & Ellegaard, 1993). When there are many such factors, the market will split, and become segmented according to which key success factors the participating businesses compete on. In trying to find out, as part of the situation analysis, how the business stands with respect to its competitors on a particular market, the key success factors can supply the dimensions on which to make such comparisons (Grunert, 1990).

How does one identify key success factors? Looking at the literature, you find statements that it is usually quite obvious what the key success factors are. For psychologists with their emphasis on subjective perception and intersubjective measurement this may sound very strange. In the limited discussion on how to determine key success factors empirically, the method most widely advocated is asking managers (e.g., Boynton & Zmud, 1984; Rockart, 1979). Given the well-known biases in attribution, it seems very doubtful whether a manager spending most of his/her day working in a company in that particular market will be able to attribute the major causes of business success correctly. A nice study by Stevensen (1976) is one of the very few investigating such possible biases. He asked managers at various levels and in various departments of the organization about what they thought the key success factors are. Not surprisingly, he found that the factors regarded as most important were those related to the department one worked in - marketing people find marketing factors most important etc. But there are also differences with respect to the levels in the organization. Top management usually believed that personnel factors are decisive for business success. The middle management believed that marketing factors are decisive, and the lower management that production factors are decisive. This study demonstrates clearly

Figure 6.3

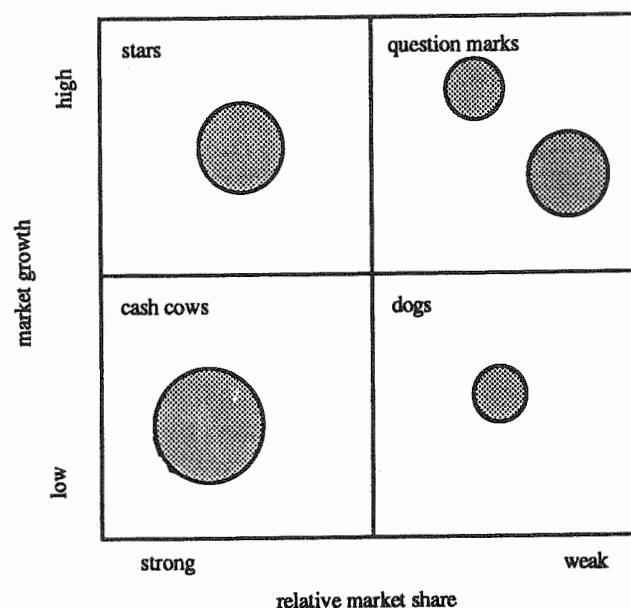
An Example of a Sophisticated Model of Strategic Management (from Ansoff, 1965)



the problems involved in just asking managers what the key success factors in a market are, expecting that they will be able to give a valid response.

Another example of a widely used and widely advocated planning instrument is the *portfolio matrix*, which can be used in connection with both situation analysis and strategy generation. The basic idea is that a company operating on several markets should classify these various businesses on two dimensions, which mirror the attractiveness of the market and the strength of the business's competitive position on that market. In the simplest version of this instrument, depicted in figure 6.4, market attractiveness is measured by market growth, and competitive strength by relative market share with respect to the main competitors. The various businesses of a company can then be plotted in a diagram like figure 6.4 as circles, the size of the circle corresponding to the sales volume of that business. Depending on which cell of the matrix the businesses are located in, they are called *stars*, *cash cows*, *questions marks*, and *dogs*.

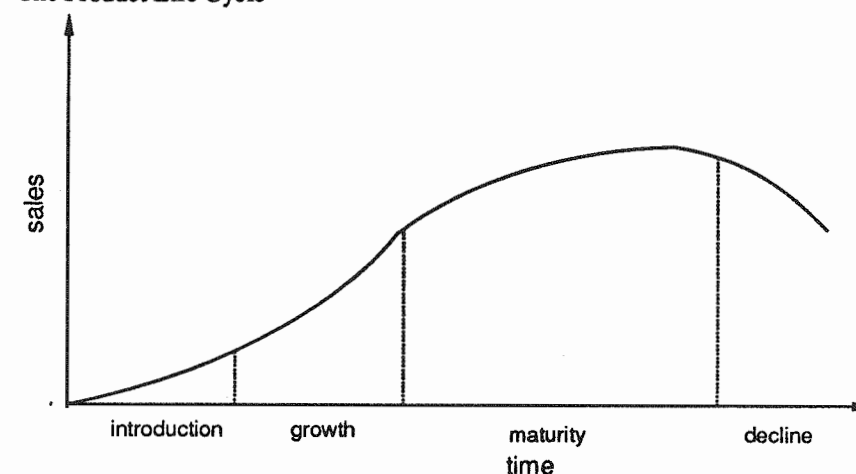
Figure 6.4
An Example of a Portfolio Matrix



It is then argued that the four cells of the matrix will differ in the cash flow generated by the businesses in that cell. A strong competitive position, mirrored by a high market share, will lead to more cash generation, but if the market is rapidly growing at the same time, this will also call for heavy investment, using up most of the cash generated, resulting in a zero net cash flow for *stars*. For similar reasons, *cash cows* will have a positive and *question marks* a negative cash flow,

while the cash flow for *dogs* may be zero or slightly positive. The normative implication is that the portfolio of businesses should be distributed such that there is an overall balance in the cash flow, i.e., there should be cash cows generating the cash necessary for building question marks into stars, which then later can become cash cows financing the building up of new businesses.

Figure 6.5
The Product Life Cycle



The reasoning is based on the assumption that a business cannot remain in any of the cells forever. Any product or market follows a certain *life cycle* (figure 6.5), so that any star at some time will become a cash cow and eventually a dog. The product life cycle concept, widely used and accepted, is actually a rather complex behavioral theory (cf. Wärneryd's chapter on *Economic Psychology and telecommunications research* in this volume). It is a complex behavioral theory, because there is of course no natural law saying that sales will develop over time in the way depicted by a life cycle curve. Many factors contribute to the shape of that curve. First of all, when a new product is introduced, the adoption behavior of customers will determine how fast sales rise. The middle phase is largely determined by competitive reactions: Other businesses are picking up the idea, offering similar products etc. Maturity and decline is connected to questions of technological and psychological obsolescence. Are there new products which are better, perhaps based on a new technology? Is the product no longer fashionable? The product life cycle is, hence, a rather complex behavioral model, where psychologists have a lot to contribute (cf. the early work by Reynaud in his *Economic Psychology*, translated 1981). Actually, product life cycles may look very different. A business can, under certain circumstances, change the shape of the product life cycle, by, for example, finding new uses for the product as soon as sales start

to decline. Doing this repeatedly (and successfully) in every maturity phase results in a form of the life cycle called a *scallop* (cf. Rink & Swan, 1979).

The portfolio matrix in figure 6.4 cannot be applied to scallops. It has also many other limitations, which cannot be addressed here, and there have been many improvements, which cannot be addressed either (cf. Robens, 1985). The point to be made here is that the argument rests on some very simple, mechanistic assumptions about the development of markets, which could profit from some input from behavioral scientists.

A portfolio analysis can generate strategies on how to distribute resources to various businesses - for example, whether to build, hold, or harvest. It says nothing about how that building, holding or harvesting can be achieved on any particular market. This aspect of strategy generation is dealt with under the heading of *generic strategies*.

The idea is that there are certain basic strategies any business can choose, and probably should choose - that businesses not designing their strategy on the basis of these generic strategies are not going to be successful. This point has been forcefully put forward by Porter (1980, 1985). He defines generic strategies along two dimensions. The first one is *scope*: either you have a broad scope, serving the whole market, or you concentrate on a small part of the market, i.e., have a narrow scope. The other dimension concerns the basis of competitive advantage and is already known from the Day and Wensley model: either one pursues lower costs

Figure 6.6
Generic Strategies

		competitive advantage	
		low costs	high quality
scope	broad	<i>cost leadership strategy</i>	<i>differentiation strategy</i>
	narrow	<i>focused strategy</i>	

than competitors, or higher quality. These two dimensions then lead to three generic strategies, as depicted in figure 6.6: the cost leadership strategy, the differentiation strategy, and the focused strategy. The normative implication is that concentrating on one of these strategies is the basis for success. One cannot have low costs and high quality at the same time, and businesses trying to do this anyway are bound to fail.

This taxonomy of generic strategies clearly mirrors the top-down perspective of strategic management. They are generated from the question on how the business should use its resources, not from the question what market offerings may be attractive to customers. Still, these strategies can be analyzed from a behavioral perspective. What does it actually mean to pursue a differentiation strategy? Quality, as every behavioral scientist would readily agree, is not a one-dimensional, but a complex multi-dimensional concept. Saying that one follows a differentiation or high quality strategy has no clear-cut implications, unless one knows how the customers in the market actually perceive quality. There may also be markets where consumers do not really care very much about quality. With respect to many low involvement products, consumers do not care very much about product differences. In this case a differentiation strategy would not make any sense. One cannot know without analyzing the customers and their needs and wants.

The cost leadership strategy makes the lack of concern with the customer side of the market, and with a behavioral analysis, still more obvious. Why should it be a good strategy to be a cost leader? Two reasons could be imagined. Lower costs, along with prices which are similar to the competitors', will lead to a higher margin, which may lead to higher profits. But why should the customers buy our product and not the competing ones? So imagine that the lower costs allow lower prices. If the customers in the market are price sensitive, if they perceive few differences between products, and if the offering of the business pursuing the cost leadership strategy fulfills the core quality criteria which the customers implicitly have in defining an "average" product, then the cost leadership strategy may be successful. Again, the probability of success of the strategy can be evaluated only on the grounds of a behavioral analysis of the customer side of the market - i.e., by supplementing the top-down view with a bottom-up view.

Strategic management needs psychological analysis, in the case of generic strategies the analysis of customer behavior. The main weakness of the planning school has been that, because of its normative thrust, hardly any empirical research has been conducted. Such research could be helpful not only in analyzing the effects of possible strategies, but also in investigating the appropriateness of the assumptions behind certain planning instruments (like the product life cycle in connection with portfolio analysis), and in general the effects the implementation of strategic planning procedures has in the company.

The shared experiences school

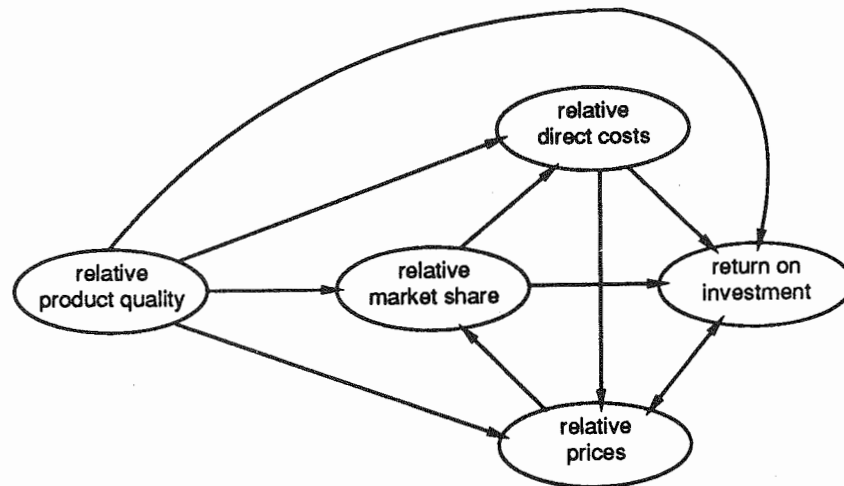
Actually, there has been research showing that it is perfectly possible to have high quality and low costs at the same time. Figure 6.7 (from Phillips, Chang & Buzzell, 1983) shows a causal diagram linking quality, costs, market share, and return on investment. Quality can have an impact on return on investment in two

ways: as a niche strategy, where it commands higher margins, but precludes a large market share because of higher costs and prices, or as an attribute strategy, where quality determines consumer preference and hence increases market share. One might think that an attribute strategy is not possible because higher quality leads to higher costs, leading to higher prices, which will reduce market share. However, quality may indeed lower costs - either directly, because tighter quality controls minimize waste and customer complaints, or indirectly, because the larger market share resulting from higher quality will lead to scale economies and learning effects which reduce costs.

Phillips et al. have estimated path coefficients for the causal relationships depicted in figure 6.7 for several sets of data. Their results show that in many cases higher quality indeed leads to lower costs, both directly and indirectly, bearing out that differentiation and cost leadership do not necessarily exclude each other.

Figure 6.7

Relationships between Quality, Costs, Market Share, and Return on Investment (adapted from Phillips, Chang & Buzzell, 1983)



This study is an example of the kind of research that has been done in what I called the *shared experiences school*. It is the school of thought in the area of strategic management whose view of the role of science comes closest to the view prevailing in psychology and most of the behavioral sciences. It is the view that the area of business strategies is amenable to research aimed at finding nomological statements. It believes it to be possible to find out how different strategy types are linked to business success under various conditions. I call this the *shared experiences school*, because it builds on the expectation that if experience of business

strategies is shared, it becomes possible to build up general empirically based theoretical knowledge which then can guide the selection of business strategy.

The data used in the study just mentioned comes from the PIMS data base, which is the largest and most well-known research effort within this school. PIMS stands for *profit impact of market strategies*, and is a large scale research project based in the United States, where several thousand businesses regularly submit information on their business results and key strategy variables, which then can be used for statistical analysis (see Buzzell & Gale, 1987). The study by Phillips et al. involved estimating a structural equation model and was technically rather sophisticated. Some of the earlier and widely publicized results from PIMS were based on much more simple analyses, and were also much more doubtful with respect to their validity and practical implications (e.g., Schoeffler, Buzzell & Heany, 1974). Thus, it was proposed, based on simple regression analysis, that return on investment increases with market share, increases with product quality, decreases with investment intensity, and decreases with marketing expenditure, especially when quality is low. None of these results are especially surprising, but especially the statement about the relation between market share and return on investment has stirred a lot of discussion, because the direction of causality is unclear, and different interpretations would lead to different strategic implications. More sophisticated analyses like the one by Phillips et al. can help to solve these questions about causality.

More sophisticated analytic techniques cannot, however, improve the quality of the data base, and this is another example of how the area of strategic management research could profit from some input from psychology. The PIMS data are collected by sending questionnaires to the participating businesses, which are to be filled out by a business executive. The major part of the variables do not involve data available in the company, but have to be intuitively estimated by the executive. Most of these estimates take two forms. The first one is where the manager is asked to give a *quantitative estimate*, for example when answering questions about relative product quality and relative costs. Thus, the *relative quality* estimate is measured using the following instruction:

Estimate the percentage of this business's sales volume in this year accounted for by products and services that from the perspective of the customer are assessed as "Superior", "Equivalent" and "Inferior" to those available from the three leading competitors (NOTE: The sum of these 3 percentages should be 100%). In assessing quality, the customer's perception of both the intrinsic characteristics of the product or service and any associated services (delivery time, warranties, application assistance, etc.) should be taken into account where these are important criteria to purchase.

The answers are then to be given as three percentages. Such a kind of measurement, as any psychologist will immediately agree, will be heavily influenced by informant biases and ignorance (Phillips, 1981): the executive may be ignorant

about the quality of competing products, about the criteria customers apply in judging quality etc. Also, these criteria may change or be situation-specific, making quality an ill-defined concept. There are thus numerous possible sources for both systematic and random measurement error.

The second kind of estimate which prevails in the PIMS database are *ordinal measures of underlying quantitative variables*, using single-item three- or five-point scales. Examples are the variable *relative forward vertical integration*, to be answered by *more, about the same or less* relative to competitors, and the variable *relative product image/company reputation*, to be answered by *much more, somewhat more, about the same, somewhat less or much less* relative to competitors. Such single item ordinal scales may be subject to considerable measurement error (cf. Venkatraman & Grant, 1986).

Thus, one of the major criticisms that one can direct at the PIMS measures is that their psychometric properties and the kinds of cognitive biases to be expected in data collection seem to have been of no concern when designing the PIMS questionnaire, mirroring again the widespread neglect of behavioral theory and methodology among strategy researchers.

A much more general criticism can be directed towards the main thrust of the shared experiences school. To which extent is it possible to find law-like generalizations of the effects of business strategies? How stable can we expect such relationships to be, and will such relationships not crumble once they have been published and decision-makers start to act on the basis of the findings? Competition is a dynamic process, and being first in acting in a certain way can give a company a first-mover advantage, while the advantages of acting in any particular way necessarily will disappear once everybody has adopted this same course of action. Here, as elsewhere in the social sciences, the fact that a phenomenon is investigated changes that phenomenon. Researchers in the shared experiences school assume, explicitly or implicitly, that the phenomena investigated are stable enough over a period of sufficient length in order to warrant investigation, but they should also acknowledge that their research, if successful, may change these phenomena, albeit slowly (cf. Grunert & Ellegaard, 1993, for a more detailed discussion of this problem in the context of key success factor theory).

Conclusion and perspective

Strategic management is a concept used and accepted in business. It is characterized by a top-down view of markets, competitor-centered, in contrast to the customer-centered bottom-up view of markets, which traditionally has drawn on behavioral theory. Academic analysis of strategic management has not (with very few exceptions) drawn on psychology and the behavioral sciences. As I hope I have been able to show, I regard this as an unfortunate state of affairs. A psycho-

logical approach could demystify the role of intuition in strategy formation. It could lead to better predictions about the effects of possible strategies. It could lead to a refinement of some of the very mechanistic concepts on which certain planning devices are based. Finally, and perhaps most importantly, it could lead to better measurement methods, which take into account the problems in obtaining reliable and valid data from a human informant.

I have argued in this chapter that strategic management, both as a business phenomenon and as a topic of research, has developed in response to changes in market conditions which started in the early seventies. A timely question would therefore be: what next? In which ways do markets develop, and how will business orientations develop accordingly? I will only name three factors and leave it to the reader to speculate about the implications. The *globalization of markets* is widely regarded as a major change in the business environment, resulting in both new threats and opportunities. *Flexible automatic manufacturing* does, at least in some areas, defy the classical trade-off between low costs and products tailored to individual customers, and may create new problems for manufacturer-customer interaction. Finally, Western managers are increasingly looking to the Far East. The success of especially the Japanese has triggered a number of studies on the business orientation prevailing in Japanese companies. The concept of *total quality management*, widely associated with Japanese management, may be an example of an increased integration of top-down and bottom-up approaches to conducting business.

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Chapter 7

Economic Psychology and Telecommunications Research

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Summary

Telecommunications are in a period of rapid change. There is a technological development leading to new services and there is deregulation aimed at increasing competition for earlier telecom monopolies. Economic psychology has a long tradition in communication research and has due to its interdisciplinary approach some unique advantages in research on telecommunications. Some examples of current research are given and certain problem areas are pointed out as being especially interesting for researchers in economic psychology.

Key words

Communication research, economic psychology, innovation diffusion, telecommunications

Why economic-psychological research on telecommunications?

The relative advantage of economic psychology

The area of telecommunications invites researchers from many fields. For a long time researchers in economic psychology have devoted interest to studying communication. Relying on communication theory, they have carried out studies of marketing communication with the ultimate goal of finding out the effects of ad-

vertising and other forms of marketing communication. There have also been studies of the effects of economic and other information on economic expectations and attitudes.

Economic psychology comprises the study of economic behavior. The research problems involve economic aspects of human behavior, and the research ultimately aims at describing and explaining economic behavior. Particular interest is devoted to choices with economic implications and factors influencing such choices.

In economic psychology, research focuses on subjective perceptions, expectations and other subjective phenomena. While the rationality postulate in economics implies that on average and over time objectively assessed and subjectively perceived phenomena tend to coincide, psychological research has shown that there may be considerable discrepancies. It follows that subjective perceptions of phenomena, if they can be measured, should give better explanations of behavior than objective descriptions alone. While the psychologist is rather optimistic about the feasibility of such measurement, the economist tends to be skeptical.

Applied to the telecommunications area, this means that economic-psychological research should aim at shedding light over how users perceive present services and innovations, their expectations, their values and attitudes affecting reactions to changes. Telecommunications answer the needs for communication that involve non-present people. The study of needs for communication and the perception of modes of fulfilling these needs in a social system is the subject matter of economic psychology and telecommunications.

Telecommunications research

When innovations are introduced, the emphasis often lies on what they replace, presumably in a superior way, rather than on possible new uses. It took some time before the potential uses of the telephone became clear; it was invented as a device to help blind people to communicate. With recent product development in the area, previously unexpected new uses have consistently been found. When the broadcast media were first introduced, nobody seems to have realized their potential for communicating about all sorts of events. The radio was seen as a means of speaking to large audiences at the same time, to facilitating public speaking, not as something that permitted immediate reporting of events and transmission of music and plays.

It is often asserted that the present and the near future mean a revolution in information due to the introduction and adoption of new information technology. The word revolution instills a feeling of sudden changes that are met with heavy resistance from some of the concerned parties; people protest against what they

hold to be further dehumanization and alienation. The swift adoption of the telefacsimile which will, perhaps some time in the future be interpreted with hindsight as a major revolution is hardly debated at all. Other innovations such as the picture phone have failed, not because of debate and active resistance, but rather owing to indifference and total lack of interest among the public.

I do not think there will be any real revolution, involving a whole country at one single point in time. Many of the changes will almost imperceptibly become part of our everyday life and we will accept them as pure routine. The role of communication innovations may easily escape the attention of researchers. Looking back, we may be able to see that there have been substantial changes, but the changes may be perceived as rather small, even trivial when we actually first face them. It is important to start research early, even before any change is introduced so that there are benchmarks with which future events and phenomena can be compared.

In my view, behavioral research related to telecommunications serves three purposes:

1. Providing data on uses, not only on what new information technology replaces, but rather on new and unforeseen uses of new communication media, as early as possible so that forecasts and product development can be based on realistic assumptions.
2. Providing data on users so that advantages of new media can be exploited and disadvantages overcome.
3. Training researchers so that there will be people around who are capable of structuring problems in a meaningful way for policy discussions. There will and certainly should always be policy discussions in connection with new media since they often involve large investments, not only for governments and business firms, but also for households.

Modern telecommunications

Developments in communications technology

Over the last few decades, information has become an increasingly important concept. The majority of the work force work with information in some sense. The information industries are larger than, for example, the automobile industry. There have been many forecasts and judgments of the likelihood of different scenarios concerning the changes that the so-called information society will involve. As to the importance of telecommunications, let me quote from a pamphlet published by the Commission of the European Communities:

"Telecommunications pose a major challenge for the completion of the single European market by 1992. Rapid technological change has turned this sector into a spearhead of market growth and of our future prosperity. Modernizing telecommunications increases productivity throughout the economy and makes a range of services more marketable; it requires enormous investment, both in infrastructure and in high value-added telecommunications services. Telecommunications, along with space technology, will receive the major share of European civil investment in new technology." (European File, 1988, p. 1)

The same source also predicts that telecommunications will increase from two percent to seven percent of the Gross National Product in the EC countries. Even if this is seen by many experts as an exaggeration, the text is right in stressing that telecommunications will be a powerful factor in economic and social development (cf. Antonelli, 1993).

The concept of technology assessment was launched a few decades ago (see e.g. Short, Williams and Christie, 1976; Pool, 1983). The idea behind it was that new technology should be evaluated in advance with regard to all of its effects, not only those that were promulgated by those introducing the innovation. What followed was mostly of a very speculative kind, with little foundation in research. Surveys reporting opinions on the usefulness of future innovations turned out to have little predictive value. A common flaw in such studies is that cost considerations are not included because of the difficulty of posing meaningful questions about hypothetical future prices.

Many innovations in communications technology have recently been introduced or will be introduced before long. The use of computers is an integral feature of many of the contexts in which telecommunications are used. Some innovations are simply developments in existing products, others have to do with improved capacity of networks, and still others are real innovations.

Here is a list of the most important ones:

- telefacsimile (the fax machine)
- PCfax
- electronic mail
- videotex (such as the French Minitel)
- high definition TV
- videophone (picturephone)
- satellites for telecommunication
- optic fibres (new, high-capacity channels for fast communication).
- new telephone calling services.

Examples of new telephone calling services are:

- call waiting information
- forwarding of calls
- three-way calling
- memory for short forms of telephone numbers.

Surprisingly little is known about the current uses of modern communications technology. The telephone has been with us for over 100 years, but very few studies have tried to find out, what the telephone is actually used for and what its use means to humans in social systems (cf. Pool, 1977 and 1983). Many studies have been made of the introduction of computers into office environments and routines, but the highlight has been primarily acceptance and resistance, while little is known about the actual uses of personal computers in offices and homes. More research should focus on what people do with telecommunications and what those mean to them in their different capacities as social beings.

The deregulation of the telecommunications industry

Traditionally, most countries have had de facto telecommunications monopolies, often a combination of Post, Telephones, and Telegraph (PTT). In recent years, attempts have been made to break up these monopolies and promote competition (see e.g. Mansell, 1993). Some skepticism has been voiced regarding what will actually happen to competition.

"In spite of the formidable forces that reinvigorate the EC, the considerable parochial, bureaucratic, and nationalistic obstacles to integration must not be underestimated. In the case of telecommunications, the very institutions that give lip-service to '1992' will strongly resist deregulation if their own narrow interests are at stake." (Thimm, 1989, p. 56).

If you believe that the new developments will be influenced by human conflict, you have definitely arrived in the area of psychology! Where subjective perceptions, feelings, and attitudes are involved, psychology has a job to do. Psychological research may not solve the conflicts, but it can clarify the issues and provide basic data for assessing the difficulties.

To understand what is happening, it is necessary to distinguish between the different hierarchical components of telecommunications. At the bottom, there are the suppliers of equipment. These are mostly private companies which face competition for the attractive contracts with telecom administrations. The latter handle the equipment and the networks (carriers) that transmit the communications. Finally, there are the services that are performed with the equipment and the networks - the uses and users of telecommunications. The networks are being used for an increasing number of services. The fax machine is, for example, an important innovation and owing to new switchboards the telephone can perform a number of new functions. These are often referred to as value-added-network services (VAN services).

Deregulation involves privatization of telecom administrations and the promotion of competition. British Telecom has, for example, been put onto the stock market and sold to the public. Competition has been invited through the estab-

lishment of the competitor Mercury. There is now a discussion about the desirability of adding another competitor. British telephone users do not seem convinced that there have been any significant changes from the consumer point of view. Earlier on it was thought that the carriers, i.e. the telephone cables, could be taken care of by a monopoly, but there are now in many countries developments of privately owned networks, especially in connection with mobile telephones. In some countries like UK and Sweden state-owned railways which also face deregulation have opened up for new telecommunication carriers using the railway lines. In the USA and Japan telephone users can choose between several telecom companies using different carriers (see Pospischil, 1993).

It should be pointed out that deregulation does not mean total absence of regulation, but rather that attempts are made to loosen up earlier monopolies and regulate in accordance with these new strivings. Economists who are working on the problems of deregulation often say that there is no good economic theory for the effects of deregulation. One of the difficulties is that user reactions are hard to foresee.

The role of research

Examples of problems for research

At first, the questions raised by the new developments in communications were almost exclusively technical. The heavy investment costs aroused a demand for economic research which is now being followed by an increased demand for research carried out by other social and behavioral sciences. Human beings and social systems are involved in the changes. Technicians have often been surprised at the lack of enthusiasm that the ultimate users of innovations display. Increased effectiveness and efficiency are not in themselves enough for the acceptance of innovations.

"Hitherto, the fact that face-to-face contact has almost always been the most satisfactory form of communication has been a fundamental constraint on society." (Short et al., 1976, Preface p.V)

Forecasts of the rate of acceptance have notoriously underestimated the time an innovation will take before it is accepted to any considerable extent. In this context there is an important role to play for economic psychology.

Let me mention three problem areas:

- * acceptance of and demand for new products and product services
- * reactions to changes in prices and tariffs
- * effects on social structures.

As noted above, forecasts of future demand for innovations in communications have tended to be overoptimistic. In the early 1960's computers were expected to revolutionize society before the end of the decade. The picturephone has been launched on the market and failed several times. Every time there have been forecasts of rapid diffusion. The videotex has not had much success in comparison to the high expectations, except in France where the success was produced through enormous investment. Television, on the other hand, was quickly accepted and diffused in many countries. In Israel it took two years to reach 50 percent coverage of the households. Color television had a slow rate of adoption which is often explained, at least to some extent rightly, as depending on the lack of social influence. Studies show that the adoption of black-and-white television was heavily influenced by social contacts (Robertson, 1971).

Economic forecasters have had difficulties with the variables they usually employ and have looked for additional variables such as social influence. They then face the difficulty of finding measures or good proxies for these variables. While economic forecasts of demand for black-and-white television for a long time grossly underestimated the demand, forecasts for color television consistently overestimated the demand and the rate of adoption. (Olshavsky, 1980)

Behind some of the interest in telecommunications hides the question whether energy resources and also human resources can be saved through replacing face-to-face contacts with improved telecommunications services. What does it take to incite organizations to move messages rather than people? There are no technical hindrances and with respect to costs often vast superiority. Obviously economic explanations alone do not suffice. It is necessary to look into social and psychological factors. The telephone now offers a number of new calling services and more are coming. To what extent are people really interested in using these services and how much are they willing to pay for them in the long run? Does it take time to learn the use of the new devices?

Telecom tariffs have traditionally been influenced by political considerations. The PTT monopolies were given social responsibilities so that individual tariffs and prices have not been cost-based. Typically, business firms and those consumers who make long-distance calls have had to pay more in order to keep down the costs of the local calls of households. The price sensitivity of the consumers and of business firms is not well known. If prices are more cost-based, there may be unwanted social effects.

Important questions that are hard to tackle concern what effects the widespread use of communications technology will have on society, both at the national and the local level. If the innovations are not used, there will hardly be any effect that can be measured. How much does it cost not to utilize an innovation? Many issues connected with the prospective uses concern such dimensions as centralization versus decentralization. Increases in both directions seem possible and it is too early to make any general estimate of how opposing tendencies will work out.

Studies will have to be carried out to decide under what circumstances centralization and decentralization will be favored by the availability and use of telecommunications.

Modern telecommunications make it feasible to move work from ordinary offices to homes or to specially established work centers. Certain tasks can be distributed to other work places, carried out and the results used by means of telecommunications in combination with computers. Not only simple secretarial assignments and certain types of supervision, but also managerial tasks can be carried out. I know for example one CEO (Chief Executive Officer) who every Sunday night gives information and instructions to his managers by PC and telecommunication.

The possibilities of breaking up work places and moving certain functions to other locations have been much debated already. Some attempts to let people work in their homes have failed. Apparently there is often the difficulty that people want to meet their fellow workers personally rather than through media. This has come as no surprise to those who know about the psychology of work motivation. Social contact is one of the important factors that make people want to work (for a review of reasons for working, see e.g. Lea, Tarpy, and Webley, 1987, ch. 6).

Markets - in the geographical sense of places where people meet to make business deals - may exist as electronic marketplaces. There are already such markets in oil and in finance. Conceivably, the stock exchanges, which, by the way, already use computers extensively could be replaced with electronic marketplaces - if it were not for human reasons.

Research traditions in the study of communications

There are three major research traditions in the study of communications:

1. Communication and attitude change theory
2. Media usage and effects studies
3. Diffusion and adoption of innovation.

All of these three traditions are of use in telecommunications research today, but they are not sufficient (cf. Rowland, 1993). The problems make it necessary to combine approaches and to use economic thinking along with psychological methods and theories.

During World War II laboratory experiments in the tradition of learning theory research were started to find out what factors influenced the effects of propaganda and persuasion (Hovland, Janis, and Kelley, 1953). Many factors relating to the medium used, the source of the message, and the message format were studied. The results were widely discussed and stimulated an enormous interest among researchers and practitioners in many fields, including advertising. The typical pro-

cedure was to measure attitudes towards an issue before an experimental influence in the shape of a message in a medium was imposed and then to measure again some time after the exposure.

Attitude change theory was developed through experimental research and theoretical discussion for many years. The work of Ajzen and Fishbein (1980) is particularly well known since in their theory of reasoned action they have incorporated ideas of how attitudes and social influence combine to form intentions. With the development of cognitive theory, the interest in theoretical attitude studies has decreased. It has been replaced by a focus on information processing, a study that also gives some clues for telecommunications research.

Media usage studies include many things from simple readership studies that are mostly carried out for advertising or editorial purposes to sophisticated analyses of what uses people actually make of the media. Comparatively few studies have been carried out concerning the uses of telecommunications. Studies have comprised comparisons between personal contacts face-to-face and via auditory and visual media such as a telephone or a videophone (Short et al., 1976). Most of those have been laboratory experiments, in some cases combined with survey research. The work by Short et al. (1976) is, however, based on a well-designed, forward-looking research program in which several approaches were combined. Recently there has been a notable interest in finding out more about individual and household uses of the telephone (see e.g. Noble, 1991).

The study of the diffusion of innovation has often been associated with the name of Everett Rogers (1983), who in the early 1960's summarized a number of studies and launched a diffusion of innovation theory. He asserts that the adoption of innovations tends to follow a certain pattern. The individual adopter passes through some characteristic stages: from awareness (first knowledge) of the existence of the innovation over an information/persuasion stage to trial and possible adoption. Studies show that while the mass media spread the information and thus influence the awareness stage, it usually takes personal influence to reach a decision to try out the innovation. The diffusion of innovation occurs in a social system and the properties of the system have to be taken into account.

This research has yielded a number of hypotheses about the characteristics of early adopters as compared to later adopters. Quite often those few who are the first to adopt are not representative of the rest of the population. They are more venturesome, less well integrated in social groups, and have a high degree of innovativeness independent of context. Innovations also have characteristics that will decide their destiny. They differ in the advantages they give in relation to earlier alternatives (profitability), compatibility with social norms, how easy to understand and to use they are, and to what extent they communicate the fact that they have been adopted. High values in these dimensions increase the probability of adoption and the rate of diffusion.

Some examples of current research problems and methods used

Choice models for new services

An individual may choose to accept or not to accept an innovation. While this is obviously true about innovations that can be individually adopted, like telephone calling services, to some extent it also holds true for innovations that are introduced as part of a whole system, for example, changes in office routines. Since launching an innovation is expensive, the launcher is anxious to base expectations of demand on as much data on individual preferences as possible. When something really new is introduced, there are no preferences for the innovation. A rule of thumb is usually to take preferences for something that is similar and already exists on the market and study what they are like and what factors influence them. If something is really novel, almost by definition nothing similar exists.

The problem of assessing preferences was neatly stated by the economist Jevons (1871) who referred to the psychologist Alexander Bain when he formulated something akin to the theory of revealed preferences: "As Mr. Bain says, it is only an identical proposition to affirm that the greatest of two pleasures, or what appears such, sways the resulting action; for it is this resulting action that alone determines which is the greater." (Jevons, 1871/1911, p. 12)

If the view is adopted that preferences can only be studied through observation of behavior, preferences for innovations can only be studied in actual choice situations, and preferably in the market place. To be able to make a forecast of demand, it is then necessary to arrange choice situations in which actual choice behavior can be studied. Since prices of innovations like new calling services are important for acceptance or non-acceptance, the choices must admit studies of price sensitivity, or price elasticity as the economists define it.

While economists tend to restrict the study of preferences to preferences as revealed through market behavior, psychologists are usually convinced that people can state their preferences. This conviction is often combined with the idea that people can base their statements of preferences on information. Thus people are fed with information about a new service or product and then asked to state their choices. From market research there is now a lot of evidence that the predictive value of such preference statements is typically very low (see e.g. Ben-Akiva and Lerman, 1985).

The goal of this type of research is practical: to estimate a demand function that makes it possible to make good predictions of the actual demand. The new thinking involves (Ben-Akiva, 1990):

- * specified models
- * collection of data through combinations of methods

* estimation of parameters in the model.

Even apart from the fact that the research has a rather practical goal, this thinking is new to many psychologists in the sense that the problems are translated into specified models and that combinations of data collection methods are used. Data are obtained from studies of past choices, if possible for innovations that are similar to the present one, from running experiments with real choice situations, and the use of scenarios to get statements of preferences.

The adoption of innovation

The Concept of Critical Mass. For many innovations it is assumed that the acceptance curve is S-shaped. This means that the rate of acceptance is at first very slow and then, after some point, it changes and becomes much more rapid. Ultimately a ceiling is reached and the further acceptance rate declines to zero. In diffusion theory, the slope of this curve is assumed to depend on the social system in which the innovation is introduced. If there are social contacts that work in favor of acceptance, the slope will be steeper and the process more rapid than when the social system does not influence or counteracts acceptance.

In principle, two main types of communication technologies can be distinguished. With one type the adopter does not interact directly with other partners. Adopting text processing does not require the other party to have the same equipment. In the second type the receiver must have compatible equipment. In many cases, new communication technology actually involves such interaction with other parties. When the communication technology serves interactive purposes, it is important for the adoption whether a network of holders of the equipment exists or has to be created. If there is an existing network the situation is similar to the case where there is no interaction with another party. An example of the latter is when new functions such as call forwarding are added to the functions of the telephone.

If there is no network, but a need for one, the concept of critical mass comes into focus. It is used to designate the number of adopters that is essential for the sustained growth of the acceptance. In its original sense "critical mass" is defined as the minimum quantity of radioactive material which will enable a chain reaction to proceed by means of nuclear fission (Bullock and Stallybrass, 1977). In a discussion of interactive communication technology, Rogers (1990, p. 6) says: "In the context of the diffusion of an interactive technology, the 'critical mass' is defined as the point in time at which enough individuals have adopted an interactive innovation that the perceived cost-benefit of adopting the innovation changes from negative to positive for the individuals in the system." This can also be formulated as a question as to how much of a network is necessary for making poten-

tial adopters interested to join. It should be admitted that the concept is vague and hard to make operational.

Even when an innovation does not entail interaction, but communicates its existence by being visible or is the object of person-to-person communication, there is obviously a critical mass in the sense that at first very few people own the innovation and display it or talk about it so there is little influence on other potential adopters. When a certain number of people have adopted the innovation, there is a higher probability that those who have not yet adopted will be confronted with it or exposed to communications about it.

There have been attempts to describe the development using mathematical functions from studies of the spread of contagious diseases. The typical curve is positively accelerated at first which means increasing growth rate up to the point where fifty percent of the population in question has adopted the innovation. After this point the curve is negatively accelerated, that is, with decreasing rate of growth. When the adoption of an innovation is studied after the point in time when there is no new acceptance, the critical mass is often said to be at the time when 16 percent of those who ever adopt have adopted (Rogers, 1990). This roughly corresponds to the point where the rate of growth makes the greatest change upwards.

For some innovations that presuppose interaction for their use, it is necessary that a minimum number of adopters exists. Otherwise new adopters cannot enjoy their adoption of the innovation. The number that is necessary varies. Some people may want to have a videophone of their own even if there is only one other person with whom they can communicate and make actual use of the video. Others may want an almost complete, open network before they become interested. In such cases, the individual threshold (critical mass is an aggregated concept) may vary from two upwards. The question is whether it is possible to investigate in advance how many potential users there are in each size group. The question of course amounts to whether people, before there is any actual chance of trying the innovation, can be meaningfully asked under what conditions they will adopt the innovation.

It follows from this reasoning that it may be inappropriate to start with the needs of a single unit when the fate of an interactive service is discussed. It seems advisable also to check with the other intended party. This means using a dyad as a unit or whatever minimum group size is necessary for the service to function and be appreciated. Theoretically it is, for example, possible to investigate how many people are interested in acquiring the videophone for dyadic communication with one single party, how many see three as the minimum number, and so forth, and use this as a basis for a forecast of demand (or for developing facilities such as special telephone booths for such dyadic or triadic communication by videophone to occur).

With respect to the videophone, there is evidence from many unsuccessful launchings that there is little demand for the innovation - no really useful functions have to my knowledge been reported. There may still be dyadic communications for which the picture could serve appreciated functions. Anecdotal reports on the French large-scale experiment in Biarritz indicate that there was more interest in the households to communicate by picturephone with near relatives living at a distance (with whom they could not communicate by video) than with the local food store keeper (with whom they could communicate by video).

A study that sheds some empirical light over the issue of critical mass without actually being aimed at finding the critical mass is briefly described below. The first study which was carried out in 1978 suggested that there were two types of early adopters of the fax machine. One group, the majority, needed the fax for dyadic communication. The second group, the members of which had a generally high adoption rate for innovations, found little use for the fax machine since there was no network. When the second study was carried out in 1988 there had been an explosion in the use of the fax and the reasons for adoption had changed.

The Adoption of the Fax Machine in Sweden. Two studies of the adoption of the fax machine in Sweden will be briefly described here as examples of economic-psychological studies in the telecommunications area. In Sweden, like in many other countries the telefacsimile which was in fact invented already in 1843 was extremely slow to be adopted. Then all of a sudden the development exploded in the middle 1980's (see Figure 7.1). Apparently the fax machine reached a kind of critical mass at that time. Two studies carried out by my research group at the Stockholm School of Economics help us to understand what happened.

The use of the critical mass concept is often tied to the idea of (social) networks (Rogers, 1990). There is an emphasis on how individuals adopt an innovation. In the case of the fax machine individuals make decisions in the framework of organizations like business firms. Adoption by households is still uncommon. Such adoption occurs mostly at a late stage in the development process.

In business and industry, three types of adoption patterns for innovation in information technology can be distinguished (Wärneryd and Holmlöv, 1992):

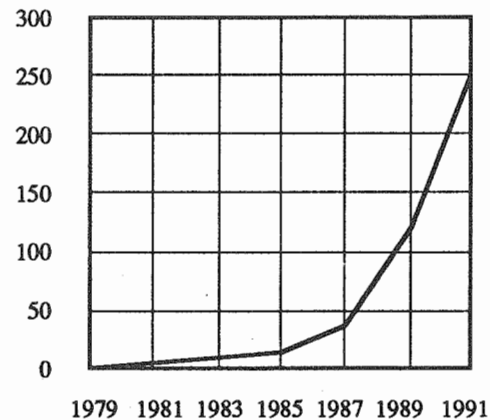
1. The adoption of a single unit. Purchasing a PC for use by a single person or a small group of persons may not change much in the administrative structure of a business firm or a government agency.
2. Introducing a set of information innovations that must be accompanied by far-reaching work routine and perhaps also staff changes. Computerization of departments and whole companies is a case in point
3. A change in information carriers makes it possible and even mandatory to change the use of information media. The increasing availability of ISDN (Integrated Services Digital Network) is an example. The digital networks provide new broadband services with extremely rapid transfer that were earlier impossible

or very expensive. This involves modernization that may occur piecemeal or in a more global fashion.

Figure 7.1

The Growth of Fax Machine Use in Sweden 1979-1991

ESTIMATED NUMBER OF FAX MACHINES
IN SWEDEN 1979-1991 (in 1000's)



The decision processes can be assumed to vary in the three cases (Zaltman and Wallendorf, 1983). The acquisition of a single unit of something that is not too expensive may be decided by the intended user or the next superior whereas acquisitions of new information systems may involve large parts of the organization and the final decision be made by the board of directors (see e.g. Pettigrew, 1990).

The telefacsimile mainly belongs in the first group. A single unit can be purchased, it is easily installed and no dramatic changes in work routines are necessary. In some cases, though, the fax is adopted as part of a more comprehensive introduction of information technology in attempts to make most information handling electronic.

The first of our studies was done in 1978 when there were in all 400-500 fax machines in the country. It is reported in Jundin and Lindqvist (1978). The second study collected its data in November, 1988, when there were approximately 130 000 fax machines in use (for a fuller report, see Wärneryd and Holmlöv, 1992). Since then the number of fax machines has continued to increase rapidly and in September, 1991 was more than double that number (Figure 7.1).

Around 100 users of the fax machine were interviewed by telephone in 1978. They were asked questions about the adoption and use of the fax machine. There were also some questions about reasons for acquiring it and the satisfaction with

the outcome. The results indicate that there were two groups of users. One group which comprised the majority, consisted of very satisfied users (despite some inconveniences such as noisy, smelly machines) who communicated frequently with one or a few other parties. Typical examples were fax communications between a laboratory that made tests and a hospital, or an advertising agency and a publisher.

The second group comprised those who had bought the fax machine because they wanted to use it to communicate with different parties within a network. Since fax machines were uncommon both nationally and internationally at the time, they were dissatisfied with the fax and the machine was little used, if at all.

The study indicated that the main reason for adoption was the need for frequent communication with one or in some cases a few parties. There was no critical mass in the sense that the adoption would depend on other organizations adopting. The essential thing was that there was a pressing need for a certain type of dyadic communication. The need involved speedy transfer, often complex material like designs or pictures. The telefacsimile replaced something that was clearly inferior in terms of speed and costs.

Looking for a critical mass does not seem meaningful since the adoption was apparently not influenced by the number of adopters. The market share of the largest manufacturer was very high which means that most fax machines were compatible and there was a directory of holders containing somewhat over 300 fax numbers, but this did not provide enough of a network. The number of adopters may have had some influence on the dissemination of information: potential adopters, with specific needs for the appropriate type of communication, are more likely to find out about the potentiality if more people are regular users and talk about their use of the innovation.

Studying the first-mentioned group of adopters gives information about the motives behind some adoptions. Further research along these lines could have revealed how many prospective adopters there were in this category which comprised those who primarily communicated with a few fixed parties. A first estimate of the growth rate of adoption could have been based on the willingness of potential adopters with similar needs to adopt.

From the point of view of future adoption and critical mass the second group may have been the most interesting to do follow-up studies on. They may be seen as "lead users" (von Hippel, 1988), with pressing communication needs of a different type. Early users with high needs for the product are according to von Hippel (1988) especially interesting to study. They foretell future needs and uses and can thus give important clues to the development of demand for the product. Their expectations could be studied more in detail. The market potential would then be estimated on the basis of research focusing on the expectations of other potential participants in a network to find out how many potential communicants they would want before adopting the fax.

The fact that more and more organizations adopted the fax for specific communication purposes, say, dyadic communication, certainly led to the discovery in some cases that the fax machine could also be used for communication with other parties. The advertising agency perhaps found, for example, that it had become possible to communicate with other publishers or a bank. Adding to this the fact that some organizations adopted the fax out of innovativeness, enough adopters ultimately emerged so that a critical mass for a functioning communication network emerged around 1984-1985. Then came the rapid expansion that still continues. At the time of the first study, we were unfortunately not able to reason in these terms so what has just been said is a somewhat speculative hindsight.

Against this background a new Swedish study of fax adopters was carried out in 1988. The sample consisted of 371 fax numbers drawn from a directory. Questionnaires addressed with a person's name were sent by fax and completed questionnaires were received from 76 percent of the sample. A comparison of the two studies is made in Holmlöv and Wärneryd (1990). Only a few results can be given here.

In 1978 55 percent of the faxes were leased compared to only 20 percent in 1988. This reflects the fact that fax machines were rather expensive in 1978 which of course slowed down the adoption rate. Table 7.1 shows another interesting difference. As noted above, in 1978 fax machines were mostly acquired for serving a specific purpose. In 1988 this was still an important reason, but there was also a new reason which did not exist at the time of the earlier study: external pressure. The fact that many organizations had fax machines and wanted to communicate via this medium exerted a pressure on non-adopters to adopt. It should also be noted that such pressure tended to increase the number of fax machines within an organization.

Table 7.1
Main Reason for Adopting a Fax Machine (Percent)

	1978	1988
Specific Purpose	69	44
General Purpose	31	39
External Pressure	-	17
N	99	254

In the 1988 study, there was an indication that those who had adopted the fax earlier were more innovative. An index of innovativeness in the 1988 study was based on the number of information technology devices such as PCs, mobile tele-

phones, electronic mail, videotex etc. that they had adopted. Those firms that adopted earlier were more innovative in the sense that they had acquired more innovations in information technology.

Based on adoption theory, the study tested some other differences between earlier and later adopters. It turns out that a high need for external communication, a specific need for communication with a few parties and for fast communication with complex messages characterized the earlier adopters. Attitudes towards the fax were highly positive in general and did not distinguish between the early and late adopters. The positive attitude towards the fax was true even among some respondents who were unfavorable towards increased use of modern technology.

The slow beginning of fax adoption may to some extent have been a consequence of the characteristics of the first variants of the product. They had some definite disadvantages: they were slow (one page required a transfer time of three minutes in 1978), noisy, foul-smelling, and rather expensive. With the third generation of fax machines most of these inconveniences were gone and the fax could replace some other means of communication in a superior way. At first the acquisitions were primarily made for specific connections and later, when a critical mass of machines was reached, the development exploded.

In the longer run, what is most interesting about the fax are the new, not foreseen uses. The studies briefly summarized here were only able to deal with this aspect to a very limited extent. The use of the fax for social communication which, judging from everyday observation, is rapidly increasing is, for example, not covered.

One implication of these two studies is that it is important to look at adopter characteristics and to scrutinize their reasons for adopting an innovation. Using the critical mass thinking in a superficial way may give wrong conjectures about future developments. If there is no network, the case for interactive communication technologies is different from when there is a network. Extended telephone functions like three-way calling do not require the emergence of a new network. There is no critical mass in the sense of number of members of a network. It is rather a question of dissemination of knowledge and the attitudes towards the service and its price.

The study of price sensitivity

"The psychology of price" is a label that includes many interesting problems. Many psychological researchers would probably find the area fruitful for research if they knew more about it. In the context of telecommunications, user reactions to different tariffs and prices of services are very important. The experience of telecom administrations indicates, for example, that people may prefer a higher rate to

a lower rate under certain conditions. This clashes with the economic rationality postulate (Ben-Akiva, 1990).

The pricing problem can be illustrated by the choice between flat rates and bills based on measured services. In the first case, the user pays one price and has the freedom to use the service as much as s/he likes. In the second case, every single use of the service is paid and the number and length of uses decide the fee. There are findings (see Ben-Akiva, 1990) indicating that low-usage users opt for measured services since they feel that they pay too much if they choose the flat rate. They tend to decrease their use of the services. While this agrees with economic rationality, some low-usage users prefer the flat rate because then they know how much they will have to pay. There is no uncertainty about the size of the bill. In a way, it is like buying an insurance policy.

Choice of a flat rate leads to increased use of the service. This is hardly surprising, but if Thaler's (1980) interpretation of prospect theory is predictive, an increase in the flat rate could lead to increased use. The idea is that the increased cost which is seen as a loss, is felt to give more "pain". To restore the balance, the user must increase her/his use more than the increase in loss. This may more than compensate for the decreased use of some users who go over to measured services and reduce their consumption of the service.

Price sensitivity can be studied by measuring the demand before and after a tariff change. The number and length of telephone calls can be expected to change. Interviews with users can contain questions about consumer knowledge of the price change, usage patterns and so on. It seems desirable to include also questions regarding price sensitivity or/and to make controlled experiments.

The future role of economic-psychological research on telecommunications

The area of telecommunications offers many research opportunities, partly because it is as yet no one's specialty. No social or behavioral science is as yet established as the main contributor of interesting research results, even though economics may be a little more dominant than the others. Problems and issues are and are seen as interdisciplinary, requiring attacks that can combine thinking from different disciplines.

Psychological research in general and economic-psychological research in particular have a lot to contribute to telecommunications research. The characteristics of actual and potential users have to be considered and studied from other vantage points than that of pure technology. Research on acceptance of innovations and on the learning of new user habits belong to the field of psychology.

There are some further implications of the above presentation of research issues. Research in economic psychology can and should be both basic and applied. There

is definitely a great need for theory development linking ideas from psychology and economics. Psychological research on phenomena that are of interest in economic contexts, has often been accused of neglecting cost aspects and of focusing exclusively on attitudes (preferences), but not on prices. Economic psychology can have an advantage here.

To gain more acceptance in the long run, it is essential for this research to prove that it has something to contribute to the solution of problems in modern society. Combining economic thinking with psychological theories and methods, economic psychology has an important role to play in this context.

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Chapter 8

Distributive Justice

A Behavioral Theory and Empirical Evidence

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Summary

We speak of a distribution conflict if at least two individuals have to share a burden or reward or both. Based on equity theory we try to outline a behavioral theory which predicts the allocation results for such situations. Whereas our conditions specify when equity considerations will be applied, the hypotheses predict the way in which individual contributions and rewards are measured in case of competing standards. As empirical evidence we provide experimental observations as well as supporting real life situations. It is also discussed how justice considerations can be incorporated into a general theory of human decision making.

Key words

Equity theory, distribution conflicts, experimental economics, ultimatum bargaining, reward allocation, distributive justice, fairness.

Introduction

Distribution conflicts result if at least two individuals have to decide how to share a burden or a reward or both. We refer to such situations as conflicts regardless whether the participating parties agree or disagree on how to share the resources, i.e. "conflict" refers to competing ways of using resources. Behavioral theories try to explain how human decision makers behave in such situations.

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Starting point of our theoretical considerations is equity theory which views distribution conflicts as input-output-relationships, i.e. if there is a reward to be shared, it is seen as the result of the contributions by the participants. For such situations equity theory predicts that all individuals are rewarded proportionally to their contributions. We try to develop equity theory further by specifying conditions which tell us when and how equity theory is applied. The main question here is which variables are used to measure individual contributions or rewards. Our conditions essentially restrict the use of equity theory. They, unfortunately, do not always define a unique way of applying equity theory whenever it is applicable.

Situations with competing ways to measure individual contributions and rewards is the next topic of our theoretical considerations. The main idea is that such competing standards can often be hierarchically ordered in the sense that a superior way of measurement requires more information than the inferior one. We specify three hypotheses predicting which of two such competing standards will be applied.

Thereafter we try to support our hypotheses by empirical evidence. We first review observations from experimental studies of cake division and then discuss some real life situations where equity considerations are quite powerful in explaining the typical behavior.

Finally it is analyzed how the desire to comply with basic norms of distributive justice can be incorporated into a more general behavioral theory of human decision making. Clearly, distributive justice is not the only goal which a decision maker wants to achieve. In his dynamic decision process he will first try to qualify whether in a certain situation justice considerations should be applied or whether one can behave selfishly. Such a decision seems to be supported by clustered data near to allocations implied by basic norms of distributive justice or by selfish strategic considerations. We, however, do not deny that in a later phase of the dynamic decision process one can be confronted with intrapersonal decision conflicts due to competing goals. Compromise solutions of such intrapersonal decision conflicts might account for behavior which appears partly guided by justice norms and partly by selfish considerations.

The behavioral theory of distributive justice

Distribution conflicts are situations with at least two participants who have to share a burden or a reward or both. Such situations can be analyzed by asking different research questions: Here we do not answer, for instance, the welfare theoretic problem, which allocation results are most desirable from an ethical point of view, or the game theoretic question how rational decision makers should behave. What we try to study instead is how human individuals behave in such situations,

i.e. we try to describe a behavioral theory of distributive justice. Since we only try to make a further step in the process of developing such a theory, we do not require that it is always applicable, nor that it yields unique predictions when it is applicable.

Since our concept of distributive justice is based on equity theory, which will be described in the following, it is based on the convincing ethical principle of equal treatment. An open question is, however, to which variables equal treatment should apply. We will specify five conditions which a variable has to satisfy in order to qualify as a standard for measuring individual contributions or rewards. Thereafter we discuss some hypotheses predicting the standard which is used when there exist competing standards which can be hierarchically ordered according to their prerequisites, i.e. to their information and measurability requirements.

Equity theory

In traditional economic theory distribution conflicts were often considered as exceptional situations where standard economic principles cannot be applied. It was often argued that the resolution of such distribution conflicts will depend on individual bargaining power and on the personal characteristics of the interacting parties without specifying how individual bargaining power can be defined and what the relevant personal characteristics are. Fortunately, (social)psychologists were ingenious enough to also view distribution conflicts as social production phenomena where unlike in firms with private ownership one cannot distinguish between employers and employees.

The basic idea is to view distribution conflicts as input-output-relationships even if at first sight one of the two, i.e. the input or the output variable, is far from being obvious. As an example imagine an experiment where two persons are asked to share a given positive monetary reward, e.g. one hundred dollars. This could be done, for instance, by either asking one of two individuals to allocate this amount among the two (see Forsythe, Horowitz, Savin, and Sefton, 1988) or by having one of the two make such a proposal with the right of the other to veto it what implies conflict (for a discussion of such experiments see Güth and Tietz, 1990, Prasnikar and Roth, 1990, Thaler, 1988, and Roth, 1994). Whereas the output to be distributed is clearly the monetary amount, i.e. the one hundred dollars, there seems to be no input at first sight. Later on we will try to demonstrate that in the sense of equity theory such situations can also be interpreted as input-output-relationships.

To describe equity theory (see Homans, 1961, Walster, Berscheid, and Walster, 1973, Mikula, 1980, Selten 1978a) we denote by I_i individual i 's input or contribution to the success of group $N = \{1, 2, \dots, n\}$ with $n \geq 2$ and by R_i the

amount of this group success received by individual i . In the following we will refer to I_i as to i 's input, contribution, or investment and to R_i as to i 's reward or payoff. To apply equity theory it is essential that both I_i and R_i are real numbers and that all individual contributions are positive. If this is true, the essence of equity theory can be described by the following simple equation:

$$R_i/I_i = R_j/I_j \text{ for all } i, j \in N$$

This equation, which is the essential hypothesis of equity theory, will be called equity norm. According to equity theory all individual contributions have to be rewarded proportionally. Or in other words: every unit of individual input is rewarded equally.

Certainly equity theory has a great appeal as an ethical norm since any deviation from this norm appears arbitrary if it cannot be justified by arguments why individual investments and contributions should not be treated equally. Although the ethical appeal of equity theory is highly responsible for its empirical relevance, we will view equity theory simply as an empirical hypothesis about allocation results in distribution conflicts.

To validate this hypothesis empirically one has to specify which variables can be used to measure individual contributions and rewards and how one can still apply equity theory if there is more than just one variable to do so. Unfortunately, we will not be able to give rigorous answers to both questions, nor do we claim that equity theory can always be applied to predict the allocation results in distribution conflicts. Although we try to define equity theory more clearly, a rigorous and general formulation seems to be impossible at present. Nevertheless we hope that our study will provoke explanations of distribution behavior by equity theory even in situations where the equity norm as such is of little help.

In view of methodological individualism equity theory could be criticized since it predicts allocation results without explaining why and how the various interacting parties realize them. Will nobody dare to suggest unfair results or is it that unfair proposals are rejected and maybe punished, e.g. by isolating the proposer? To answer such questions one would need a theory of individual decision making in distribution conflicts (Güth, 1993, outlines such a theory which views individual decision making as a dynamic process). But even without such a theory there are many experimental results which shed some light on the problem that guarantees fair results in view of equity theory (a rich source is the survey by Roth, 1994). Güth and Van Damme (1993), for instance, conclude from their experimental studies that fairness is a social norm which needs monitoring. In other words: One can only rely on results as suggested by equity theory if allocation results can be observed, especially by those who have to fear exploitation. This already demonstrates that the conditions which limit the applicability of equity theory can also clarify how individuals try to guarantee fair allocation results.

Requirements for measuring contributions and rewards

In the following we want to specify some conditions which an individual variable has to satisfy to become a candidate for measuring individual contributions or rewards (see also Selten, 1978a, for an earlier attempt). Obviously, individual investments have to be a burden whereas a reward has to be something enjoyable. Since there must be a social pressure for using equity theory, near to the fair solution all individuals must care for smaller contributions and higher rewards. Let $I^* = (I_1^*, \dots, I_n^*)$ with $I_i^* > 0$ for $i = 1, \dots, n$ and $R^* = (R_1^*, \dots, R_n^*)$

be an investment and reward constellation (I^*, R^*) satisfying equity theory. To induce a social pressure for satisfying the equity norm one needs

Condition (LM):

"Local Monotonicity"

At (I^*, R^*) satisfying equity theory all participating individuals have to be interested in smaller individual contributions and higher individual rewards.

Obviously, condition (LM) limits the applicability of equity theory to situations with conflicting individual objectives. One often implicitly relies on condition (LM) when speaking of a distribution conflict. Limiting the applicability of equity theory means, of course, not to accept all deviations from equity theory as negative evidence.

A typical example for such a situation is, for instance, a picnic with nearly equal contributions of all individuals whose individual satiation levels vary considerably. Even if the total supply is not sufficient to satisfy all individual needs completely, we often observe that some persons claim less than their equal share simply because that is more than they are willing to eat. Such deviations from the equity norm do not disprove equity theory since condition (LM) is not satisfied. The fact that there exist individual satiation levels in food consumption shows that local monotonicity can restrict the use of equity considerations.

An essential prerequisite for an individual variable to measure individual contributions or rewards is that it is interpersonally observable and measurable. Selten (1978a) speaks of accessibility. If this would not be true, nobody could control whether the equity norm is satisfied or not. Consequently, there would be no social pressure to comply with this norm.

Condition (IOM):

"Interpersonal Observability and Measurability"

For all participating individuals $i = 1, \dots, n$ the individual investment I_i as well as the individual reward R_i have to be observable and measurable by all n interacting parties.

Nearly all proposals for settling distribution conflicts satisfy condition (IOM) since one usually only makes proposals whose fulfillment can be checked in an objective way, i.e. by each party individually. Although condition (IOM) appears rather obvious and innocent at first sight, it has far reaching consequences.

Even if all n interacting parties would know their own utility function, the disutility of an individual input, the utility of an individual output, or the utility of an individual net trade, i.e. individual output minus the individual input, would not satisfy condition (IOM) since such variables would not be interpersonally observable and measurable. Thus equity theory in the sense of condition (IOM) denies the relevance of utility theory for resolving distribution conflicts. What matters is only local monotonicity in the sense of condition (LM). If an individual claims that he needs a reward more urgently or that an investment is more costly for him although there is no objective indication for it, others will not accept such arguments.

A less controversial requirement seems to be that within the limitations, set by condition (IOM), investments should be the most relevant variables for causing the group's success and that rewards should be the most closely related variables to the individual satisfaction resulting from participating in the group's success (this is also required by Selten, 1978a).

Condition (R): *"Relevance of Contributions and Rewards"*
Among all individual variables satisfying condition (IOM) the individual contribution I_i must be the most relevant investment for the group's success and the individual reward R_i the most related variable to enjoying the group's success.

It is hard to find examples contradicting condition (R). Critical ones seem to be wages which increase with seniority, i.e. wages depending on how long one has been working for the employer. Here one could argue that working time or, if known, individual productivity is much more relevant and that wages should be determined correspondingly. On the other hand individual productivity or working time may not be interpersonally observable and measurable, but highly correlated with seniority. For German university professors, whose wages increase with seniority, one may argue that individual productivity and working time can hardly be judged in an objective way. But it is very questionable whether these are highly correlated with seniority. Thus it is at least doubtful whether this payment scheme is consistent with equity theory.

In our view, payment schemes contradicting equity theory endanger the working incentives of those who suffer from such deviations. This explains to a large degree the inefficiency of the former or present socialist countries as well as the apparent inefficiency of the public sector in market economies which, at least in some countries, includes the education sector. Even if seniority rules are justifiable by other principles, e.g. by saying that older workers need more income since they have children, etc., the disincentives due to deviations from equity theory can easily outweigh other potential advantages.

Our next condition specifying when and how to use equity theory is concerned with comparisons of the contributions I_i and I_j or rewards R_i and R_j of two differ-

ent individuals i and j . To have something specific in mind consider the case when I_i is i 's and I_j is j 's working time. If the type of work performed by individuals i and j is very similar and if working time is measured in the same way, e.g. as working hours or days, then equal rewards for all invested units of working time are an obvious norm. If on the other hand the work performed by i requires far more skills and education than the work performed by person j , equal rewards for one unit of working time by i and j becomes less acceptable and it is less likely to result. Thus equity theory will, in general, only be applied in situations where the rewards and contributions of the various interacting parties are very similar.

Condition (H): *"Approximate Homogeneity of Individual Contributions and Rewards"*
If $N = 1, \dots, n$ with $n \geq 2$ is the group of individuals involved in a distribution conflict, then the equity norm is only applied to two individuals i and j in N for which both the investments I_i and I_j , as well as the rewards R_i and R_j , are very similar, i.e. nearly exchangeable in view of the group's input-output-relationship.

An obvious example for condition (H) is industrial wages which are equal for workers of the same profession but vary greatly from one profession to another. If working time is rewarded equally in spite of the significant differences in skills, either the efforts or the individual rewards are usually negligible. Sports clubs whose members build up and maintain their premises are a typical example for such cases. Although this may call for different skills, only members investing less than average time have to fear complaints by other members.

Our last condition specifying and limiting the use of equity theory concerns the relationship of input and output in general. Proportional rewards, as required by equity theory, are much more acceptable if also the input-output-relationship is also a proportional one, i.e. if the output level depends linearly on the input level.

Condition (L): *"Nearly Linear Relationship between Input and Output"*
At (I^*, R^*) satisfying equity theory the marginal productivity of a further input by an individual i in N should be close to R_i/I_i , i.e. at least locally near to (I^*, R^*) the input-output-relationship appears to be linear.

To illustrate condition (L) consider the situation where group N is a joint venture, e.g. a German Produktionsgenossenschaft, with a single output of which it can sell at most M units. Assume, furthermore, that all members of N have produced individually more than M/n but that the individual production amounts vary rather greatly in this range. Since excess output is useless, it is very likely that N will allow each member to sell exactly M/n instead of allocating sales amounts proportionally to individual output levels.

Other examples are quota cartels, i.e. cartel agreements specifying how much each member is allowed to sell. Usually the quotas are either equal or proportional

to individual production capacities if these can be judged by all cartel members in the sense of our condition (IOM). Even if condition (IOM) applies to individual production capacities, one will expect equal quotas if production capacities can be easily adjusted.

Sales constraints are, of course, rather special examples of non-linear relationships between input and output. A typical example of a non-linear relationship over the whole range of positive inputs is the relationship between heating cost and room temperature for imperfectly isolated rooms.

It can be argued that long existing and generally accepted norms can become internalized moral standards. This might make it possible to rely on such standards even when their prerequisites, specified above, are not guaranteed. A typical example could be anonymous gifts, e.g. by agreeing about everybody's fair share and collecting the money anonymously. We do not recommend such a procedure since the result may be rather embarrassing, especially since there is no way to reimburse everybody in case the collected amount is too small. More generally, such internalized moral standards become very unreliable when strong incentives to exploit others exist. It has, for instance, been shown by so-called double blind dictatorship experiments (see Hoffman, McCabe, Shachat, and Smith, 1992) that anonymity enhances selfish decision behavior. That is why we want to restrict the behavioral theory of distributive justice to situations where its compliance can be interpersonally verified. This does not exclude its wider applicability but only refuses rejections in situations not satisfying the prerequisites elaborated above.

Hierarchy of standards

Conditions (LM), (IOM), (R), (H) and (L) are requirements which a variable will usually have to satisfy to be used for measuring individual contributions or rewards. If a variable fulfills all five conditions, we will refer to this variable as a standard to measure contributions or rewards or a contribution or reward standard. In general, many individual variables might fulfill these five conditions. To give an example consider cost allocation in condominiums where the unit of investment is either the apartment, the square meter, or the person and where often different contribution standards are used for allocating different types of costs.

In an earlier paper (Güth, 1988) it has been demonstrated that competing standards to measure individual investments or payoffs can often be hierarchically ordered according to their inferior or superior information and measurability requirements in the sense of condition (IOM). For allocating costs in condominiums equally to square meters or persons for instance, one has to know exactly the size of each apartment or the number of persons living in the apartments whereas this information is not needed if each apartment has to bear the same cost.

We do not claim that in all situations all variables satisfying conditions (LM), (IOM), (R), (H) and (L) can be hierarchically ordered. In the case of condominiums the different contribution standards, square meters and persons living in the apartment, rely on different information requirements which cannot be hierarchically ordered in the sense that one needs more information than the other. Nevertheless it is often possible to establish such hierarchies between competing standards to measure contributions or rewards. It has been claimed and empirically validated (Güth, 1988) that one can quite well predict for such situations which of the competing standards will be used.

Here we do not want to repeat all theoretical considerations in Güth (1988). What we will do instead is to filter out the main hypotheses underlying them (see especially the flow charts in Figure III.1 and III.2 of Güth, 1988). In the following we speak of two hierarchically ordered (contribution or reward) standards if according to condition (IOM) one of the two standards requires more information than the other, i.e. the interpersonal observability and measurability requirements of one standard implies the ones for the other but not vice versa. In all such cases the standard with the stricter information requirements is called the superior standard whereas we refer to the other variable as the more basic or inferior standard.

Hypothesis (AIS): "Avoidance of Inferior Standards"

A superior standard is used instead of a more basic standard

- (i) if the information conditions of the superior standard can be established at reasonable costs and
- (ii) if there is a strong indication that this yields an essentially different allocation than the one implied by the more basic standard.

Here, of course, the cost in condition (i) is the cost increase between applying the superior and the inferior standard. Both conditions have to be satisfied if the previously applied standard has been the inferior one. Condition (ii) does not make sense if the status quo is the superior standard. We do not exclude that a superior standard is substituted by a more basic one.

Hypothesis (SSS): "Substitution of Superior Standard"

If the superior standard has been previously used, it will be substituted by the inferior standard

- (i) if both standards imply nearly the same allocation results and
- (ii) if the cost of applying the inferior standard is significantly lower.

According to both hypotheses it depends on the (likely) difference in allocation results, on the cost increase of the superior standard, and on the status quo whether the superior or the more basic standard is used. If the inferior standard is the status quo it will be substituted only if both condition in hypothesis (AIS) are fulfilled.

Similarly, both conditions of hypothesis (SSS) must hold to offset the superior standard as status quo. If there is no status quo, as it often happens in laboratory experiments, we believe that both conditions in hypothesis (AIS) have to be true for using the superior standard. In our view, one naturally will start with the more basic way to measure individual contributions or rewards and only consider the superior standard if this yields unsatisfactory results in the sense of condition (ii) of hypothesis (AIS). Thus in the intellectual process of analyzing distribution conflicts the inferior standard serves as the status quo although no previously used standard exists.

Cost allocation in condominiums is again a good example to demonstrate how a more basic contribution standard has been substituted by a superior one. Practically, in all countries one first allocated costs equally to apartments. Later one often used more superior standards, quite frequently also different superior standards for different cost categories like heating costs (usually allocated by square meter and/or by individual consumption) and cost of water usage (where we find various rules ranging from equal costs for apartments, per square meter, per person living in the condominium to allocating according to individual consumption when (drinking) water is very expensive).

In our view, hypotheses (AIS) and (SSS) predict the choice of contribution and reward standards in all situations where the allocation results are of considerable importance for the individuals involved. In situations, where the allocation results are only of little relevance, as it is, unfortunately, true for many laboratory experiments, one often will rely on the more basic standard even if both conditions in hypothesis (AIS) are satisfied:

Hypothesis (ERM): "Exceptional Rule for Minor Results"

In situations where either the individual contributions or the individual rewards are rather minor, i.e. of little relevance as to how successful the day will be, the more basic standard of reward will often be used even if both conditions in hypothesis (AIS) are satisfied.

In our view, hypothesis (ERM) explains the striking experimental results of Hoffman and Spitzer (1982). In their experiment one individual was able to dictate the final allocation of primary monetary rewards for both participants by choosing one of finitely many primary reward vectors. To change this primary reward allocation both individuals could, furthermore, sign a binding contract on side payments after a face to face-communication phase. One potential primary reward allocation gave \$ 12 to the allocator and nothing to his partner. Another primary reward vector with more balanced results gave \$ 14 in total to both participants. Thus the allocator could guarantee himself \$ 12 whereas both participants together could share \$ 14. The surprising result has been that all pairs of participants split the largest total monetary reward of \$ 14 equally.

The two obvious reward standards in this experiment are the monetary reward, which is the more basic reward standard, and the superior reward standard of monetary dividends, i.e. the monetary reward minus the monetary amount which a participant can guarantee himself. According to the experimental design both reward standards were applicable, i.e. the information conditions for both standards were available. Whereas the basic standard implies a payoff of \$ 7 to both participants, the superior standard assigns \$ 13 to the allocator and only \$ 1 to his partner.

According to hypothesis (AIS) one would have to expect the superior standard. This also explains why the experimental observations of Hoffman and Spitzer (1982) caused such a surprise. Although the monetary rewards are rather substantial for a laboratory experiment, the participants apparently did not perceive their individual investment as substantial since both positions were allocated by chance. In view of hypothesis (ERM) it becomes understandable why the more basic standard of equal monetary rewards has been used.

Hoffman and Spitzer (1985) themselves attributed the surprising results to the minor relevance of individual contributions and tried to repeat the experiment with more 'entitled' participants who had to win a hash mark game to become the allocator. Although entitlement influences the results, we do not think that winning a hash mark game is a relevant contribution in the sense of condition (R). On the one hand one can easily derive the solution of such a game so that winning the hash mark is strongly influenced by who is the first one to move in the hash mark game; this was again decided by chance. More important, however, is that the hash mark game is totally unrelated to the distribution conflict. Allocating positions according to the success in the hash mark game is therefore no relevant contribution which fulfills condition (R) (for alternative and better ways to provide entitlement see Güth and Schwarze, 1983, whose procedure has also been used by Güth and Tietz, 1985, as well as by Güth, Ockenfels, and Tietz, 1990).

Empirical evidence

In the following we will try to demonstrate that our theoretical considerations are well supported. More specifically, we will review laboratory experiments and describe real life situations whose allocation results can be easily explained by applying the equity norm together with our five conditions and three hypotheses. Partly, these empirical observations have been crucial for developing our theoretical ideas. Here we just want to illustrate that both experimental data and real life evidence support our theoretical considerations without employing sophisticated test statistics. Statistically satisfactory tests of our hypotheses will require specially designed experiments and selective field research which our study hopefully helps to inspire.

Competing standards in experimental studies of cake division

In the following we will briefly review some experimental results in order to discuss whether they support or contradict the theoretical considerations. Since we are mainly interested in certain aspects of these experiments, we do not always describe these experiments in full detail. Often experiments, which were designed for a rather unrelated purpose, have been very influential in our understanding of how people behave in distribution conflicts.

Nydegger and Owen (1974) have performed a face to face-bargaining experiment with free communication in which both parties had to agree how to allocate two piles of chips with different colors. Whereas for one party all chips had the same monetary value, only one type of chips had an equal monetary value for his partner. The value per chip of the other color was only half as high. By experimental design side payments were excluded. Since the monetary values of chips in both colors and for both participants were common knowledge, there were two reward standards satisfying conditions (LM), (IOM), (R) and (H), namely amounts of chips on the one hand and monetary income on the other.

To measure individual contributions there is not much of a choice since positions were assigned randomly to both participants. In our view, one then has to apply the personal standard which means that individual contributions or rewards should be equal, i.e. the personal contribution standard assumes $I_i = I_j$ for two participants i and j and the personal reward standard $R_i = R_j$. What a participant contributes is therefore being a person with equal rights as in democratic societies. The personal standard will always be used if there is no superior standard for measuring contributions and rewards. In case of the personal standard, condition (L) becomes obviously meaningless and will therefore be neglected.

Of the two reward standards monetary income is clearly superior since it requires knowing the monetary values of both chips for both players, an information which is not needed for the more basic reward standard 'number of chips received'. Apparently, there are no essential cost of using the superior instead of the inferior reward standard. Hypothesis (AIS) therefore predicts that the superior standard should be applied. Actually all pairs of participants have allocated chips in such a way that both participants received an equal monetary income (Nydegger and Owen, 1974) which is a sound confirmation of our hypothesis (AIS). Notice that one party could have asked for a larger monetary reward by arguing that its value for the two piles of chips is greater.

In a similar experiment Roth and Malouf (1979) assigned monetary prizes P_1 and P_2 to both participants which only one of the two will win. Participants were able to bargain via computer terminals about the probability p by which participant 1 wins his prize P_1 whereas participant 2 receives his prize P_2 with the complimentary probability $1-p$.

Since positions were allocated randomly, we rely again on the personal investment standard. To measure rewards there are two candidates, namely the more basic standard of winning probabilities and the superior one of expected monetary income.

The experiment of Roth and Malouf is especially illuminating since they varied the information concerning the monetary prize of the partner. According to condition (IOM) one has to expect the basic standard of winning probabilities if the monetary prize of the other party is not known. Actually, $p = 1/2$ has been the main focal point if only the own monetary prize has been known.

If, however, both prizes are common knowledge, expected monetary income can also be used as a reward standard. According to hypothesis (AIS) it should be used if equal expected incomes lead to an essentially different allocation. Although 'essentially different' is not a clear condition, it definitely was true for one of the games analyzed by Roth and Malouf (1979) for which the monetary prize was three times as high as the one of his partner. Thus hypothesis (AIS) predicts equal expected monetary incomes for both participants as observed by Roth and Malouf (1979): in situations where P_1 and P_2 are common knowledge the main focal point of their experimental data is the winning probability p for which the expected income for both parties is equal, i.e. $p P_1 = (1-p) P_2$ or $p = P_2/(P_1+P_2)$.

Reward allocation experiments are examples of experiments with a unique reward standard, usually monetary income, but competing contribution standards. In such an experiment both participants have to perform a certain task, e.g. writing addresses on envelopes. One of the two participants, whom we will call the allocator, is then informed about his share s of the total work and asked to take away from the total reward what he claims for himself. The residual amount is what he leaves to his partner. Unfortunately, there has not always been a partner in reward allocation experiments, mainly in those performed by (social) psychologists. Typically, the share s differed from $s = 1/2$. Great care was taken that the allocator did not meet his partner, nor the experimenter after having taken away his own reward.

The competing investment standards in such experiments are either the personal investment standard or the individual contribution s , respectively $1-s$, which all satisfy our five conditions (LM), (IOM), (R), (H) and (L). In light of hypothesis (ERMR) the more basic personal contribution standard should be used in situations where the task is rather inessential and not accepted as real work producing the monetary reward. The superior standard requires the additional information about s and should be applied according to hypotheses (AIS) and (ERMR) when the task has been a real burden and when s is essentially different from $1/2$.

Shapiro (1975) investigated situations in which the expectation of future interaction with the partner was varied. If future interaction was expected, allocators with $s < 1/2$ used the contribution standard s as predicted by hypothesis (AIS). But allocators with $s > 1/2$ expecting future interaction have predominantly applied the

personal reward standard, a result which contradicts hypothesis (AIS). Of course, one might argue that the effort becomes more negligible if future interaction and thereby future investments have to be expected and that therefore one has to rely on hypothesis (ERMR) which predicts the personal investment standard. In our view, games with repeated interaction involve new kinds of rewards like cooperative climate or prospects for friendly cooperation in later rounds. Investments to achieve such rewards may explain why allocators deviate from the superior standard to their own disadvantage. Generosity of the more powerful allocator, which at first sight looks like a politeness ritual (Mikula, 1972 and 1977), can rely on reciprocity in the form of future compensation, i.e. on equal contributions in the sense of equity theory.

In a labor allocation experiment (Güth, 1984) two participants together had to solve 12 tables of complex multiplication tasks. Whereas the allocator had a calculator, his partner could not rely on such a tool. A participant received 10 German marks if he correctly solved all the tables which the allocator has assigned to him. By an additional questionnaire participants were asked how much time one needs to solve one table with and without a calculator.

Since monetary rewards were equal (10 German marks per person), equity theory predicts that the allocator will distribute the tables in such a way that individual investments are equal, too. The basic investment standard is obviously the number of tables to be solved. None of the 62 allocators has used this basic standard. Except for 5 selfish allocators, who assigned all the work to their partner, all non-selfish allocators tried to allocate the 12 tables in such a way that the expected working time of both participants is nearly equal. Mostly this meant that allocators assigned to themselves 9 to 12 of the 12 tables.

Expected working time is a rather questionable contribution standard since condition (IOM) is apparently not fulfilled. On the other hand in a reward or labor allocation experiment the allocator has all the strategic power and can therefore rely completely on his personal view of how much time is needed to solve a multiplication table with and without a calculator. The condition that a variable must be interpersonally observable and measurable becomes therefore obsolete.

Another experimental study of cake division is the ultimatum bargaining experiment by Güth and Tietz (1985) in which the positions of both bargaining parties were auctioned. In an ultimatum bargaining experiment two individuals, player 1 and 2, can distribute a given positive monetary amount c which is usually named the cake(size). The very elementary bargaining process is as follows: First player 1 determines his demand d_1 where d_1 can be any monetary amount between 0 and the total cake size c . After having learned about d_1 then player 2 can reject or accept the demand d_1 by player 1. If 2 rejects, both players receive 0-monetary payoffs. In the case of acceptance player 1 gets what he demanded for himself, i.e. d_1 ,

whereas 2 receives the residual amount $c - d_1$. Güth and Tietz (1985) performed an independent auction for the position of player 1 as well as one for the position of player 2. A position was assigned to the highest bidder at the price of the second highest bid.

It was explained to all participants that bidding the true value is always optimal in such auctions (a pretest in the form of normal auctions indicated that around 85 % of all subjects accepted truthful bidding as optimal behavior). Since the monetary payoff of an auction winner is his monetary payoff in the ultimatum game minus his position price, monetary payoffs can be negative, especially if ultimatum bargaining ends with a rejection by player 2.

Unlike in other ultimatum bargaining experiments (for surveys see Thaler, 1988, Ochs and Roth, 1989, Güth and Tietz, 1990, Roth, 1994) allocating strategic positions by auctioning them provides superior standards according to which the cake c can be divided equally. In addition to the more basic reward 'monetary payoffs in ultimatum bargaining' now there also exists a superior reward standard 'dividends' which is the monetary payoff in ultimatum bargaining minus the position price. Of course, to apply the superior standard both position prices would have to be common knowledge according to condition (IOM). In the same way a superior standard can result from the way cake c is generated (for experimental studies where the cake is produced rather than exogenously given see Berg, Dickhaut, and McCabe, 1993, Hackett, 1993, as well as Königstein and Tietz, 1993).

In the experiment of Güth and Tietz (1985) both position prices were private information. Nevertheless their experimental results are quite different from the usual results for ultimatum bargaining (for a comparison see Table 1 in Güth and Tietz, 1990). By an additional questionnaire it has been asked which prices a participant expects for both positions. The answers indicate a much higher expected price p_1 for the position of player 1 than the price p_2 for the position of 2. Actually, the prices for the position of 1 were in average twice as high as the ones for the position of 2. Thus condition (ii) of hypothesis (AIS) is clearly satisfied which explains why demands d_1 with the auctioning procedure are considerably higher than the demands d_1 with randomly assigned positions. If one uses the superior reward standard, the equity norm

$$d_1 - p_1 = c - d_1 - p_2$$

together with the empirical fact $p_1 = 2 p_2$ implies

$$d_1 = (c + p_2)/2;$$

i.e. compared to the case where both position prices are 0, the demand d_1 should increase from $d_1 = c/2$ by $p_2/2$.

One also might interpret position prices as investments. Of course, one would then have to apply the reward standard 'monetary payoffs in ultimatum bargaining'. From the equity norm

$$d_1/p_1 = (c - d_1)/p_2$$

and the empirical fact that p_1 is nearly equal to $2 p_2$, one derives $d_1 \sim 2/3c$, i.e. a demand by player 1 for nearly $2/3$ of the cake c which is a very good prediction for the actually observed demands d_1 .

The ambiguity whether position prices have to be subtracted from rewards or should be viewed as investments cannot be solved by our conditions and hypotheses. For the experimental data of Güth and Tietz (1985) the two formulae $d_1 = c/2 + p_2/2$ and $d_1 = 2 c/3$ yield very near predictions. To resolve this ambiguity one therefore would need a specially designed experiment for which the two formulae imply quite different predictions. Of course, also the empirical fact $p_1 = 2 p_2$, on which the two formulae are based, has to be checked. An even better way would be to announce both position prices before ultimatum bargaining takes place and to test the two hypotheses $d_1 - p_1 = c - d_1 - p_2$ and $d_1/p_1 = (c - d_1)/p_2$ directly.

Here we have confined ourselves to experimental studies of cake division. This does not mean that the behavioral theory of distributive justice is less useful in explaining other experimental observations. Selten (1978a), for instance, tries to explain other experimental results by using equity theory (see also Selten and Kuon, 1993). Especially the most successful concepts for explaining the results of characteristic function bargaining experiments are based on equity considerations which are closely related to the equity norm (see Selten, 1987). The behavioral theory of distributive justice may not be always applicable since other motivational forces become overwhelming but it definitely has far-reaching consequences whenever several individuals cooperate in achieving certain goals.

Non-experimental evidence

Already during our theoretical considerations we often referred to real life situations to exemplify and validate our theoretical ideas. The most frequently cited example was the allocation of costs in condominiums for which one finds various rules in different countries depending on their stage of development and their geographical characteristics which often influence the relevance of certain cost types. So heating costs may be negligible in Tel Aviv, Israel, but are probably a big issue in the northern part of Sweden. Mostly the actually used rules can be easily explained with the help of our conditions and hypotheses. But there are also situations where this seems to be more difficult. As an example for such a situation we consider the German legal rules for the allocation of heating costs which, of course, may change in the near future.

The legal rules for the Federal Republic of Germany are described in the Heizkostenverordnung of February 23, 1981. According to these rules at least 30 %, but not more than 50 % of the total heating costs can be allocated by square meter, i.e. according to the size of an apartment, whereas the remaining part has to

be distributed according to private consumption whose measurement is unfortunately rather inaccurate and known as being manipulable. Cost allocation according to square meter or to private consumption, if the cost of applying the superior standard 'private consumption' is rather low, is certainly in line with our theoretical considerations, especially with hypothesis (AIS). What is difficult to explain, however, is why one should use the more basic standard to allocate a certain quota q of the total cost and the superior standard to distribute the residual share $1 - q$.

Of course, there are some obvious explanations which are in line with the behavioral theory of distributive justice, e.g. that part of the heating costs has to cover amortization and repair costs which should be allocated according to size since these costs are closely related to the number of heating devices in an apartment. Nevertheless it seems difficult to justify general restrictions for the quota q , especially since the share of amortization and repair costs will depend on the technology used, how well the house is insulated as well as on other specific characteristics of the condominium. Being aware of the varying characteristics one must ask why one needs legal rules for cost allocation in condominiums at all. Probably legal rules have some advantages. But forcing people to allocate considerable costs in a way which they do not accept as fair is a major disadvantage which can be hardly overcompensated.

Other frequent distribution conflicts are social meals in restaurants where we assume that the participants are neither relatives, nor close friends who live in a continuing exchange relationship. Lack of experience forces us, furthermore, to restrict ourselves to highly developed economies like the ones in North America and Western Europe. The basic standard in such events is certainly the personal standard according to which costs are allocated equally. However, as predicted by our hypothesis (ERM) this standard is usually applied only if the costs are relatively small or if there are no essential differences in consumption. Although the monetary costs of the superior standard 'Individual Consumption' are rather low or even non-existing, it is often considered as embarrassing to enforce it, e.g. if one wants to allocate the cost of a bottle of wine according to individual consumption. We do not deny that costs are sometimes shared equally even in the case of expensive meals with significantly different consumption. In view of the behavioral theory of distributive justice we, however, warn those with high individual consumption, to suggest the more basic standard instead of the superior one. If they do so, it is rather likely that there will not be many more social meals which they can enjoy. It is the privilege of those, who suffer from the more basic standard, to substitute the superior standard so that it becomes a politeness ritual (Mikula, 1972 and 1977) and not a deal.

One of the most important applications of equity theory is piece rate or hourly wages which fulfill all our five conditions. In principle, both investment standards 'number of pieces produced' and 'working time' seem to rely on different information requirements. But quite often piece rate wages also assume a given working

time per day. At least in these cases 'number of pieces produced' has to be considered as the superior investment standard which according to hypothesis (AIS) should be applied if its information prerequisites are not too costly and if one has to expect wage incomes which deviate essentially from the ones implied by hourly wages.

We do not claim that one will never find an example of wage determination which contradicts our conditions and hypotheses. But serious deviations from distributive justice can cause frustration and endanger labor productivity. In view of hypothesis (AIS) such a deviation can take the form of applying the more basic standard 'working time' although the superior standard should be used according to hypothesis (AIS).

The intrinsic motivation of more productive workers is highly endangered if their less productive coworkers receive the same wage income. Even a profitable firm will run into difficulties soon or later if this happens too often, especially if competitors use wage schedules which are in line with the behavioral theory of distributive justice.

In our view, one should be careful when trying to explain the differences between wages for workers of different skills and profession by applying the behavioral theory of distributive justice that contradicts the homogeneity condition (H). We are aware of attempts to define basic aspects of work efforts in order to make different types of work comparable. But if this is not in line with commonly accepted views about the difficulties involved, wage differences based on such studies are by no means considered justified.

Voting bodies are another important example where equity theory can be used to predict individual voting power. The principle 'one (wo)man one vote' clearly conforms to the norm of equality. Here the investment standard is the personal one, whereas the reward is the right to vote. At least in history other principles have been used, some relying on superior contribution standards. Constitutions like medieval societies which made voting power depend on the (income)tax paid by the voter are a typical example. According to our hypothesis (AIS) voters who pay far more taxes than the average voter should favor such a constitution. At least they may claim that they should be more powerful when deciding on governmental expenditures.

Important voting bodies with differences in voting power are, of course, corporate firms which assign voting power to shareholders proportionally to their share of equity. We do not know of any example where the superior contribution standard 'share of equity' is substituted by the more basic personal investment standard. Shareholders with no voting power, an institution which exists at least in Germany, are no exception since this is a mixture of an owner and a creditor relationship. According to condition (H) such shares cannot be compared with normal shares.

Other interesting examples are private clubs which often provide certain rewards equally and require proportional investments for other rewards. So a tennis club might provide a weekly tennis training freely, whereas one may have to pay for using the tennis courts. This would mean that the basic personal standard and the superior standard 'individual consumption' are used simultaneously.

Cartel agreements and joint ventures are further situations where the behavioral theory of distributive justice is very powerful in explaining how profits are shared. Especially, hypothesis (AIS) seems to be very useful in explaining whether profits are shared equally or proportionally to investments like, for instance, production capacities. If production capacities of others can hardly be judged and if, furthermore, one does not expect great differences in capacities, hypothesis (AIS) predicts the personal investment standard, e.g. equal profit shares. If, however, it is clear that production capacities are very different, one will rely on the superior investment standard 'production capacities' even if one has to rely on rough estimates.

The fact that members of a producers' cartel treat each other fairly in the sense of equity theory does by no means exclude that they are quite unfair with respect to their customers. Actually, condition (H) restricts equity considerations to members of homogeneous groups. So a producers' cartel will typically conclude an agreement which treats all its members equally and provide equal sales conditions for all customers although it tries to exploit customers by monopolistic prices. Similarly, criminals are known to split rewards equally although they can be rather cruel otherwise. This demonstrates that considerations of distributive justice should be restricted to investments satisfying the homogeneity assumption (H). For the personal investment standard condition (H) implies that one has to partition the group of interacting individuals into homogeneous subgroups like the sellers and the customers on a market with a sellers' cartel.

Distributive justice and human decision making

Human decision making is at most boundedly rational in the sense of being goal oriented and well considered given the obvious limitations of the human cognitive system. In view of some recent studies in game theory which refer to bounded rationality if recall is limited, it seems worthwhile to mention that limited recall does not at all capture the essential restrictions of human cognition. Due to limited recall rational decision makers often have to solve far more complicated optimization problems than with perfect recall. We, of course, do not question that it is very interesting from a normative point of view to explore the consequences of limited recall.

In our view, distributive justice should be included into a theory of boundedly rational decision behavior as one of the competing goals which the decision maker may want to achieve. To demonstrate the need for doing so consider the acceptance behavior by player 2 in ultimatum bargaining experiments. If the only motivation of player 2 is to earn money, player 2 must accept all demands d_1 by player 1 satisfying $d_1 < c$. But what one observes is that players 2 are willing to sacrifice quite substantial amounts (up to 20 German marks in the laboratory experiments of Güth and Tietz, 1985) to punish a greedy player 1. Furthermore, such a behavior does not disappear with experience. On the contrary, experienced players more often induce conflict than inexperienced players (see Güth, Schmittberger, and Schwarze, 1982). If we do not want to qualify the acceptance behavior by players 2 as irrational, we must include a desire to comply with basic norms of distributive justice (see Güth, 1993, for a systematic attempt).

A far more difficult question is, however, how a theory of boundedly rational decision making can be based on such different objectives as, for instance, to earn money and to comply with basic norms of distributive justice. In our view, there is not only just one incidence when justice considerations enter the dynamic process of decision making (Güth, 1993). Partly, we seem to rely on a partitioning device of the following form: by a brief investigation, based on previous experiences, it is decided whether this is a situation where justice considerations should be employed or one where one can try to be selfish. This can explain the frequently observed equal splits in dictatorship (Forsythe et al., 1988,) reward or labor allocation (Shapiro, 1975, Mikula, 1972 and 1977, Güth, 1984), and ultimatum bargaining experiments (see Güth and Tietz, 1990, and Roth, 1994). Apparently, these participants immediately viewed the situation as one where norms of distributive justice should be applied.

According to this partitioning device the experimental data should be clustered, once around the decisions implied by justice considerations and once around the ones implied by selfish behavior. Here, of course, interaction processes might change an initial interpretation so that one should restrict the clustering hypothesis to initial moves.

At least for dictatorship and reward or labor allocation experiments this is essentially true. In the labor allocation experiment of Güth (1984) 5 of 62 allocators have been selfish whereas the decisions of all others could be explained by the attempt to balance expected working time.

However, this is not true for ultimatum bargaining experiments where at least in experiments with high monetary rewards (see, for instance, Güth and Tietz, 1985, Güth, Ockenfels, and Tietz, 1990, and the most recent survey by Roth, 1994) the typical demand d_1 by player 1 is nearly $2/3$ of the whole cake c . Apparently, these players 1 were faced with an intrapersonal decision conflict either to exploit strategic power and to inspire thereby a desire for revenge by player 2 or to comply with norms of distributive justice.

In our view the partitioning device describes only an initial phase of the dynamic decision process. After the initial decision of a player 1 in an ultimatum bargaining game to be selfish this player will easily see that player 2 will prefer to veto a proposal if the demand d_1 is very greedy and if the costs $c - d_1$ of such a veto for player 2 are negligible. At least such players 1 are confronted with an intrapersonal decision conflict or a trade off-relationship between conflicting goals in the later phases of their decision making process. Furthermore, the experimentally observed data suggest that such intrapersonal decision conflicts are frequently resolved by compromises, i.e. by attempts to achieve balanced levels of goal achievement. The typical demand $d_1 = 2c/3$ in ultimatum bargaining, for instance, can be viewed as an attempt to balance the desire for exploiting strategic power on the one hand and the risk of conflict on the other hand.

It seems promising to view human decision making as a dynamic process with an initial partitioning phase and a later phase where the decision maker is fully aware of intrapersonal decision conflicts due to his own conflicting goals (for a more general approach to dynamic decision making see Selten, 1978b and 1990 as well as Güth, 1993). Especially designed experiments may help to understand when a decision is already made by the partitioning device and when and how compromises result in case of intrapersonal decision conflicts.

An illuminating study is the ultimatum bargaining experiment by Prasnikar and Roth (1990) who let many participants compete for the position of player 2 and who assigned the position of player 2 to the subject whose payoff demand was minimal. All participants competing for the position of player 2 had to submit their demand d_2 meaning that one accepts all demand d_1 with $c - d_1 \geq d_2$. The actual player 2 was the one with the minimal demand d_2 . Unlike in other experimental studies of ultimatum bargaining the experimental data support the game theoretic prediction $d_1^* = c - \epsilon$ where ϵ is a positive but small monetary amount.

This shows that other motivational forces, here the desire to become player 2, can seriously suppress the norm of distributive justice. Also the impossibility to punish deviations from distributive justice may result in unfair treatment (see Berg et al., 1993). Compromises to resolve intrapersonal decision conflicts can therefore result in unfair allocations if other competing motivations become dominant. Especially, strategic considerations may completely dominate considerations of distributive justice, as argued by Binmore, Morgan, Shaked, and Sutton (1991) and demonstrated by Berg et al. (1993).

But, of course, also the converse is true. Depending on the framing of a social decision problem, which might strongly influence the initial partitioning decision, as well as its economic structure the desire for distributive justice may become overwhelming as is most clearly demonstrated by the (non double blind) dictatorship and reward or labor allocation experiments.

Conclusions

Although we claim to define the behavioral theory of distributive justice more clearly, we have not developed a really satisfactory and generally applicable concept. Neither do our conditions and hypotheses yield unique predictions, nor can we always classify them as well supported facts. On the other hand a behavioral theory of distributive justice is urgently needed and the little progress here might count more than more satisfactory results for less relevant research problems.

This study will hopefully encourage other researchers to investigate distribution behavior more thoroughly, either theoretically or by searching for empirical evidence. This hope seems to be well founded since this study points more often to open research problems instead of providing satisfying answers. What is needed are specific studies testing the validity of our hypotheses, e.g. by designing specific experiments as those relying on "cake production" rather than on exogenously given cakes (Berg et al, 1993, Hackett, 1993, and Königstein and Tietz, 1993). But like the experimental study by Roth and Malouf (1979) also other experiments with different aims may teach us a lot about when and how considerations of distributive justice become relevant.

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Chapter 9

Public Choice - Economic Theory of Politics: A Survey in Selected Areas

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Summary

In this chapter an overview over some selected areas of economic theory of politics (public choice) is given. Politico-economic models are concerned with the interaction between politics and economics and analyze the relationships between voters, government, the central bank and interest groups. One major result from studies of these models is that politicians are often motivated by self interest, for example, they attempt to secure their re-election by applying expansionist economic policy measures before election. Public choice analysis has been widened to cover other aspects, too, like international organizations, and direct investment in developing countries is influenced by political and economic factors. In the next step, particular emphasis is given to compare and analyze alternative political and economic institutions and possibly rectify not only political but also market failure.

Key words

Public choice, economic theory of politics, politico-economic model, government, voters, interest groups, international political economy

Introduction

Presenting an overview of the development and the current state of research in the field of public choice, or the economic theory of politics, is not easy because areas both inside and outside economics are included. In what follows, therefore, I concentrate on only some of the areas which fall within the economic theory of poli-

tics. It should be mentioned that good surveys are already available, for example, the publications by Mueller (1979, 1989), and in the German speaking countries the annual volumes on the economic theory of politics by Herder-Dorneich, Schenk and Boettcher, which have appeared since 1982. It is, therefore, not the intention of this paper to present a comprehensive study but rather to focus on some selected areas. Part 2, which follows, deals with the current state of research in two areas of the economic theory of politics, using positive analysis as the main approach, that is, explaining the particular circumstances in politics and economics. The first main point relates to the development of politico-economic models. These models are concerned with the interaction between politics and economics and analyze the relationships between voters, governments, the central bank and interest groups. The second section, on the development and current state of research regarding politico-economic models, ends with some remarks about the international political economy. In the final section, a summary is given and some conclusions are drawn.

One major result from studies in the economic theory of politics is that politicians are often motivated by self-interest, for example, they attempt to secure their re-election by applying expansionist economic policy measures before elections. Economic policy of this type can lead to rapid increases in the budget deficit and/or an increasingly high burden of direct and indirect taxation on the voters within representative democracies. In the latter case we observe that the voters/taxpayers react to this, and try, at least in part, to avoid state control via evasion of taxes and non-payment of national insurance. This poses the question of whether or not changes to the political institutions in representative democracies should be considered in order to prevent such "political failure".

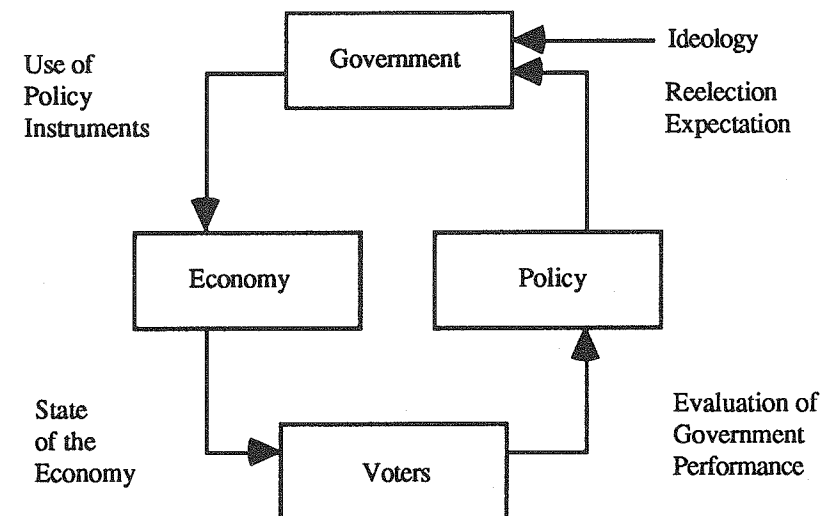
Development and current state of research in some areas of public choice

Public choice, or the economic theory of politics, is mainly concerned with the application of the economic approach to the political process. In many cases this is done using politico-economic models which take into account the interdependence of economics and politics. The first such models were developed by Downs (1957)¹⁾. Within these, the economic concept of rational behavior, i.e. the pursuit of individual self-interest, is transferred to the political process. The first models stressed the behavior of two agents, the voters and the government, in a representative democracy²⁾. It is assumed that the voters support the party whose political program most closely corresponds to their own preferences, and that the government tries to obtain a majority of votes in order to pursue its own goals.

Figure 9.1 shows a simple representation of the political and economic connections.

The upper part of the diagram shows that the government implements economic policy to pursue its own ideological aims or to secure re-election. The arrow in the middle indicates that the government is subject to economic restrictions (e.g. limits on deficit financing, worsening balance of payments, etc.). The implementation of such measures influences the economic situation for the voters. The voters also judge the overall state of the economy according to the situation in the labor market, changes in prices and incomes as well as the provision of state services. The (subjective) estimation of general and personal economic well-being is an important factor in the evaluation of the government performance - this is shown in the lower part of the diagram. Depending on the result of this evalua-

Figure 9.1
A Simple Politico-economic Model



tion, the voters either support or reject the government in elections or opinion polls. Public opinion also supplies information to the government regarding the extent to which they should implement policy measures to increase their chances of re-election. This type of interdependence between politics and economics has been analyzed theoretically for approximately 25 years and empirical investigations have already been carried out on numerous representative democracies. Country cases are well documented in many studies³⁾ and I shall not go into detail here. In summary, these studies confirm that, in representative democracies, a statistically reliable and quantitatively significant connection exists between the state of the economy in a country and election results or the popularity of the government:

the healthier the economy is perceived to be, *ceteris paribus*, the higher the standing of the government in the estimation of the voters, and the better its chances of re-election. The strength of this relationship, however, varies from country to country and does not remain constant over time. Governments also appear to manipulate this relationship, i.e. they introduce "popular" measures before the election but otherwise pursue policies which are close to their ideological position. This can lead to a politico-economic business cycle. By this it is meant that during the first half of its period in office the government consciously pursues restrictive policies in order to "produce a recession", and during the second half it implements expansionary economic policy to bring about economic recovery and thus improve the voters' economic circumstances⁴. Such politico-economic business cycles result in the re-election of the government only when the voters' take a myopic view, i.e. when they judge the government's economic policy record and the resulting state of the economy in the election year alone. Such conclusions from politico-economic models and the appearance of politico-economic business cycles have led to criticism of these models⁵. The criticism is at two levels. Up to now the economic theory of politics has been unable to explain the high level of turnout in elections, as observed in many countries, using the economic model of behavior; the economic theory of voting suggests that for the vast majority of voters it would be rational not to participate in a vote or ballot. All attempts to explain this contradiction or to avoid it by using a better theory have proved unsuccessful, and the high participation rates in elections can only be "explained" when sociological factors are applied, such as, the feeling of well-being experienced by the voter when he takes part in an election and thus fulfills his duty as a citizen⁶.

In addition, doubts were cast on some developments within the economic theory of politics and in particular on whether the politico-economic models which had been developed were at all consistent with rational behavior on the part of the participating agents. Stigler (1973), for example, had already had his doubts about this. Kirchgässner (1984) and Ramser (1985) particularly objected to the theories by Nordhaus and MacRae on the politico-economic business cycle as they were not compatible with rational behavior by the economic agents. This relates particularly to the voters' behavior as represented in the politico-economic models. Most empirical investigations of the influence of the state of the economy on voting behavior come to the conclusion that the voters have extremely short memories: events which take place more than one year before the election do not affect the way the people cast their votes. Such results are achieved by estimating the vote and popularity functions⁷. From this one can conclude that the voters are "short-sighted"⁸, i.e. myopic. This is clearly not consistent with rational expectations or even limited rational expectations. Similarly, the rational expectations approach is not compatible with the fact that voters assess the performance of a government only on past economic policies and their effects. If the voters make

their decisions on the basis of rational expectations, as is assumed in many cases relating to economic activity, then they should look not only to the past but also to the future. Past values of economic variables are of interest to the voters only in so far as they contain information about the expected behavior of a government in the future. Voters who behave according to the theory of rational expectations will only then change their opinion of a government's performance when new information becomes available. Only when estimates for vote and popularity functions take this into account is it possible to see whether the voters behave rationally in the sense of Muth (1961). Kirchgässner (1984, 1984a, 1985, 1986) carried out such estimates for the Federal Republic of Germany and came to the conclusion that voters react in a consistent way regarding the popularity of the West-German Government not only to unexpected economic events (e.g. an unexpected increase in the number of unemployed), but also to expected events. Kirchgässner showed that a considerable number of voters did not behave according to the theory of rational expectations and thus the traditional estimates from vote and popularity functions could be valid. One can assume, therefore, that when the concept of limited rationality, as developed by Simon (1955) is used as the basis, the results from vote and popularity functions are completely consistent with rational voting behavior. If one accepts these empirical results, that in the long term voters do not behave according to the theory of rational expectations, then a government appears to have room to maneuver, and to engineer political business cycles⁹. In terms of policy control of the business cycle, however, this does not always have to be the case. According to Kirchgässner (1986, p. 117) it is quite possible that a large number of economic agents/voters do not behave in line with the theory of rational expectations but that the actions of a small minority, who do behave according to this theory, are sufficient to weaken (or even prevent) economic policy which systematically influences demand and the economy. This question of the extent to which a government can influence the business cycle via economic policy to suit its own intentions remains open and presents researchers with an insoluble problem. On the other hand, the theory of rational expectations has led to the current view that simple models of politico-economic business cycles, as developed, for example, by Nordhaus, can no longer be scientifically justified. In particular, the assumption in these models that voters allow themselves to be systematically and repeatedly deceived does not make sense when the learning process of economic agents is adaptive and not rational. Even if the assumption of voters' behavior based on rational expectations has to be rejected in a political context, the theory of rational expectations has still provided an important stimulus to research in the area of the economic theory of politics.

Politico-economic models have been widened still further to cover other aspects. For example, Schneider (1979) and Frey and Schneider (1981) incorporated the Central Bank into a politico-economic model for Germany. The idea here is that the Central Bank primarily pursues a policy of price stability because by doing

this it strengthens its position vis à vis Government and increases its prestige within the banking sector (its main point of reference). In pursuing this policy, the main limiting factor is that of avoiding conflict with the stated economic policy of the Government. If such a conflict arises, for example, if the Government pursues expansionary policies in order to reduce unemployment, the Central Bank tends to support the Government and subordinate its own self-interest - otherwise it would have to fear erosion of its independence in the longer term. In the United States and in Germany there is empirical evidence for such behavior¹⁰. Interest groups have also been integrated into politico-economic models. For example, Gärtner (1981, 1981a) investigated to what extent incomes policy depended on political influences as well as the usual economic factors (e.g. inflation rate). In terms of political influences, the following hypotheses were developed.

(i) Each shift to the left in government ideology results in an increase in the rate of wage inflation: other demands made by the unions (such as more participation in decision-making or improved social services) have already been taken up by a social democratic government and the unions can therefore concentrate on wage negotiations.

(ii) Wage inflation rates are cyclical with the election cycle: in an election year, the unions will demand lower wage increases if the Government is one which is well-disposed (social democratic) towards them as opposed a conservative administration. Both hypotheses were tested and overwhelmingly confirmed by Gärtner using quarterly data for the period from 1960 to 1976 for Germany.

Schneider (1985a) attempted to assess the influence of the four leading interest groups (the industrialists' association, the employers' association, the unions, and the farmers' association) on Swiss economic policy. He developed a politico-economic model for the Swiss system of direct democracy which took account of these four associations as well as the voters and the Swiss Government. The interaction of these three agents and the influence of these four leading interest groups was investigated in terms of the outcomes of referenda on economic issues and on the budget policy pursued by the Federal Government. An empirical analysis of the positions taken up on these referenda shows that two of the four leading associations (the unions and the farmers association) had a quantitatively significant influence: if these two associations put out the YES (NO) recommendations, then the vote on referenda with some economic policy content increased (decreased) - *ceteris paribus* - by an average of 8 percentage points. Empirical results on the influence exerted by the leading associations on economic policy measures implemented by the Federal Government of Switzerland clearly implies that an influence exists, but that its strength varies from association to association. Thus the industrialists' association and, to a lesser degree, the employers' association have a greater influence on the development of Federal Government expenditure policy than the farmers' association or the unions.

It should be clear from this discussion that it makes sense to take into consideration the activities of other agents (e.g. interest groups) in politico-economic models; further research is needed regarding their importance for economic policy in western democracies. This applies particularly (but not only) to the inclusion of public administration.

Public choice analysis now extends to international organizations and international politico-economic connections¹¹. Frey and Schneider (1986) show that a model based on the "self-interest of World Bank bureaucrats" is much more appropriate in explaining the behavior of the World Bank regarding the granting of loans to developing countries than, for example, the approach which assumes that the poorest countries should receive the greatest share of development aid. The empirical results from this study show that the former French and British colonies, as well as countries which are very dependent on the United States, are consistently given preference by the World Bank. This is due to the fact that these three countries provide the largest share of the credit available to the World Bank.

In addition, it can be shown using public choice analysis that direct investment in developing countries by multinational companies is influenced by political and economic factors. Weck-Hannemann, Schneider and Frey (1987) constructed a behavior model in which, alongside the usual economic factors, political determinants are included. On the basis of politico-economic theories, hypotheses for direct investment were derived. Finally, the model was tested for direct foreign investment in developing countries. The model was also contrasted with conventional, purely political and purely economic models, and with a model in which all political and economic factors are represented by an index of risk for each country. The simultaneous consideration of political and economic factors of influence within a politico-economic model is shown to be the most appropriate method of explaining direct foreign investment.

The international political economy is an area of research where the theoretical hypotheses and, in particular, the empirical applications are still being developed. Up to now, analysis has concentrated on single areas of the international economic network. The areas described have often been investigated separately by political scientists and by economists but rarely from the point of view of the new political economy.

Summary and some conclusions

In representative democracies, governments very often use increases in expenditure (especially transfer payments) to strengthen their own interests. Elected politicians can do so because the institutional framework in many representative democracies places them - albeit for a limited period - in a monopolistic position. This allows them to pursue economic policy which is either in line with their own

(ideologically determined) policy goals, or "popular" in the sense of being necessary to secure re-election. In representative democracies, the voters welcomed an extension to state intervention only as long as the increased tax burden was not noticeable, and if, in the eyes of the public, the load was spread more or less evenly. Only when many western countries were affected by the deep recession of the 1970's, and were no longer able to finance the budget deficit by just increasing the national debt, did the voters begin to feel the increasing tax burden. When, in this situation, governments planned a cutback in expenditure (especially on the social services), then they were quickly confronted by the "public good dilemma". This cutback in expenditure gave rise to a stiff resistance by those affected, as they alone had to bear the costs. However, the beneficiaries from these measures, almost every voter/taxpayer, did not particularly support these government measures because they all benefited from lower taxes and thus for the individual it was an insignificant improvement.

From this discussion of the behavior of the voters and the Governments in representative democracies, the following conclusion can be drawn: as long as costs of continuously increasing state activity are not particularly noticeable to the voters/taxpayers, the voters will approve of additional expenditure. If the Government is compelled to make cuts in its expenditure, because of deficits and/or exhausted sources of finance, then it will not receive any particular support from those who benefit (all voters/taxpayers), but it will meet with considerable resistance from those who suffer, and may lose their votes. This is particularly true when the voters/taxpayers have no influence over which programs are to be cut and how the money saved is to be spent. In this situation the Government has only limited room for maneuver to secure its re-election by increasing public expenditure, and it will therefore try to attract extra votes by introducing other economic policy measures (e.g. by announcing lower rates of taxation during the election year).

In contrast to representative democracies, voters in direct democracies, e.g. in Switzerland, can influence particular changes in policy via referenda. In addition, groups of voters can try to bring about changes, for example in social policy, by means of a peoples' initiative. An empirical investigation of two referenda on the provision for old aged pensioners, (as well as widows and widowers) in Switzerland in 1972 by Pommerehne and Schneider (1985) clearly demonstrated that the voters are not only guided by their short-term interests but also take long-term aspects into account.

The interesting question now is what further conclusions can be drawn from this discussion when the economists have the job of advising politicians who want to carry out "reforms", which these days often means drastic cuts in transfers, subsidies and other expenditures. The demand made by Thompson (1983) for more "fiscal discipline", or to link any additional expenditure on social services to tax increases, may well be seen as a step in the right direction, but an analysis of how such proposals can be implemented in representative democracies is lacking. The

same applies to demands to link the financing of social projects to particular types of taxation and to fix the time scale for the project by law¹²). Furthermore, such "reform proposals" often underestimate the creativity of the civil service and other interest groups, whose arguments may be based, for example, on the needs of those who receive social assistance¹³). Taking these difficulties into consideration, Frey and Pommerehne (1983) suggest that in order to carry out far-reaching and often unpopular changes in economic policy as part of the on-going political process, two types of institutional change are needed:

(1) the creation of smaller and more open political units which can be more easily understood by the individual citizen/voter/taxpayer, and which provide a greater incentive for the public to inform themselves about political issues and then, possibly, contribute to the decision-making process;

(2) the possibility of having particular political decisions confirmed directly by voters in national referenda. As has been shown in Switzerland, the voters are able to decide on complicated issues such as the future form of the pension system. It may well be that such consensus in the political process carries over to other issues more easily in a direct democracy than in a representative democracy.

However, the question remains as to how the institutional changes proposed should be achieved. To answer this question, a theory of institutions (and the process of change within them) needs to be developed; at the moment, such a model is available in only a simple form¹⁴). The first stage is to analyze the existing institutions and the second step is to compare various political institutions. From these results it is possible to assess more accurately the feasibility of putting proposed changes in economic policy into practice.

Self-interest on the part of governments in representative democracies has contributed to a considerable increase in the burden of taxation on the poor and middle income members of society (especially in the 70's). Increasing taxation and the accompanying set of government rules and regulations, which for the individual citizen are often incomprehensible, lead to the situation where more and more people try to avoid both. This evasion manifests itself, on the one hand, in a much intensified do-it-yourself movement, and, on the other hand, in the emergence of a shadow or underground economy¹⁵).

Intensified activity in the shadow economy can aggravate still further the political dilemma of "the empty coffers", as this causes additional tax losses and, to a greater extent, a fall in income from national insurance contributions¹⁶). If the Government now increases taxation to cover these losses, this may lead to further evasion in these areas, which raises the whole question of the financing of state services. Estimates of the size of the shadow economy in the OECD countries demonstrated a considerable increase over the last two decades¹⁷). By the end of the 70's the majority of OECD countries already had a shadow economy equivalent to 8 % of gross national product. For Austria a value of between 5 % and 6 % was estimated¹⁸). According to Frey and Pommerehne (1984) and Schneider and Neck

(1992), the most important reasons for the growth in the shadow economy are the rapid increase in the burden of taxation, the multiplicity of state regulations and state interference, and the deterioration in the tax ethic. If the shadow economy is to be contained, reliable measures must address these issues. However, if the financing of previously safeguarded services (e.g. the state pension scheme) is in doubt, the citizens will lose faith in public institutions and this can again provide the incentive to obtain additional income by working in the shadow economy. The economic policy measures necessary to break this vicious circle are difficult to define and research is only just beginning in this area¹⁹⁾.

It is clear from these considerations and conclusions that many interesting results have been achieved using the public choice approach, and that they provide at least a partial answer to many open questions. Particular emphasis should be given to those studies which compare alternative political and economic institutions, making it possible to recognize and possibly rectify not only political but also market failure. The public choice approach is especially useful with respect to political failure in representative democracies, because it shows how limited the scope for change as part of the on-going political process actually is²⁰⁾.

Footnotes

¹⁾ Hotelling (1929) and Schumpeter (1946) had already applied the economic approach to the political process, and as such were forerunners to Downs.

²⁾ For further early studies see Frey & Lau (1968), Nordhaus (1975), Frey & Schneider (1975).

³⁾ Cf. Kirchgässner & Schneider (1979), Paldam (1981), Borooah & Ploeg (1983), Van Winden (1983), Schneider (1978, 1982, 1985), Kirchgässner (1984, 1986), Schneider & Frey (1988), Alesina & Carliner (1991) and Nannestad & Paldam (1993).

⁴⁾ Politico-economic business cycles were first developed by Nordhaus (1975), Lindbeck (1975) and MacRae (1977).

⁵⁾ This criticism is not only directed at the theoretical approach (cf. Kirchgässner (1984a) and Ramser (1985)), which is discussed later in the text, but also at the numerous - but not very successful - attempts to confirm empirically such politico-economic business cycles (cf. McCallum (1977), Lächler (1978, 1982), Paldam (1979), Breuss (1980) and Kirchgässner (1984), Mueller (1989).

⁶⁾ Some methods of explaining voter participation using the economic behavior model are presented in Schneider (1978) and, taking political institutions into consideration, Jackmann (1987), Mueller (1989) and Darvish & Rosenberg (1988).

⁷⁾ There are now so many empirical investigations into vote and popularity functions that here only summaries are mentioned: Paldam (1981), Schneider

(1982, 1985), Kirchgässner (1986), and Schneider & Frey (1988), Borooah and Schneider (1991), Paldam (1991).

⁸⁾ Most models used to estimate vote and popularity functions are based on the assumption that voters are conscious of the past, i.e. only past events are used to judge the government. This approach is theoretically justified by the concept of limited rationality due to Simon (1955). A good survey of the more recent methods is given in Kiewiet (1983) and Kiewiet & Rivers (1985) and in Mueller (1989).

⁹⁾ This is also shown both theoretically and empirically in the studies by Kirchgässner (1984, 1984a, 1985), Hibbs (1987).

¹⁰⁾ Cf. Parkin & Bade (1978), Puchta (1981), Woolley (1983), Willett & McArthur (1985), and Anderson & Schneider (1986).

¹¹⁾ Frey (1985), Vaubel (1986), Weck-Hannemann (1990, 1992) explain international organizations. Baldwin (1976), Magee (1982) and Findlay & Wellisz (1983) have carried out public choice analyzes to explain duties and trade restrictions. Frey (1984) and Frey & Schneider (1984) give surveys for the international political economy.

¹²⁾ Cf. Thompson (1983) who discusses these proposals in great detail and, in his conclusions, is skeptical as to whether these measures alone can lead to the desired result.

¹³⁾ Pommerehne (1979) gives a good example of the interaction of bureaucrats, politicians and voters in the case of housing grants in Germany.

¹⁴⁾ Gäfgen (1983) and Frey (1984a) made the first attempts in this direction. Schneider (1986) carried out a comparison of various socio-political institutions.

¹⁵⁾ Cf. Pommerehne (1983) and Weck (1983), who deal in great detail with the causes of the shadow economy.

¹⁶⁾ Whether really significant tax losses result, depends, among other things, on how much of the money earned on the "black market" is spent in the "legal" economy. First results from Schneider & Hofreither (1987) show that in Austria the marginal rate of consumption from income earned on the black market is approximately 0.50 and the tax loss is therefore limited.

¹⁷⁾ The number of studies, which deal with the calculation of the size of the shadow economy, is such that here only some of the surveys are mentioned. Cf. Frey & Pommerehne (1983), Weck-Hannemann, Pommerehne & Frey (1984), and Schneider & Weck-Hannemann (1986).

¹⁸⁾ Cf. Hofreither & Schneider (1987), and Schneider, Hofreither and Neck (1989).

¹⁹⁾ Cf. Weck-Hannemann, Pommerehne, & Kirchgässner (1987).

²⁰⁾ The question of how political self-interest in representative democracies can be limited by constitutional measures was investigated by Buchanan (1977), Brennan & Buchanan (1980) and Frey (1981).

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Chapter 10

Measuring the Size and Development of the Shadow Economy.

Can the Causes be Found and the Obstacles be Overcome?

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Summary

In this chapter the various methods to measure the size and development of the shadow economy are shortly discussed and criticized. Then the results of two methods, the currency demand and model approach are presented and it is shown that for the year 1978 and for 16 of 24 OECD-countries the size of the shadow economy is more than 5 % of the GNP. The next step is to analyze the effects of policy change on the shadow economy, like a major decline of the direct tax burden in the year 1989 in Austria with the expectation of a decreasing shadow economy. However, one observes an increasing shadow economy in the years 1989 to 1991 in Austria, because other important factors, why people work in the shadow economy, like regulation, have increased during this time, which offset the lower tax burden.

Key words

Shadow economy, tax burden, regulation, currency demand approach, model approach, Austrian shadow economy

1. Introduction

Over the last two decades a growing concern over the phenomenon of the shadow economy has increased attention among officials, politicians and social scientists. For Austria as for many other industrial countries, there are several important rea-

sons that politicians should be concerned about the size and growth of the shadow economy. Among the most important of these are:

1) An increase in the size of the shadow economy is mainly caused by a rise in the tax burden which may lead to an erosion of the base and finally to a decrease in tax receipts and thus to a further increase of the budget deficit or to further increases of direct and / or indirect tax rates with the consequence of an additional increase in the shadow economy, etc.

2) Under a growing shadow economy, (economic) policy is based on mistaken "official" indicators (like unemployment, official labor force, income, consumption), it may to say the least, be wrong in magnitude. In such a situation a prospering shadow economy may cause politicians severe difficulties, because it "provides" unreliable official indicators; the very direction of intended policy measures may therefore be questionable.

3) The rise of the shadow economy can be seen as a reaction by individuals being overburdened by state activities (such as high taxes and an increasing number of regulations).

4) A growing shadow economy may have strong incentive effects to attract workers (domestic and foreign) to work in the shadow economy and work less (efficiently) in the official economy.

These growing concerns have led many economists to the challenging task to measure the size and development of the shadow economy, to trace back the main causes of it and to analyze the interactions of the official and unofficial economies. Various approaches have been used; they are presented in section 2. In section 3 some results of two most commonly-used methods for almost all OECD-countries are shown and the development of the size of the shadow economy over time for Austria is presented. In section 3, too, it is investigated whether the size of the shadow economy can be influenced by economic policy conditions. Finally, section 4 summarizes the results as well as the difficulties of measuring the shadow economy.

2. Methods to estimate the size of the shadow economy

Most studies measuring the shadow economy start with a commonly-used working definition of it: all currently unregistered economic activities which contribute to the "value added" should be included in the national income, in accordance with national accounting conventions ¹⁾. Using this definition three different types of methods are most widely used to measure the size and development of the shadow economy.

2.1. Direct approaches

These micro approaches employ either well designed surveys and samples based on voluntary replies ²⁾ or tax auditing and other compliance methods ³⁾. In most cases they lead only to point estimates (i.e. one estimate at a specific point of time). It is unlikely that they capture all "shadow" activities, so they can be seen as providing lower bound estimates. Moreover, they are unable (at least at present) to provide estimates of the development and of the growth of the shadow economy over time. But they have at least one considerable advantage - they can provide detailed information about shadow economy activities and the structure and composition of those who work in the shadow economy.

2.2. Indirect approaches

These approaches, which are also called "indicator" approaches, are macroeconomic ones and use various economic indicators that contain information about the development of the shadow economy (over time). There are at least four macro-economic indicators which leave some "traces" of the development of the shadow economy:

(1) *A Discrepancy between National Expenditure and Income Statistics.* In most OECD countries the size of the GNP is computed both from the expenditure and income side of the national accounts, which often reveal that expenditure is higher than income. This "initial discrepancy" can be seen as a result of activity in the hidden economy ⁴⁾. The weakness of this "fiscal" method is that the differences may arise not only because of activities in the shadow economy, but also because of well-known errors in measurement statistics. These estimates may therefore be very crude and of questionable reliability.

(2) *The Discrepancy between the Official and Actual Labor Force.* A decline in participation in the labor force in the official economy can be seen as an indication of increased activity in the shadow economy. If it is assumed to be constant, a decreasing official rate of participation can be seen as an indicator of an increase in the activities in the shadow economy ⁵⁾. The weakness of this method is, that differences in the rate of participation may also have other causes. Moreover, people can work in the shadow economy and have a job in the "official" economy. Again, such estimates may be viewed as weak indicators of the size and the development of the shadow economy.

(3) *The Transactions Approach.* This approach has been developed by Feige ⁶⁾. It assumes that there is a constant relation over time between the volume of transaction and official GNP. This approach therefore starts from Fisher's quantity equation, $M \cdot v = p \cdot T$ (with M = money, v = velocity, p = prices, and T = total

transactions). Assumptions have to be made about the velocity of money and about the relationship between the value of total transactions ($p \cdot T$) and total nominal GNP. Relating total nominal GNP to total transactions, the GNP in the shadow economy can be calculated by subtracting the official GNP from total nominal GNP. However, to derive figures for the shadow economy, Feige has to assume a base year in which there is no shadow economy, and therefore the ratio of $p \cdot T$ to total nominal (official = total) GNP was "normal" and would have been constant over time if there had been no shadow economy. This method, too, has several weaknesses: for instance, the assumption of a base year with no shadow economy, and the assumption of a "normal" ratio of transactions constant over time. Moreover, to obtain reliable estimates precise figures of the total volume of transactions should be available. This availability might be especially difficult to achieve for cash transactions, because they depend, among other factors, on the quality of paper of the bills used in the currency ⁷⁾. In general, although this approach is theoretically attractive, the empirical requirements, necessary to obtain reliable estimates, are so difficult to fulfill, that its application may lead to doubtful results.

(4) *The Currency Demand Approach.* This approach assumes, that shadow (or hidden) transactions are undertaken in the form of cash payments, so as to leave no observable traces for the authorities ⁸⁾. An increase in the size of the shadow economy will therefore increase the demand for currency. To isolate the resulting "excess" demand for currency, an equation for currency demand is econometrically estimated over time, with controls for all conventional possible factors, such as the development of income, payment habits, interest rates, and so on. Additionally, such variables like the direct and indirect tax burden, government regulation and the complexity of the tax system which are assumed to be the major factors that cause people to work in the shadow economy, are included in the estimation equation. The "excess" increase in currency, which is the amount unexplained by the conventional or normal factors (mentioned above), is then attributed to the rising tax burden. Figures for the size and development of the shadow economy can be calculated by a comparison of the difference between the development of currency, when the direct and indirect tax burdens government regulations are held at its lowest value, and the development of currency with the current (much higher) burden of taxation and government regulations. The currency demand approach is one of the most commonly used approaches. It has been applied for 17 of the 24 OECD-countries ⁹⁾, but has nevertheless been criticized on various grounds ¹⁰⁾. The most commonly raised objections to this method are:

1) An objection relates to the fact that not all transactions in the shadow economy are paid in cash. Isachsen and Strom (1980, 1985) used the survey method to discover that in Norway in 1980 roughly 80 % of all transactions in the hidden sector were paid in cash. The size of the total shadow economy (including barter)

may thus be even larger than previous estimates. Most studies consider only one particular factor, the tax burden, as a cause of the shadow economy. But others (such as the impact of regulation, taxpayers' attitudes to the state, "tax morality" and so on) are not considered because reliable data for most countries is not available. If, as seems likely, these other factors also have an impact on the extent of the hidden economy, it might again be higher than reported in most studies ¹¹⁾.

2) A further weakness of this approach, at least when applied to the United States, is discussed by Garcia (1978) and Park (1979), who point out that increases in currency demand deposits are due largely to a slowdown in demand deposits rather than to an increase in currency caused by activity in the shadow economy. Blades criticizes Tanzi's studies on the grounds that the US dollar is used as an international currency, so that Tanzi should have considered (and controlled for) the amount of US dollars held in cash abroad. Moreover, Frey and Pommerehne (1984) claim that Tanzi's parameter estimates are not very stable ¹²⁾.

3) Another weak point of this procedure is the assumption of the same velocity in both types of economies used in most studies. As Klovland (1984) argues for the Scandinavian countries, there is already considerable uncertainty about the velocity of circulation of currency in the official economy; the velocity of currency in the hidden sector is even more difficult to estimate. Without knowledge about the velocity of currency in the shadow economy, one has to accept the assumption of an "equal" currency velocity in both sectors.

4) Finally, the assumption of no shadow economy in a base year is open to criticism. Relaxing this assumption would again imply an upward adjustment of the figures attained in the bulk of the studies already undertaken.

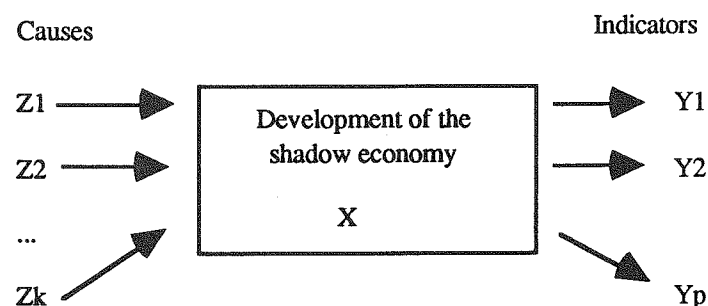
2.3. The model approach¹³⁾

So far all previously discussed methods designed to estimate the size and development of the shadow economy consider only one indicator to capture all effects of the shadow economy. But it is obvious that the shadow economy effects show up simultaneously in the production, labor, and money markets. An even more important criticism is, that the various determinants of the size of the hidden economy are taken into account only in some of the monetary approach studies, which in most cases consider a single cause, the burden of taxation. The model approach explicitly considers multiple causes of the existence and growth, as well as the multiple effects, of the shadow economy ¹⁴⁾. The empirical method used is quite different from the approaches discussed above. It is based on the statistical theory of unobserved variables which considers multiple causes and multiple indicators of the phenomenon to be measured. For the estimation, a factor analytic approach is

used to measure the hidden economy as an unobserved variable. The unknown coefficients are estimated in a set of structural equations within which the "unobserved" variable cannot directly be measured. The MIMIC (Multiple Indicators Multiple Causes) model in general consists of two parts, the measurement model and the structural equations model. The measurement model links the unobserved variables to observed indicators. The structural equations model specifies causal relationships among the unobserved variables. In this case, one has only one unobserved variable, the size of the shadow economy. It is assumed to be influenced by a set of exogenous causes, namely another set of variables is assumed to serve as the set of indicators for the size of the shadow economy. The interaction between the causes Z_i ($i = 1, 2, \dots, k$), the size of the shadow economy X , and the indicators Y_j ($j = 1, 2, \dots, p$) is shown in Figure 10.1.

Figure 10.1

The Development of the Shadow Economy



There is a large body of literature ¹⁵⁾ on the possible causes and indicators of the shadow economy, in which the following three types of causes are distinguished:

- i) The burden of direct and indirect taxation, both actual and perceived: a rising burden of taxation provides a strong incentive to work in the shadow economy
- ii) The burden of regulation as proxy for all other state activities. It is assumed that increases in the burden of regulation gives a strong incentive to enter the shadow economy.
- iii) The "tax morality" (citizens' attitude toward the state) which describes the readiness of individuals (at least partly) to leave their official occupations and enter the shadow economy. It is assumed that a declining tax morality tends to increase the size of the hidden economy.

A change in the size of the shadow economy may be reflected in the following indicators:

- i) Development of monetary indicators: if activities in the shadow economy rise, additional monetary transactions are required.

- ii) Development of the labor market: increasing participation of workers in the hidden sector results in a decrease in participation in the official economy. Similarly, increased activities in the hidden sector may be expected to be reflected in shorter working hours in the official economy.

- iii) Development of the production market: an increase in the shadow economy means that inputs (especially labor) move out of the official economy (at least partly); this displacement might have a depressing effect on the official growth rate of the economy.

A weakness of the model approach is that at present the estimation procedure does not allow for undertaking a pure time series analysis ¹⁶⁾. Another difficulty, when applying this approach for European countries, is to obtain reliable data for the cause series besides the ones of direct and indirect taxes burden.

This discussion of the various methods for estimating the shadow economy demonstrates that each one has difficulties, and that none is anywhere near perfect. Because of their various strengths and weaknesses, it makes sense to apply as many as possible, using a variety of methods so that they may complement one another and to produce results which may be better valid. However in most cases only the currency demand and model approach is used for most OECD-countries.

3. Empirical results of the size of the shadow economy

3.1. Findings for OECD-countries

As already argued, two methods, the currency demand approach and the model approach, have been applied for most OECD-countries. The results are shown in table 10.1.

If one first compares the results for the currency demand approach, it can be seen that Italy (1978:30%), Spain (1978:23%), Belgium (1980:20,8%), and Sweden (1978:13,6%) have the largest shadow economies. In the mid group are the UK (1978:11%), Norway (1978:10%), Germany (1978:9,2%), Ireland (1980:8%), Denmark (1978:8%) and at the lower end Canada (1979:7,8%), France (1979:6,7%), Switzerland (1978:6,2%), the USA (1978:6,1%) and Austria (1978:2,6%). If one compares this ranking with the model approach by Frey and Weck-Hannemann, Italy, Belgium and Sweden are then also among the four countries with the largest shadow economy. In the middle group are also Norway and Germany, and at the bottom ranges Switzerland, too. This comparison, which is crude and should not be given too much credence shows a degree of coincidence between the two approaches. In general, both approaches demonstrate that the results for Austria, Denmark, Norway, Germany, Sweden, Switzerland and the USA, where values from 1970-1980 are available, are all plausible.

Table 10.1

Size of the Shadow Economy Applying the Currency Demand and Model Approach^{a)}

Country	Author(s)	Currency-demand-approach						Model approach	
		Size of the shadow economy (in % of official GNP)						Size of the shadow economy (in % of official GNP) for	
		1960	1965	1970	1975	1978	1980	1960	1978
Austria	Schneider and Neck (1992)	--	1.1	1.8	1.7	2.6	3.1	4.6	8.9
Belgium	Geeroms (1983)	--	--	--	--	--	20.8	4.7	12.1
Canada	Mirus and Smith (1982)	--	--	--	5.8-7.2 ^{b)}	--	10.1-11.2	5.1	8.7
Denmark	Schneider (1986)	3.8-4.8	5.0-6.3	5.3-7.4	6.4-7.8	6.7-8.0	6.9-10.2	3.7	11.8
Federal Republic of Germany	Kirchgässner (1984)	2.0-2.1	3.6-4.3	2.7-3.0	5.5-6.0	8.1-9.2	10.3-11.2	3.7	8.6
Finland		--	--	--	--	--	--	3.1	7.6
France	Barthelemy (1982)	--	--	--	--	6.7	--	5.9	9.4
Ireland	Boyle (1982)	--	--	--	--	--	8.0	1.7	7.2
Italy	Saba (1980)	--	--	--	--	30.0	--	4.4	11.4
Japan		--	--	--	--	--	--	2.0	4.1
Netherlands		--	--	--	--	--	--	5.6	9.6
Norway	Lundager and Schneider (1986)	1.3-1.7	3.2-4.1	6.2-6.9	7.8-8.2	9.6-10.0	10.2-10.9	4.4	9.2
Spain	Lafuente (1980)	--	--	--	--	23.0	--	2.6	6.5
Sweden	Lundager and Schneider (1986)	1.5-1.8	3.7-4.6	6.8-7.8	10.2-11.2	12.5-13.6	11.9-12.4	5.4	13.2
Switzerland	Weck-Hannemann, Pommerehne and Frey (1986)	1.2	1.6	4.1	6.1	6.2	6.5	1.1	4.3
United Kingdom	Matthews (1981)	--	--	--	--	3.3-11.0 ^{c)}	--	4.6	8.0
United States	Tanzi (1983)	2.6-4.1	2.5-3.8	2.6-4.6	3.5-5.2	3.7-5.3	3.9-6.1	6.4	8.3

^{a)} Explanations: A slash means that there exists no value for this period for this country. The ranges in the size of the shadow economy for the single years come from (i) the use of different tax variables; (ii) different specification of the dependent variable and estimation equation; and (iii) different assumptions about the velocity of currency.

^{b)} For 1976

^{c)} Value for 1979

SOURCES: For the currency-demand-approach see references. The values for the model approach are taken from Weck-Hannemann, Pommerehne and Frey (1984).

3.2. The calculation of the Austrian shadow economy over time

In view of the large number of studies measuring the size of the shadow economy in Austria's neighboring countries Germany, Switzerland and Italy, it is somewhat

astonishing that only a few attempts have been made to measure the size of the Austrian shadow economy ¹⁷⁾: Frey and Weck-Hannemann (1984) used the technique of the unobserved variables to compute the size of the shadow economy for the OECD-countries and estimate a shadow economy of 8.9 percent (of official GDP) for Austria in 1978: Franz (1985) computed the scope of the shadow economy on the basis of official data, which were available in a very detailed form only for 1976. Comparing the income earned in different occupational sectors, Franz estimated the size of the shadow economy to be 3.5 percent of official GDP for 1976. Furthermore, he argued, that this figure remained more or less constant until 1982 ¹⁸⁾. Apart from these two studies, which only provide results for certain years, only Hofreither and Schneider (1987), and Schneider and Neck (1992) have made an attempt to measure the size and the development of the Austrian shadow economy over an extended period of time.

The method chosen for estimating the shadow economy in Austria over time is the currency demand approach. After having discussed its major weaknesses in section 2, the question may arise as to why this approach has been chosen. The answer is that (i) one has reliable time-series data for Austria over the period 1958 to 1991 concerning the monetary sector and different measures of the direct and indirect tax burden, the intensity of regulation and the complexity of the tax system as major causes of the shadow economy; and (ii) as the currency-demand approach is the most widely used approach, one can make comparisons of the development of the shadow economy of other countries.

In applying the currency-demand approach, the procedure developed by Klovland (1984) is applied. His basic model relates the stock of currency demanded by the public to the price level, the volume of transactions in the regular economy and the interest rate as a measure of the opportunity costs of holding currency. Furthermore, he employs only the marginal tax rate as one causal variable for shadow economy activities. A similar function has been estimated by Schneider and Neck (1992) and is used to calculate the development of the shadow economy in Austria from 1960 to 1991 ¹⁹⁾. As in most studies for Austria's neighboring countries, as well as for the Scandinavian countries, it is assumed that there would have been no shadow economy if the direct marginal and the indirect tax rate as well as the amount of regulation had remained at their historical minimum from 1965 until 1991. Keeping tax rates and regulations at the minimum level of the year 1960, the "normal" (without shadow economy) level of currency holdings is calculated by undertaking a dynamic simulation. The difference between the actually observed and the simulated currency holdings is assumed to reflect the amount of currency used for shadow economy transactions. Assuming the same income velocity for currency used in the shadow economy as for legal M1 in the official economy, the size of the shadow economy is computed and compared to the official GDP ²⁰⁾.

In Table 10.2 the results for the development of the Austrian shadow economy over the period from 1965 to 1991 are shown.

Table 10.2

Size of the Austrian Shadow Economy *)

Year	Official Gross Domestic Production, real	Value Added in the Shadow Economy, real	Size of the Shadow Economy in percent of the "official" GDP
1965	440.95	5.14	1.16
1966	466.20	5.69	1.22
1967	479.98	5.49	1.14
1968	502.18	6.21	1.24
1969	533.44	8.31	1.56
1970	571.25	10.47	1.83
1971	600.31	9.58	1.59
1972	637.69	10.41	1.63
1973	669.29	13.34	1.99
1974	695.79	12.46	1.79
1975	692.84	12.02	1.73
1976	724.75	15.39	2.12
1977	756.12	18.01	2.38
1978	760.23	20.06	2.63
1979	795.96	22.88	2.87
1980	820.03	24.98	3.05
1981	818.58	26.96	3.29
1982	828.62	29.09	3.51
1983	845.54	31.86	3.77
1984	862.66	36.38	4.22
1985	887.39	36.98	4.16
1986	904.46	39.64	4.38
1987	924.84	41.22	4.46
1988	959.91	46.37	4.83
1989	1004.09	50.89	5.07
1990	1061.32	54.56	5.14
1991	1126.32	59.89	5.32

*) Assumptions made for the calculation of the shadow economy:

- (i) All transactions in the shadow economy are made in cash.
- (ii) Direct and indirect tax burdens, the visibility of the tax system and the intensity of regulations are the reasons for working in the shadow economy.
- (iii) In 1960 the shadow economy did not exist.
- (iv) The velocity of currency is the same in the shadow economy as in the official economy and is calculated by dividing total official income by M1.
- (v) For the calculation, the currency-demand equation from the appendix (first equation) is used!

The Austrian shadow economy increases more or less steadily and reaches a peak value in 1991 with 5.32 percent of official GDP. The conclusion by Franz (1985) and Mooslechner (1985) that the shadow economy has not grown from 1976 to 1991 is not supported by these findings.

3.3. The influence of major tax reforms on the shadow economy

What is amazing too, is, that the size of the shadow economy more or less steadily grew up to the year 1991 and in the year 1991 it reached a value of 52.56 billions of schillings (over 5 percent of official GDP). Especially from this rather strong increase in the last four years it is astonishing that the major tax reform Austria had in the year 1988, with significant lower direct average and marginal tax rates, had no negative effect on the increase of the shadow economy.

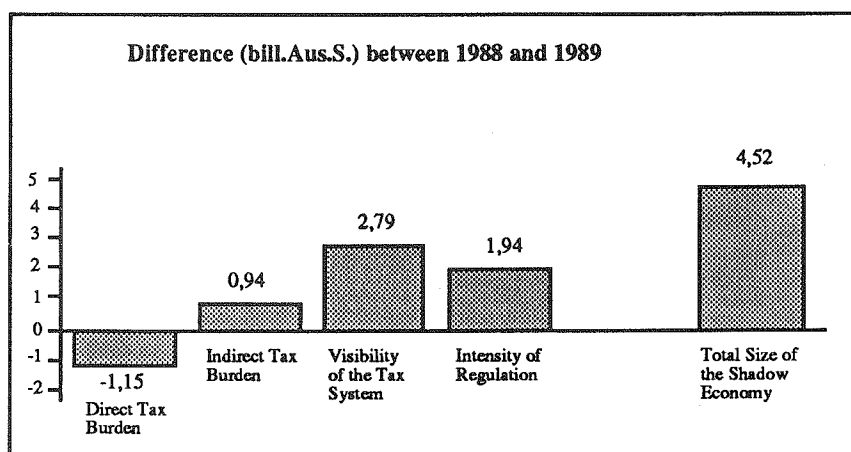
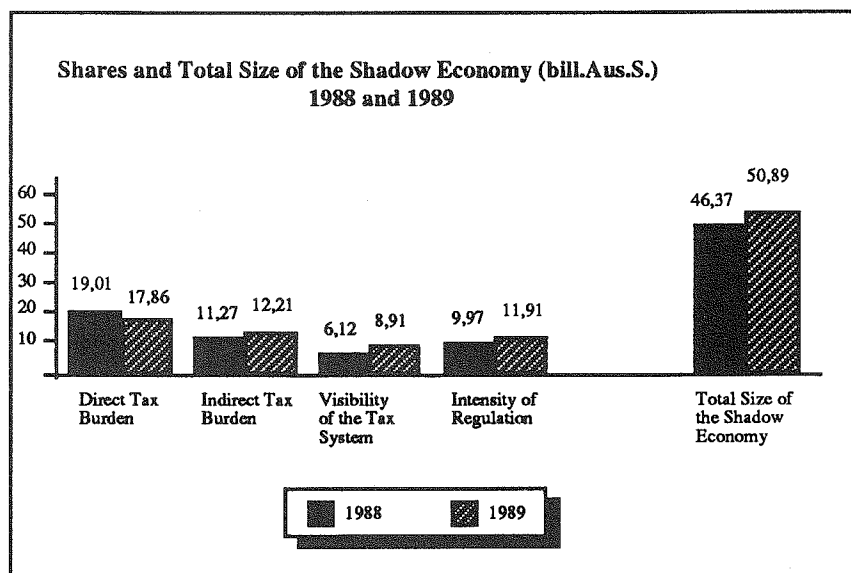
This increase of the size of the shadow economy obtained by Schneider and Neck (1992) "contradicts" simulation results of Schneider, Hofreither and Neck (1989), where it could be shown that lowering the direct tax burden has a significant negative impact on the shadow economy. In their study it is as in most other studies assumed, that a high tax burden is one of the major causes of the shadow economy activities. If now the government undertakes tax reform, i.e. it lowers direct tax-rates it can hope that people will reduce their shadow economy activities due to the lower incentive to work in the shadow economy. In the study by Schneider, Hofreither and Neck (1989) it is simulated, that the government reduces the wage earners tax burden by 5 percent every year as compared to the control solution starting in the year 1975. The aim of lowering the marginal tax burden to such an extent is to let people have less (more) incentive to work in the shadow (official) economy. This means that the suppliers of unofficial activities will react with a reduction of the shadow economy activities when tax rates are decreased ²¹⁾. The simulations in Schneider, Hofreither and Neck (1989) show that the shadow economy decreases on average by 8.81 percent over the period 1975 to 1985 which is quite a considerable amount. However, as the results in this study indicate (compare Table 10.2), such a development seemed not to occur in the years 1988 to 1991 when in 1988 we had significant decrease of the direct (average and marginal) tax burden.

An explanation why the shadow economy did not decline is offered in Schneider and Neck (1992). They investigate the influence of three major tax changes (1972/73, 1983/84 and 1988/89) on the development of the shadow economy. If one considers the effects of the major tax reform in 1988/89, which is shown in figure 10.2, one realizes that under ceteris paribus conditions a considerable reduction in the direct tax burden would have decreased the Austrian shadow economy by 1.15 billion schillings (a reduction of 2.5 % of a shadow economy of

Figure 10.2

The Influence of Changing Tax Systems on the Shadow Economy

Year	Size of Shadow Economy (bill.Aus.S.)	Share of			
		Direct Tax Burden	Indirect Tax Burden	Visibility of the Tax System	Intensity of Regulation
1988	46,37	19,01	11,27	6,12	9,97
1989	50,89	17,86	12,21	8,91	11,91
Diff.	4,52	-1,15	0,94	2,79	1,94



46.37 billion schillings in 1988). However, the negative effect of the decreased personal income tax rates is more than twice offset by the strongly increased visibility of the tax system²²). The increased regulation intensity compensates the negative effect of the reduced income tax rates, too. To summarize: The decline of the direct tax burden had a negative effect on the size of the shadow economy, but it had been offset by a decrease in complexity of the tax system and an increase in regulations (especially in the labor market).

3.4. Summary and future research areas

Coming back to the question in the beginning of this contribution, one realizes that there are many obstacles to be overcome to measure the shadow economy and to analyze the consequences on the official and unofficial economy, but some progress has been made. I hope to have shown that, though it is difficult to estimate the size of the shadow economy, it is not impossible! I have introduced, discussed and criticized various methods and have demonstrated that with at least two methods, the currency demand and the model approach, some insights can be provided about the size and development of the shadow economy of the OECD-countries. The general impression from the results of the two methods is that the shadow economy is sizable. For 17 of the 24 OECD-countries the estimate of the studies shown in table 10.1 for the year 1978, demonstrate that its size is more than 5 % of GNP. As it has already been argued, there is no "best" or commonly accepted method; each approach has its specific strengths and weaknesses and each specific insights and results. Although the different methods provide a rather wide range of estimates, there is general agreement that the size of the shadow economies for the OECD-countries has been growing over the recent decades. This has been especially demonstrated for Austria, where the shadow economy increased from 12.02 (1.73 % official GNP) billion Austrian schillings in the year 1975 to 59.89 (5.32 % of official GNP) billion schillings in the year 1991.

The next step is to analyze in much greater detail the effects of a changing official economy on the development of the shadow economy. When the official economic conditions change like a decrease of direct tax burden one would expect that the shadow economy would decrease. However, I observed in the period from 1988 to 1990 that even when a major tax reform with significant lower tax rates had been undertaken that the shadow economy is still increasing, because other important factors, why people work in the shadow economy, like regulation, have increased during this time. Hence, the effect of a lower direct tax burden was more than offset by the increased regulation.

In general these results should be seen as a first step to study the interactions between the official and the shadow economy. In the next steps it should be worked out in a much more precise way how the shadow economy is also influenced by non-economic influences (like the tax morale and peoples' attitude towards the state) and what consequences these influences have on the official and unofficial (= shadow) economy.

Footnotes

¹⁾ This definition is used in most studies which try to measure the size of the shadow economy, see e.g. Smith (1981), Feige (1982), Frey, Weck and Pommerehne (1982), Frey and Pommerehne (1984), Kirchgässner (1984), Schneider (1986), Schneider and Neck (1992), and Thomas (1992).

²⁾ The direct method of voluntary sample surveys has been extensively used for Norway by Isachsen, Klovland and Strom (1982), and Isachsen and Strom (1985). An attempt for Denmark to use this method is made by Mogensen (1985) in which he reports an "estimate" of the shadow economy of 5.5 % (of GNP) for the year 1984 as a minimum figure.

³⁾ Compare for the United States, International Revenue Service (1979), Simon and Witte (1982) and Clotefelter (1983)

⁴⁾ Compare, e.g., Macafee (1980) for Great Britain, Petersen (1982) for Germany, and for the United States, Park (1979).

⁵⁾ Such studies have been made for Italy, see e.g., Contini (1981) and Del Boca (1981), for the United States, compare O'Neil (1983), for a survey compare Thomas (1992).

⁶⁾ For an extended description of this approach see Feige (1979, 1982), and for a further application for the Netherlands, Boeschoten and Fase (1984), and for Germany, Langefeldt (1984).

⁷⁾ For a detailed criticism of the transaction approach see Boeschoten and Fase (1984), Frey and Pommerehne (1984), and Kirchgässner (1984).

⁸⁾ The currency demand approach was first used by Cagan (1958), who calculated a correlation of the currency demand and the tax pressure as one cause of the shadow economy for the United States over the period 1919 to 1955. 20 years later Gutman (1977) used the same approach but did not use any statistical procedures; instead he "only" looked at the ratio between currency and demand deposits over the years 1937 to 1976. Cagan's approach was further developed by Tanzi (1980, 1983) who estimated a currency demand function for the United States for the period 1929 to 1980 in order to measure the shadow economy; for a survey

compare Thomas (1992).

⁹⁾ Compare e.g. Boeschoten and Fase (1984), Lundager and Schneider (1986), and Schneider and Neck (1992).

¹⁰⁾ See e.g. Garcia (1978), Park (1979), Blades (1982), Frey and Pommerehne (1984), Klovland (1984), Kirchgässner (1983, 1984), and Schneider (1986).

¹¹⁾ One (weak) justification for the only use of the tax variable is, that this variable has by far the strongest impact on the size of the shadow economy in all studies known to the author. The only exception is the study by Frey and Weck-Hannemann (1984) where the variable "tax immorality" has a quantitatively larger and statistically higher influence than the direct tax share in the model approach. In the study of Pommerehne and Schneider (1985) where in the case for the U.S. shadow economy besides various tax measures data for regulation, tax immorality, minimum wage rates are available, the tax variable has a dominating influence and contributes roughly 70 - 78 % to the size of the shadow economy.

¹²⁾ However in studies for European countries Kirchgässner (1983, 1984) and Schneider (1986) reach the conclusion that the estimation results for Germany, Denmark, Norway and Sweden are quite robust when using the currency demand approach.

¹³⁾ This part is a summarized version from a longer study by Aigner, Schneider and Ghosh (1988) applying this approach for the United States over time.

¹⁴⁾ The model approach has been developed in pioneering studies by Weck (1983) and Frey and Weck-Hannemann (1984) who applied this approach for cross-section data from the 24 OECD-countries for various years.

¹⁵⁾ Compare, e.g., Weck (1983), and Gaertner and Wenig (1985).

¹⁶⁾ Aigner, Schneider and Ghosh (1988) make an attempt to use this method for a time series estimation of the U.S. shadow economy.

¹⁷⁾ This short description is a summary of a study by Hofreither and Schneider (1987). A comprehensive survey concerning all aspects of the Austrian shadow economy is given in Skolka (1985).

¹⁸⁾ Franz reached this conclusion quoting a study by Mooslechner (1985) who tried to apply monetary approaches (including the currency-demand approach) for measuring the shadow economy. Mooslechner argued that, on the basis of financial indicators, it is not likely that the shadow economy has increased significantly in the last decade.

¹⁹⁾ The estimation in the study by Schneider and Neck (1992) which differs from the estimation in Schneider, Hofreither and Neck (1989), has been extended from 1985 to 1991 and measures for regulation and the complexity of the tax system have been added. The empirical results are shown in Appendix 1.

²⁰⁾ Because of a complete lack of knowledge about the velocity of money in the shadow economy, this assumption is made here as in most other studies using

this approach (e.g., Tanzi, 1980, 1983; Kirchgässner, 1983, 1984; Isachsen and Strom, 1985; Schneider, 1986).

²¹ This assumption of such a "symmetric" reaction is implicitly made in the currency demand approach; it is, however, questionable whether people react to a decrease of the tax burden with a reduction of the shadow economy activities to the same amount (compare Pommerehne (1986)), because they may have invested in the shadow economy activities to a considerable extent and do not find it worthwhile to reduce their activities.

²² In the study of Schneider & Neck (1992) and Schneider (1994) it is assumed that the complexity/visibility of the tax system is also an important factor for determining the shadow economy. They argue, as the existence of revenue sources and tax exemptions increase, the tax system becomes more and more complex and less visible. In this case, a tax increase is much less recognizable ("felt" much less) by the taxpayer and hence leads to a lower increase in the shadow economy activities than under a simple tax system.

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Appendix

Appendix 10.1

Estimation Results of the Currency Demand Function for Austria *)

Independent variables	Dependent variable: Real currency per capita, $\ln(\text{CUR}_t/\text{POP}_t)$	
	Estimation period:	
	1956 - 1991	1956-1985
Lagged dependent variable $\ln(\text{CUR}_{t-1}/\text{POP}_{t-1})$	0.534 ** (8.91)	0.551 ** (9.43)
Real consumption c, per capita $\ln(C_t/\text{POP}_t)$	0.703 ** (5.49)	0.724 ** (5.99)
Number of eurocheque systems per capita $\ln(\text{ES}_{t-1}/\text{POP}_{t-1})$	- 0.213 * (- 2.51)	- 0.174 * (-2.65)
Real interest rate on bonds $\ln(\text{IR}_t)$	- 0.123 * (-2.51)	- 0.139 * (- 2.65)
Direct tax burden (incl. social security payments) $\ln(\text{DIRT}_t)$	0.173 ** (3.09)	0.182 * (2.86)
Indirect tax burden $\ln(\text{INDT}_t)$	0.117 (*) (1.88)	0.123 (*) (1.92)
Complexity of the tax sys- tem $\ln(\text{COMP}_t)$	0.154 ** (2.77)	0.147 ** (2.86)
Intensity of regulation $\ln(\text{REG}_t)$	0.186 ** (2.94)	0.179 ** (2.72)
Constant term	- 2.24 (*) (- 1.80)	- 2.39 (*) (- 1.74)
Test Statistics		
R ²	0.992	0.990
S.E.	0.014	0.015
Durbin's h	1.06	1.16
rho (1)	0.18	0.20
D.F.	27	21
Ex-post Forecast		
1985-1991		
RMSE	-	1.51
Theil's U 1	-	0.42

) All equations are estimated by an ordinary least-squares procedure using annual data. POP is the Austrian Population (over 18 years old); t = time period (year). \bar{R}^2 is the coefficient of determination (corrected for the degrees of freedom); S.E. shows the standard error of the estimation. Durbin's h-test against autocorrelation when lagged dependent variables are used as regressors. Rho (1) is the autocorrelation coefficient of first order. D.F. stands for the "degrees of freedom". RMSE is the root mean squared error and Theil's U 1 stand for Theil's inequality coefficient. The term "ln" indicates that these variables have been transformed to natural logarithm. Numbers in parentheses below the estimated coefficient estimates are t-values. (), *, and ** indicate significance at the 90%, 95% and 99%-security-level, respectively.

Chapter 11

Why People Vote: The Role of Inter- and Intragroup Interaction in the Turnout Decision

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Summary

A model is presented which places the decision to vote or abstain in a rational choice framework. It is shown that casting a vote may well be a rational act, following from an individual cost-benefit analysis. It is argued that (reference-)group interests play an important role when an individual determines actions in the political sphere. Through its (relative) turnout, a group can affect future tax rates to which its members are liable, and an optimal turnout-level is derived for each group. Using this optimal level, within-group processes are analyzed, where certain group members ('producers of social pressure') try to convince others to go and vote. For these producers, voting is shown to be a rational act. Other members may give in to this pressure and be induced to cast a vote. These members may be thought to vote out of a sense of 'civic duty'. Equilibria for the model, characterized by positive turnout, are derived, an example is presented, and the results are discussed.

Key words

civic duty, leadership ability, opinion leaders, rational choice, social pressure, voter turnout, voting paradox

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1. Introduction

In this paper a model is presented, in which the decision to cast a vote or to abstain is made by making a cost-benefit analysis. In this respect, the model fits into the tradition of rational choice models in this field. The latter were initiated by the well-known theory of Downs (1957), who argued that expected benefits from voting in large-scale elections (computed as the benefit to be obtained if one's favorite candidate wins times the probability that one's vote is decisive in the election) are generally outweighed by the costs of this act, so that rational behavior would lead to abstention. Important extensions were brought forward by Riker and Ordeshook (1968) and Ferejohn and Fiorina (1974). The former argue that voters may derive utility from the act of voting *per se*, through the fulfillment of a sense of 'civic duty' (an idea previously launched by Tullock, 1967, p. 110), whilst the latter show that defining rationality as minimizing maximum regret instead of maximizing expected utility makes it more likely that the act of voting is rational. While Riker and Ordeshook have been criticized because of the ad hoc nature of their contribution (see Barry, 1970, for example), Ferejohn and Fiorina have been confronted with many theoretical objections to their minimax regret analysis (see a number of contributions to vol. 69 of the *American Political Science Review*, for example). One objection put forward is that in their analysis the probability of being decisive in an election has absolutely no influence on the turnout decision, a result contradicted by empirical evidence.

In more recent work turnout is analyzed through the use of game theory; see Grofman (1979), Palfrey and Rosenthal (1983), Ledyard (1984). As the criticisms concerning Ferejohn and Fiorina hold for the Grofman contribution as well, only the latter two studies will be shortly discussed here. Palfrey and Rosenthal concentrate on the probability of being decisive in the traditional Downs framework. The general idea is that if one knows that it is rational for everyone to abstain, it becomes rational to vote. Therefore, the probability of being decisive and the turnout decisions are determined simultaneously in their model. A well-known result of their analysis is the counter-intuitive equilibrium where turnout rises with costs. Ledyard also concentrates on the probability of being decisive. In a spatial setting, he presents a kind of general equilibrium analysis concerning all voters. The most important equilibrium he obtains, however, is characterized by zero turnout. Therefore, his analysis does not seem to solve the classic voting paradox.

An important aspect of the Palfrey and Rosenthal paper is that they distinguish groups of voters in their analysis. However, their solution provides each individual in a group ('team') with a (same) probability of voting, and not with a definite choice either way. Groups of voters have also been distinguished by Morton (1987, 1991) and Uhlaner (1989). These authors conclude that positive turnout may be optimal for the group as a whole. Neither Morton, nor Uhlaner can ex-

plain how individual members of a group can be withheld from free-riding on the participation of other group members.

A central role is also allocated to groups in the model to be presented in this paper, which provides a theoretical underpinning for, and an extension to the Riker and Ordeshook model. Nevertheless, it is the individual vote decision that will be focused upon. Point of departure is that, in the end, the decision to vote is an individual decision. However, social groups play a major role in determining this decision (see Wolfinger and Rosenstone, 1980, Jaarsma, Schram and Van Winden, 1986, or Schram, 1991 for empirical justification of this assertion). It will be argued that the interaction between the social (reference) group one belongs to and the other groups in society, as well as the interpersonal relationships within one's social group affect the vote decision. In short: the individual decision to vote or abstain is influenced by inter- as well as intra-group relations. It will be shown, it is hoped, that the resulting model is an important extension to the Riker and Ordeshook (1968) type of cost/benefit analysis of voter turnout.

The intra-group relations affecting the vote decision are concerned with the development of social pressure within a group, meant to convince other members of the importance of casting a vote. It will be argued that it is only worthwhile for certain group members to take the trouble of putting pressure on other members of the group. These members will be named the 'producers of social pressure'. Their activity may be seen as a kind of 'campaign activity' in the Verba and Nie sense (see Rusk, 1976, for a review of this theory). Members induced by this pressure to cast a vote will be called the 'consumers of social pressure'. These terms will be explained and elaborated in subsequent sections. By taking account of the intra-group relationships the type of rationality introduced by Riker and Ordeshook (i.e., the fulfillment of a sense of civic duty) is justified for the consumers. For this group of individuals, the model only aims at bringing the understanding of the processes involved one step further. The problem why consumers attach utility to giving in to pressure will not be addressed explicitly in the model. One can easily think of economic psychological arguments why people would give in to such pressure, however (e.g., think of the maintenance of social relationships). For a further discussion, see section 7.

Whilst the voting paradox is not completely solved for the consumers (though the analysis is -hopefully brought one step further), the model does show that casting a vote may be a rational act (in the traditional, instrumental, sense) for the other group of individuals in the model, the producers of social pressure. This is elaborated in sections 3 and 7.

Two major assumptions are made in the model. *First* of all, it is assumed that in the political sphere, purely individual interests do not count. Because there are more individuals than parties in a given society, parties represent the interests of groups of voters. In other words, the political influence of individuals is typically small. Assuming that voters are aware of this fact, they will look for other voters

with similar interests, and relate to these interests in the political sphere. For example, a worker in the private sector will not expect her or his personal income to be taken account of by any party or politician, but may -if income is an important factor in her or his utility function- look for a party that promises to take good care of the wages in the private sector. Thus, this first assumption states that the interests pursued in the political sphere are group-specific and not purely individual.

The *second* major assumption is that the political influence of a social group is proportional to its vote share. This assumption is probably more in line with the situation of proportional representation in most European countries, than with the U.S. two-party, winner-takes-all, system (see Stigler, 1972, however). However, it does not seem to be unduly restrictive: in Section 5.2 it will be shown that the present analysis may easily be adapted to cover the latter case. This second assumption justifies the group-wise analysis of turnout independent of party choice. Though in reality the relationship between group turnout and influence may not be as clear-cut as assumed in the model, the assumption is believed to be a justifiable approximation of real world tendencies, allowing for an independent analysis of turnout and party choice. This independent analysis is supported by empirical tests in Schram (1991), where support is found for the hypothesis that voters make their turnout and party choice decisions sequentially as opposed to simultaneously. In other words, voters first decide as to whether to cast a vote or not, and subsequently - in the former case - on what party to vote for.

The organization of this paper is as follows. In section 2, the optimal turnout behavior of a social group as a whole is described. Here, the inter-group relationships play a major role. Section 3 deals with the intra-group relationships, as far as the production of social pressure is concerned. The consumption of this pressure is discussed in section 4. Section 5 contains an elaboration of the model, and in section 6 an example is presented that illustrates the way the model works. Finally, in section 7 some noteworthy aspects of the model are discussed. A list of symbols used is added in appendix G in order to aid the reader when studying the model.

2. Optimal group behavior

Using the first major assumption of the previous section, we divide the N individuals in a society into $K (< N)$ groups with similar interests. For each group k ($= 1, \dots, K$) the optimal value of the turnout rate ϕ^k from the point of view of the group, will be derived. Groups are defined in such a way, that members agree on common interests, and therefore on the optimal level of ϕ^k . Denoting the number of individuals in group k by l^k , the number of voters in k by $e^k (\leq l^k)$, and optimal

values by an asterisk *, $\phi^{k*} = e^{k*}/l^k$ is sought. Optimality in this case is defined in terms of benefits and costs for (a representative member of) the group.

It is assumed that groups are defined by their common interests in the political sphere, which are assumed to be determined by the various types of income in society. One may think of wage incomes, transfer incomes, capital incomes, and the like, but income levels may play a role as well. For each group, a specific tax rate τ^k is introduced, which may be negative. By obtaining more political influence through a rise in relative turnout (our second major assumption in the previous section), individuals in group k may raise their future disposable income by effectuating a more favorable tax rate τ^k .

Assuming interests to be determined by incomes and political influence to be aimed at the tax rate would seem to restrict the applicability of the model. It is not as restrictive as it may seem at first sight, however. The assumption is mainly made to enable the use of a common measuring rod (i.e. money) when discussing interests and the effects of political influence of various groups. The analyses to be presented remain valid if the term 'utility' is substituted for 'income' and 'disutility' for the tax rate τ^k , as long as it is assumed that (dis)utility is measurable in some way.

Input, at this stage of the analysis, to each member of the group the same probability of going to cast a vote given the turnout rate ϕ^k , which seems reasonable as long as the optimal result for the group as a whole is concerned (the individual vote decision is extensively analyzed in sections 3 and 4). The expected cost of voting for a representative member of the group then becomes $\phi^k \cdot c^k$, where c^k denotes the costs made by a person in group k , who actually casts a vote. These costs only occur in election periods. Assuming constant prices, the index of which is normalized to equal 1, a representative individual obtains the following (expected) real disposable income $\bar{w}^k(t)$ in any period t in which elections are held:

$$\bar{w}^k(t) = (1 - \tau^k(t)) \cdot w^k(t) - \phi^k(t) \cdot c^k(t), \quad (1)$$

where w^k denotes the gross income of a representative member of group k .

As noted above, the benefits from voting lie in affecting the group specific tax rate. The higher the turnout of the group relative to other groups, the lower the tax rate will be after the election. More specifically:

$$\tau^k(t+j) = \tau^k(\theta^k(t)), \quad j = 1, \dots, T \quad (2)$$

$$\frac{\partial \tau^k}{\partial \theta^k}(t) < 0, \quad \frac{\partial^2 \tau^k}{(\partial \theta^k)^2}(t) \geq 0$$

$$\theta^k(t) \equiv \frac{e^k(t)}{e^{-k}(t) + e^k(t)}, \quad \theta^k(t) \in \left[0, \frac{l^k}{l^k + e^{-k}}\right],$$

where T denotes the number of periods between elections (the length of an election period), θ^k the number of voters in k relative to the total number of voters, e^k the total number of voters in groups other than k :

$$e^{-k} \equiv \sum_{j \neq k} e^j, \quad k = 1, \dots, K,$$

and t denotes an arbitrary election date. Note that all variables are taken time-dependent except the group size l^k , and the functional specification of τ^k . For the time being, it is assumed that $e^k > 0$. Note that the function τ^k is chosen such, that a rise in relative turnout diminishes taxes but at a non-increasing rate as θ^k rises. Keeping in mind that taxes have a negative impact on net (after tax) income (cf. eq. 1), eq. (2) simply states that an increase in the turnout of a group, relative to other groups, has a positive - but declining - marginal effect on net income for group members. Denoting expected future values of variables by a tilde, it is obtained:

$$\bar{w}^k(t+j) = [1 - \tau^k(\theta^k(t))] \cdot \bar{w}^k(t+j), \quad j = 1, \dots, T. \quad (3)$$

This equation describes how future net income is affected by tax rates determined by elections in period t . For simplicity's sake, it will be assumed that gross incomes are expected to remain constant:

$$\bar{w}^k(t+j) = w^k(t), \quad j = 1, \dots, T.$$

In eqs. (2) and (3) the variable T refers to one election period. Alternatively, the variable T may be seen as the number of periods the new tax rate is thought to hold by the group members.

For the time being, individuals are only supposed to derive utility from present and future income. From the perspective of the individuals in group k the optimum for the group as a whole is obtained by maximizing the sum of present and discounted (expected) future incomes. This leads to the following program, given the assumption that gross income is expected to remain constant:

$$\max_{e^k(t)} \left(\sum_{j=0}^T \left[\frac{p^j}{\sum_{l=0}^T p^l} \right] \cdot \bar{w}^k(t+j) \right)$$

which is equivalent to:

$$\begin{aligned} \max_{e^k(t)} & \{ (1-p') \cdot [1 - \tau^k(\theta^k(t-T))] \cdot w^k(t) + p' \cdot [1 - \tau^k(\theta^k(t))] \cdot w^k(t) - (1-p') \cdot \phi^k(t) \cdot c^k(t) \} \\ p' & \equiv \frac{p - p^{T+1}}{1 - p^{T+1}}, \end{aligned} \quad (4)$$

where $\theta^k(t-T)$ denotes the relative turnout of k in the previous election, and p stands for a time preference parameter ($0 \leq p \leq 1$). Eq. (4), describing the optimization problem in election period t , consists of three terms. The first represents net income in t , as determined by the previous election result in $t-T$. The second term describes the discounted net income over the forthcoming electoral term as a consequence of the election result in t . The third term describes the expected voting costs in t .

In Schram and van Winden (1991) it is shown that because τ^k is a convex function of $\theta^k(t)$ ($\partial^2 \tau^k / (\partial \theta^k)^2 \geq 0$), the expression between braces $\{.. \}$ in (4) is concave in $e^k(t)$, and a maximum is therefore found if the first derivative is equal to zero.

Denoting the value of $e^k(t)$, for which $\partial \{.. \} / \partial e^k(t) = 0$ by $\hat{e}^k(t)$ (in Schram and van Winden, 1991, it is shown that such an $\hat{e}^k(t)$ can be found), and taking account of the restriction

$$0 \leq e^k(t) \leq l^k, \quad (5)$$

it is obtained for the solution of program (4) subject to (5):

$$e^{k*}(t) = \begin{cases} 0, & \text{if } \hat{e}^k(t) < 0 \\ \hat{e}^k(t), & \text{if } 0 \leq \hat{e}^k(t) \leq l^k \\ l^k, & \text{if } \hat{e}^k(t) > l^k \end{cases} \quad (6)$$

As for the costs of voting, these are simply assumed to be a constant fraction γ of income:

$$c^k(t) = \gamma \cdot w^k(t), \quad \gamma \in [0, 1]. \quad (7)$$

It can now be derived (cf. Schram and van Winden, 1991) that:

$$e^{k*}(t) = e^{k*}(e^{-k}(t); l^k, T, p, \gamma). \quad (8)$$

The value of $\hat{e}^k(t)$, and through (6) the optimal turnout $e^{k*}(t)$ appears to depend on the turnout in other groups, as well as on group size and a number of voter characteristics, to wit: the future time-horizon, the time preference parameter, and the fraction of voting costs to income. These last three characteristics are here assumed to be equal across groups, but the analyzes would also hold if they were allowed to vary over groups. As for the turnout in other groups, either an expected value can be assumed with which the optimal turnout in k can be derived, or the optimal turnout in all groups can be derived simultaneously. An example of the first procedure is presented in Section 6. We now discuss the second procedure.

To determine the optimal turnout under the assumption of a simultaneous determination of optimal decisions in all groups, eq. (8) is extended into the following set of equations:

$$\begin{aligned} e^{1*}(t) &= e^{1*}(e^2(t), \dots, e^K(t); l^1, T, p, \gamma) \\ &\vdots \\ e^{k*}(t) &= e^{k*}(e^1(t), \dots, e^{k-1}(t), e^{k+1}(t), \dots, e^K(t); l^k, T, p, \gamma) \\ &\vdots \\ e^{K*}(t) &= e^{K*}(e^1(t), \dots, e^{K-1}(t); l^K, T, p, \gamma) . \end{aligned} \quad (9)$$

Using the vector notation $\underline{e}^*(\cdot) = (e^{1*}(\cdot), \dots, e^{K*}(\cdot))'$, $\underline{e}(\cdot) = (e^1(\cdot), \dots, e^K(\cdot))'$, $\underline{l} = (l^1, \dots, l^K)'$, this set of equations can be written as:

$$\underline{e}^*(t) = \underline{e}^*(\underline{e}(t); \underline{l}, T, p, \gamma) . \quad (10)$$

For given values of \underline{l} , T , p , and γ , the solution to eqs. (9) and (10) is found for a fixed point of eqs. (10). The fixed point theorem of Brouwer is not applicable, because e^{k*} is not defined in the point where $e^k=0$, $\forall k$. In many cases, however, a fixed point may be shown to exist (e.g., this is the case when linear tax functions τ^k are assumed, as in the example in section 6). It should be noted that the optimal turnout then achieved corresponds with the Nash equilibrium for the non-cooperative game defined by (9). The optimal turnout for the group, either determined using expectations concerning other groups' turnout, or simultaneously solved (if possible) may very well involve values larger than zero as the examples in section 6 show. An example where optimal group turnout is derived, and compared to individual-level optima, is presented in Schram and Sonnemans (1992), where experiments are used to establish the relative importance of both.

It has now been established that from the point of view of a representative individual in a group (or, alternatively, for the group as a whole) a positive turnout for the group may be profitable. This conclusion is also reached by Morton (1987, 1991), for example. However, Morton then makes the assumption that somehow the group members would be given incentives to actually go and cast a vote, and

went on to investigate party behavior. However, even if the optimal turnout is 100%, it is still not rational for a voter to cast a vote if the marginal increase in utility obtained by this act is outweighed by the individual costs. It is, therefore, now time to turn to individual voting behavior.

3. Individual turnout I: producers of social pressure

Even though a voter may be aware of the optimal turnout determined in Section 2 for the group (s)he belongs to, the final vote decision is taken to be dependent on the individual costs and benefits derived from voting. The optimal turnout for the group may, however, play a role in this decision, as we hope to show below. As a point of departure, it is assumed that an individual i of a group k knows the optimal group-turnout $e^{k*}(t)$, and has some expectation regarding the number of people in the group that will cast a vote if (s)he abstains. The latter is denoted by $\tilde{e}_{no}^{ki}(t)$, with $0 \leq \tilde{e}_{no}^{ki}(t) \leq l^k$. Furthermore, an individual is supposed to have some idea as to the expected turnout in other groups, $\tilde{e}^k(t)$ (for simplicity's sake this expectation is assumed to be equal for all members of k).

The expected relative turnout of the group if the individual abstains now equals:

$$\tilde{\theta}_{no}^{ki}(t) \equiv \frac{\tilde{e}_{no}^{ki}(t)}{\tilde{e}_{no}^{ki}(t) + \tilde{e}^{-k}(t)} .$$

If the turnout of the group in the previous period $e^k(t-T)$ was equal to or larger than the expected optimal turnout for the group in the present period, it seems plausible to assume that the voter will not be able to provide convincing arguments to other people in the group to go and cast a vote whilst referring to group interests. Note that it is in i 's personal interest that as many group members as possible cast a vote - each additional vote diminishes the tax rate, *ceteris paribus* -, but this need not be in the interest of the group as a whole, as was shown in the previous section. We shall return to this point shortly. Let us now assume that the individual can credibly predict a shortage of votes compared with the optimal group turnout by referring to the turnout at the previous elections, i.e.,

$$\Delta \tilde{\phi}^k(t) \equiv \phi^k(t-T) - \phi^{k*}(t) < 0 ,$$

(and, consequently,

$$\Delta \tilde{\theta}^k(t) \equiv \theta^k(t-T) - \tilde{\theta}^{k*}(t) < 0 , \text{ if } \tilde{e}^{-k}(t) \geq e^{-k}(t-T) ,$$

which will be assumed from here onward).

The individual may then consider whether additional members of the group can be induced to vote. The social pressure used to that purpose can be anything from trying to convince family members of the importance of voting, to campaigning among one's fellow workers in a factory, or exploiting one's reputation as a national opinion leader, for example. The way in which pressure is exerted is an interesting topic that deserves further research, but it goes beyond the scope of the present paper. Whatever the type of pressure involved, its credibility will be assumed here to demand that an individual will have to vote her- or himself. In this context, it can be noted that in most countries the media pay much attention to the voting behavior of opinion leaders at elections.

It is also important to note that the individual motivation for persuading group members to vote lies in the wish to maximize group turnout in order to minimize the tax rate. If the turnout in the previous period exceeds the group optimal level, an income maximizing individual would still want as many members as possible to vote, but in that case (s)he -assumedly- cannot credibly motivate this by referring to group interests (an opinion leader cannot tell people to go and vote because this is in her or his purely personal interest, for example). On the other hand, there is certainly no motivation for an individual to try to dissuade group members from voting. Therefore, it will be assumed that in case of a previous turnout exceeding the group optimum, an individual will, on the one hand, not be able to persuade other members to vote, but, on the other hand, refrain from trying to dissuade them to do so.

Of course, not every individual will be equally capable of persuading other people, or, as we shall call it, of producing social pressure. Therefore, individuals will be characterized by their so-called '(opinion-)leadership ability', represented by the variable a^{ki} (for convenience restricted to the zero-one interval). This concept may be compared with the concept 'entrepreneurial ability' used by, for instance, Kanbur (1979) in modeling the decision to become either an entrepreneur or an employee. In this context the reader is referred to Riker and Ordeshook (1973, p. 76-77), where it is noted that "An essential element of leadership, ... is the ability to get others to make choices they otherwise would not make", and where the importance of political entrepreneurship for the vote decision is discussed. Riker and Ordeshook restrict political entrepreneurship to candidates and parties, however, whereas here a broader view of the matter is taken. A more detailed elaboration of this concept and its relationship to other personal characteristics is not presented here, but, obviously, the economic psychological literature will have something to say on the matter. It should also be noted that attention is focused on an instrumental motivation for producing pressure, in the present analysis. Other possible motivations (e.g., prestige) are not taken into consideration.

The capability of convincing other people not only depends on the above mentioned leadership-ability, but -as discussed above- also on the value $\Delta\tilde{\theta}^k(t) \equiv \theta^k(t-T) - \tilde{\theta}^{k*}(t)$.

$\tilde{\theta}^{k*}(t) (<0)$. As this gap between the relative turnout in the previous election and the optimal relative group turnout for the present election becomes larger, the individual may be expected to be more convincing in making a case. Finally, the extent to which a social pressure producer convinces other group members would seem to depend on the effort put into its production, as measured by the number of hours involved as a fraction of the total number of hours available, for example. This fraction will be denoted by α^{ki} . Let the function f indicate the maximum number of people expected to be convinced by the social pressure produced by the individual. The number of people i expects to persuade, $\tilde{f}^{ki}(t)$, equals:

$$\tilde{f}^{ki}(t) = \min \{l^k - \tilde{e}_{no}^{ki}(t), f^k(a^{ki}(t), \Delta\tilde{\theta}^k(t), \alpha^{ki}(t))\} \quad (11)$$

$$\alpha^{ki}(t) \in [0, 1], a^{ki}(t) \in [0, 1],$$

where:

$$\Delta\tilde{\theta}^k(t) \equiv \theta^k(t-T) - \tilde{\theta}^{k*}(t)$$

$$\frac{\partial f^k}{\partial a^{ki}}(t) > 0$$

$$\frac{\partial f^k}{\partial (\Delta\tilde{\theta}^k)}(t) \leq 0, \Delta\tilde{\theta}^k(t) \geq 0 \Rightarrow f^k(a^{ki}(t), \Delta\tilde{\theta}^k(t), \alpha^{ki}(t)) = 1$$

$$\frac{\partial f^k}{\partial \alpha^{ki}}(t) > 0, \frac{\partial^2 f^k}{(\partial \alpha^{ki})^2}(t) < 0, f^k(a^{ki}(t), \Delta\tilde{\theta}^k(t), 0) = 1.$$

Note that the situation where $\Delta\tilde{\theta}^k(t) \geq 0$ has been included by assuming that the only person that i can convince in this case is her- or himself. The same holds for the case where the individual does not take any trouble to produce social pressure ($\alpha^{ki}(t)=0$). The signs of the partial derivatives of f with respect to $\alpha^{ki}(t)$, furthermore, indicate a decreasing positive marginal effect of the effort put into pressure. Finally, (11) expresses that in any case no more than the number of group members who are otherwise expected to abstain ($l^k - \tilde{e}_{no}^{ki}(t)$) can be expected to be persuaded.

Because the values of $a^{ki}(t)$ and $\Delta\tilde{\theta}^k(t)$ are given, the first decision an individual has to make concerns the effort (number of hours) to be put into the production of social pressure. In the present context, this depends on the expected costs and benefits. The costs of producing social pressure are assumed to be an increasing function q of effort and income:

$$q^{ki}(t) = q^k(\alpha^{ki}(t), w^{ki}(t))$$

$$\frac{\partial q^k}{\partial w^{ki}}(t) > 0$$

$$\frac{\partial q^k}{\partial \alpha^{ki}}(t) > 0, \quad \frac{\partial^2 q^k}{(\partial \alpha^{ki})^2} \geq 0$$

$$q^k(0, w^{ki}(t)) = 0.$$

The benefits of pressure are determined by the expected lower tax rate in future periods due to a relatively higher turnout of the group. Define:

$$\bar{\theta}_{yes}^{ki} \equiv \frac{\bar{e}_{no}^{ki}(t) + \bar{f}^{ki}(t)}{\bar{e}_{no}^{ki}(t) + \bar{f}^{ki}(t) + \bar{e}^{-k}(t)} \quad (12)$$

where $\bar{\theta}_{yes}^{ki}(t)$ denotes the relative turnout of the group as expected by individual i , if (s)he votes.

It is important to note at this stage, that the model assumes that individual producers believe that if they do not succeed in convincing $\bar{f}^{ki}(t)$ members to vote at the elections in period t , these votes will be lost for the group, i.e., no other producers are expected to convince them. This excludes the possibility of strategic behavior (i.e., free riding) among producers. In section 7 it will be argued that the group of producers may be divided into small 'inclusive' subgroups (Olson, 1965), where free riding incentives are small or absent and individuals may be assumed to make their production decisions independently.

If i decides not to vote (and, consequently, not to produce social pressure) the expected benefit, in terms of discounted disposable income, for i equals (cf. (4)):

$$\bar{V}_{no}^{ki}(t) = (1-p') \cdot [1 - \tau^k(\bar{\theta}^k(t-T))] \cdot w^{ki}(t) + p' \cdot [1 - \tau^k(\bar{\theta}_{no}^{ki}(t))] \cdot w^{ki}(t),$$

where $\bar{V}_{no}^{ki}(t)$ denotes the expected benefit for i if (s)he does not vote.

On the other hand, if this individual decides to vote, and possibly to produce pressure, the expected benefit, in that case denoted by $\bar{V}_{yes}^{ki}(t)$, follows from:

$$\begin{aligned} \bar{V}_{yes}^{ki}(t) &= (1-p') \cdot [1 - \tau^k(\bar{\theta}^k(t-T))] \cdot w^{ki}(t) + p' \cdot [1 - \tau^k(\bar{\theta}_{yes}^{ki}(t))] \cdot w^{ki}(t) + \\ &\quad - (1-p') \cdot [\gamma \cdot w^{ki}(t) + q^k(\alpha^{ki}(t), w^{ki}(t))] , \end{aligned} \quad (14)$$

where account is taken of the two kinds of costs which are then involved, i.e., the costs of voting as such and the costs of producing pressure (which are zero if $\alpha^{ki}(t)=0$). Note that the differences between eqs. (13) and (14) are these two kinds of costs, and the fact that the tax rate for future periods in (14) depends on voting and pressure by the producer, whereas it does not in (13).

For the expected net benefit $\bar{V}^{ki}(t)$ it is obtained:

$$\begin{aligned} \bar{V}^{ki}(t) &= \bar{V}_{yes}^{ki}(t) - \bar{V}_{no}^{ki}(t) \\ &= p' \cdot [\tau^k(\bar{\theta}_{no}^{ki}(t)) - \tau^k(\bar{\theta}_{yes}^{ki}(t))] \cdot w^{ki}(t) - (1-p') \cdot [\gamma \cdot w^{ki}(t) + q^k(\alpha^{ki}(t), w^{ki}(t))] . \end{aligned} \quad (15)$$

Note that $\bar{V}^{ki}(t)$ is a function of $\alpha^{ki}(t)$ through q^k and $\bar{\theta}_{yes}^{ki}$ (cf. eqs. (11) and (12)). This function is, for convenience, denoted by V^{ki} , thus, $\bar{V}^{ki}(t) \equiv V^{ki}(\alpha^{ki}(t))$. However, this (continuous) function need not be differentiable in each point $\alpha^{ki}(t)$, because of the minimum condition in eq. (11). From here onward, we shall assume this differentiability (because no producer expects to be able to convince all expected abstainers, for example), but this is only done to keep the mathematics as simple as possible.

Using the assumptions made so far concerning τ^k , $\bar{\theta}^{ki}(t)$ and f^k it can be shown that

$$\frac{\partial^2 V^{ki}}{(\partial \alpha^{ki})^2}(t) \leq 0 ,$$

and the maximizing value of $\alpha^{ki}(t)$ is that value for which

$$\frac{\partial V^{ki}}{\partial \alpha^{ki}}(t) = 0 ,$$

(see Schram and van Winden, 1991) if this value exists for $\alpha^{ki}(t) \in [0,1]$. From here on, such an interior solution will be assumed to exist, but it is noted that otherwise one of the corner solutions $\alpha^{ki}(t)=0$ or $\alpha^{ki}(t)=1$ is the maximizing value. Let the thus found effort level be denoted by $\hat{\alpha}^{ki}(t)$. It renders the maximum net benefit an individual in k can achieve by voting and producing social pressure. However, to do so the individual must cast a vote her- or himself, thereby making the costs $\gamma \cdot w^{ki}(t)$. If the maximum net benefit is not sufficient to cover these turnout costs, the individual -at this stage- is better off abstaining. The optimal effort (fraction of time spent), $\alpha^{k*}(t)$, is therefore given by:

$$\alpha^{ki*}(t) = \begin{cases} \hat{\alpha}^{ki}(t), & \text{if } \tilde{V}^{ki}(\hat{\alpha}^{ki}(t)) \geq 0 \\ 0, & \text{if } \tilde{V}^{ki}(\hat{\alpha}^{ki}(t)) < 0 \end{cases} \quad (16)$$

$$\hat{\alpha}^{ki}(t) \equiv \arg \max V^{ki}(\alpha^{ki}(t)) ,$$

where it is assumed that i will actually produce the optimal amount of pressure if the expected net benefit exactly equals zero.

It should be clear that the value of $\alpha^{ki*}(t)$ depends on the personal characteristics of individual i , to wit: her or his leadership-ability $a^{ki}(t)$, income $w^{ki}(t)$, and expectations concerning the turnout in case (s)he abstains $\tilde{e}_{no}^{ki}(t)$. Furthermore, through $\Delta \tilde{\theta}^{ki}(t)$, $\alpha^{ki*}(t)$ depends on the group turnout in the previous election and on the optimal group turnout. Finally, it is assumed that income is independent of leadership-ability.

As for the expectations $\tilde{e}_{no}^{ki}(t)$, the case where these are independent of $a^{ki}(t)$ and that where:

$$\tilde{e}_{no}^{ki}(t) = \tilde{e}_{no}^{ki}(a^{ki}(t)) , \text{ with } \frac{\partial \tilde{e}_{no}^{ki}}{\partial a^{ki}}(t) \leq 0, \forall i ,$$

are distinguished. The latter case represents the idea that people with a higher ability may expect group turnout to be more dependent on their social pressure.

The following theorem relates the production of social pressure to the ability $a^{ki}(t)$:

Theorem 1

$$\text{Let: } \begin{cases} a^{ki}(t) = 0 \Rightarrow V^{ki}(\hat{\alpha}^{ki}(t)) < 0, \\ a^{ki}(t) = 1 \Rightarrow V^{ki}(\hat{\alpha}^{ki}(t)) > 0; \end{cases}$$

Furthermore, let: $\tilde{e}_{no}^{ki}(t) = \tilde{e}_{no}^{ki}(a^{ki}(t))$,

$$\text{where } \frac{\partial \tilde{e}_{no}^{ki}}{\partial a^{ki}}(t) \leq 0, \forall i .$$

Then, for every income level $w^{ki}(t)$ there exists some ability level $a^{kp}(w^{ki}(t), \tilde{e}_{no}^{ki}(t)) \in (0, 1)$ for which:

$$a^{ki}(t) \underset{(\pm)}{>} a^{kp}(w^{ki}(t), \tilde{e}_{no}^{ki}(t)) \Leftrightarrow V^{ki}(\hat{\alpha}^{ki}(t)) \underset{(\pm)}{>} 0 . \quad (17)$$

Moreover, if pressure costs $q^{ki}(t)$ are a linear function of $w^{ki}(t)$ (with $q^{ki}(t)=0$, if $w^{ki}(t)=0$), then a^{kp} is independent of $w^{ki}(t)$, and every individual in group k with $a^{ki}(t)$ larger than or equal to $a^{kp}(\tilde{e}_{no}^{ki}(t))$ will produce social pressure. ■

The proof of this theorem is presented in Schram and van Winden (1991). Note that the case where expectations are independent of ability is included ($(\partial \tilde{e}_{no}^{ki} / \partial a^{ki})(t) = 0, \forall i$). The theorem states that - for given income and expectations - a borderline value of leadership ability will exist. Any individual with an ability above (below) this borderline will (will not) find it worthwhile to exert pressure on others to vote. In essence, the conditions started out with in the theorem imply that the benefits from adding one vote are outweighed by the costs, whereas a maximum amount of ability allows a sufficient number of people to be convinced to make pressure production always profitable. Note that the latter case requires that $a^{ki}(t)=1$ provides an expectation $\tilde{e}_{no}^{ki}(t)$ that is sufficiently smaller than 1^k to allow an interior solution in eq. (11). This seems to be a reasonable assumption, because it implies that potential producers expect turnout to be substantially less than 100% if they do not act. Furthermore, the assumption that costs are a linear function of income seems reasonable if indeed α^{ki} is seen as the fraction of time spent on social pressure production.

An example of the production of social pressure and the theorem is presented in section 6.

Theorem 1 enables a distinction of the producers of social pressure in a group by the personal characteristics of income, expectation regarding group turnout if one abstains, and leadership-ability. Note that the differences in turnout rates between social groups may, thus, be due to differences in the distribution of these factors in various groups. Interestingly, income figures as one of the important variables in the (empirical) literature on voter turnout (see Wolfinger and Rosenstone, 1980, Jaarsma et al., 1986, Schram, 1991). In any case, the effect of these variables will depend on the way in which non-producers are induced to vote by the producers of pressure, the theme to which we now turn.

4. Individual turnout II: consumers of social pressure

Having discussed the decision to produce social pressure (and, thus, the decision to vote on these grounds), we now turn to the 'consumption' of social pressure, which essentially means taking account of the interests of the producers of pressure by voting. In particular, attention is focused on the behavior of the group of non-producing individuals. The latter may be induced to vote by the social pressure they are confronted with within the group. Besides the utility derived from income, it is assumed that individuals also expect to derive positive utility from giving in to social pressure or, put differently (in terms more familiar to students

of voter turnout), from the fulfillment of a sense of 'civic duty' generated by the production of social pressure. The utility attributed to this fulfillment is thought to depend on five factors:

- 1 the total amount of effort (number of hours) put into the production of social pressure. It seems likely that the utility derived from giving in to social pressure increases with the amount of social pressure produced;
- 2 the leadership-ability of the producers. The higher this ability, the greater the (potential) effect of a given amount of effort will be;
- 3 the (relative) group turnout in the previous period minus the optimal relative turnout for the group: $\Delta\tilde{\theta}^k(t) = \theta^k(t-T) - \tilde{\theta}^{k*}(t)$. The more negative $\Delta\tilde{\theta}^k(t)$, the more credible social pressure becomes;
- 4 a 'spill-over' effect of social pressure of previous periods. It is assumed that the total amount of social pressure that is felt within a group only partially adjusts to the pressure actually produced in a given period (due to such psychological phenomena as 'internalization' or 'habit formation');
- 5 the characteristics of the individual consumer. Though a new variable may be introduced to capture this effect, we have chosen to let the leadership ability, a^{ki} , do the job. It is assumed that the individual becomes more sensitive to social pressure (attaches more utility to its consumption), the lower her or his ability is, compared to that of the producers. This assumption is in line with psychological theories on 'direct influence' and 'behavioral contagion' (see Lippitt, Polansky, Redl and Rosen, 1968, for example). These theories state that if individuals (which in Lippitt's case are children) are ranked on some 'power'-scale (determined by, i.a., liking, attributed power, projected popularity) the larger the difference between two individuals on the scale is, then the more likely it will be that the lower ranked person will copy behavior of the higher ranked individual as a response to direct influence attempts by the latter.

The first four factors indicate that some level of social pressure may exist within a group in a given period, independent of the fact whether anyone gives in to it. Individual differences through personal characteristics occur because of the fifth factor. Because it is assumed that positive utility is derived from giving in to social pressure -to be labeled the 'consumption' of social pressure- it will not alter the (positive) vote decision of producers. Consequently, only the decision of non-producers needs to be investigated here.

The fourth factor mentioned above (the 'spill-over' effect) is formalized as follows:

$$sp^k(t) = \beta^k \cdot \Gamma^k(t) + (1 - \beta^k) \cdot sp^k(t-T), \quad \beta^k \in [0, 1], \quad (18)$$

where $sp^k(t)$ denotes the total amount of social pressure in k in period t , $\Gamma^k(t)$ the total production of social pressure in k in period t , and β^k a partial adjustment co-

efficient. Eq. (18) states that a fraction $1 - \beta^k$ of the social pressure existing in a group in the previous election is carried over to the present election. If this pressure is seen as representing a type of 'social norm' in the group, this equation states that the social norm is not completely renewed in each election, but adjusted as a result of reinforcement, on the one hand, and norm decay on the other.

Let $PP^k(t)$ denote the set of social pressure producers in k in period t :

$$PP^k(t) \equiv \{i \in k : \alpha^{ki*}(t) > 0\} \quad (19)$$

The total amount of current social pressure production is dependent on the hours spent on production by the various producers (factor 1, above), on their ability (factor 2), and on the gap between previous and optimal group turnout, $\Delta\tilde{\theta}^k(t)$ (factor 3). It is assumed that the (production-)function g^k that will be used to describe this dependence is equal across producers in k , and time-independent. More specifically, it is assumed:

$$\Gamma^k(t) = \sum_{i \in PP^k(t)} g^k(a^{ki}(t), \Delta\tilde{\theta}^k(t), \alpha^{ki*}(t)) \quad (20)$$

$$\frac{\partial \Gamma^k}{\partial a^{ki}}(t) > 0, \quad \frac{\partial \Gamma^k}{\partial \Delta\tilde{\theta}^k}(t) \leq 0, \quad \frac{\partial \Gamma^k}{\partial \alpha^{ki*}}(t) > 0.$$

It is noted that the function $\tilde{f}^{ki}(t) = f^k(a^{ki}(t), \Delta\tilde{\theta}^k(t), \alpha^{ki*}(t))$, indicating the number of people that i expects to persuade, may be taken for g^k . Furthermore, from theorem 1 it follows that if pressure costs are a linear function of $w^{ki}(t)$, $\alpha^{ki*}(t)$ is independent of $w^{ki}(t)$.

From factor 5 above (referring to the personal characteristics of the consumer of social pressure), it follows that the utility derived from giving in to (consuming) social pressure will depend on an individual's own leadership-ability as well as the abilities of the producers. The latter are summarized by the average ability, $a_p^k(t)$, in this set of individuals:

$$a_p^k(t) \equiv \frac{\sum_{i \in PP^k(t)} a^{ki}(t)}{|PP^k(t)|},$$

where $|\cdot|$ denotes the number of elements in a set.

Note that $a_p^k(t) \geq \min\{a^{kp}(w^{ki}(t), \tilde{e}_{no}^{ki}(t)), i \in PP^k(t)\}$ holds (see theorem 1). The difference between individual ability and $a_p^k(t)$ is denoted by $a_d^{ki}(t)$:

$$a_d^{ki}(t) \equiv \max\{a_p^k(t) - a^{ki}(t), 0\}.$$

Denoting the (expected) utility derived from giving in to social pressure (the fulfillment of 'civic duty') by CD^{ki} , it is assumed that:

$$CD^{ki}(t) = h^k(a_d^{ki}(t), sp^k(t)) \geq 0, \quad (21)$$

$$\frac{\partial h^k}{\partial a_d^{ki}}(t) > 0, \quad \frac{\partial h^k}{\partial sp^k}(t) > 0.$$

The restrictions on (the utility function) h^k follow from the factors 1 to 5 mentioned above. They state that the utility attributed to giving in to pressure is positive, that the utility is greatest for individuals with the lowest abilities in comparison with the producers and declines with rising ability, and that the utility rises with the level of social pressure in the group.

The individual decision to vote or abstain can now be described as follows, using eqs. (15) and (16):

$$i \text{ of } k \text{ will vote} \Leftrightarrow \tilde{V}^{ki}(\alpha^{ki*}(t)) + CD^{ki}(t) \geq 0. \quad (22)$$

Note that for producers of social pressure $V^{ki}(\cdot) \geq 0$ and that something non-negative is added to this term; thus, social pressure does not affect their vote decision. In section 7 it is shown, how the pressure may induce non-producers to produce pressure after all, if the two-step model of pressure production and vote decisions is extended to an n-step model.

The following theorem is complementary to theorem 1.

Theorem 2

Let the set of producers $PP^k(t)$ and the total amount of social pressure $sp^k(t)$ be given. Furthermore, let all non-producers (or a subset thereof) have the same expectation concerning turnout if they do not vote: $\tilde{e}_{no}^{ki}(t) = \tilde{e}_{no}^k(t)$, for all $i \in k - PP^k(t)$ (or a subset thereof). Finally, let $a^{ki}(t) = 0$ imply $CD^{ki}(t) = \infty$. Then, for every income level $w^{ki}(t)$, there exists a value $a^{kc}(w^{ki}(t))$, such that:

$$V^{ki}(0) + CD^{ki}(t) \geq 0 \Leftrightarrow a^{ki}(t) \leq a^{kc}(w^{ki}(t)), \quad (23)$$

$$a^{kc}(w^{ki}(t)) \in (0, a^{kp}(w^{ki}(t), \tilde{e}_{no}^k(t))].$$

Consequently, all individuals in the set $k - PP^k(t)$ (or the relevant subset thereof) with a leadership-ability $a^{ki}(t) \leq a^{kc}(w^{ki}(t))$ will vote.

Furthermore, for these individuals,

$$\frac{\partial a^{kc}}{\partial w^{ki}}(t) \leq 0.$$

For the proof of this theorem, the reader is referred to Schram and van Winden (1991).

The assumption that $a^{ki}(t) = 0$ implies $CD^{ki}(t) = \infty$ assures that a^{kc} is positive for any wage level. If this assumption is dropped, sufficiently large values of w will cause a^{kc} to become non-positive, implying that no level of ability a^{ki} will induce consumption (voting). Note that the theorem implies that in the special case where everyone has the same income and the same turnout expectation under abstention, all those with an ability below the level a^{kc} will vote because they are induced to do so by social pressure. Furthermore, everyone with an ability above a^{kp} will vote and produce social pressure. Only the group members with an ability between these two values will decide to abstain; production is not profitable for them, whilst their ability is high enough to withstand social pressure.

The results obtained imply that an individual will vote if and only if:

$$p' \cdot [\tau^k(\tilde{\theta}_{no}^{ki}(t)) - \tau^k(\tilde{\theta}_{yes}^{ki}(t))] \cdot w^{ki}(t) - (1-p') \cdot [c^{ki}(t) + q^{ki}(t)] + CD^{ki}(t) \geq 0,$$

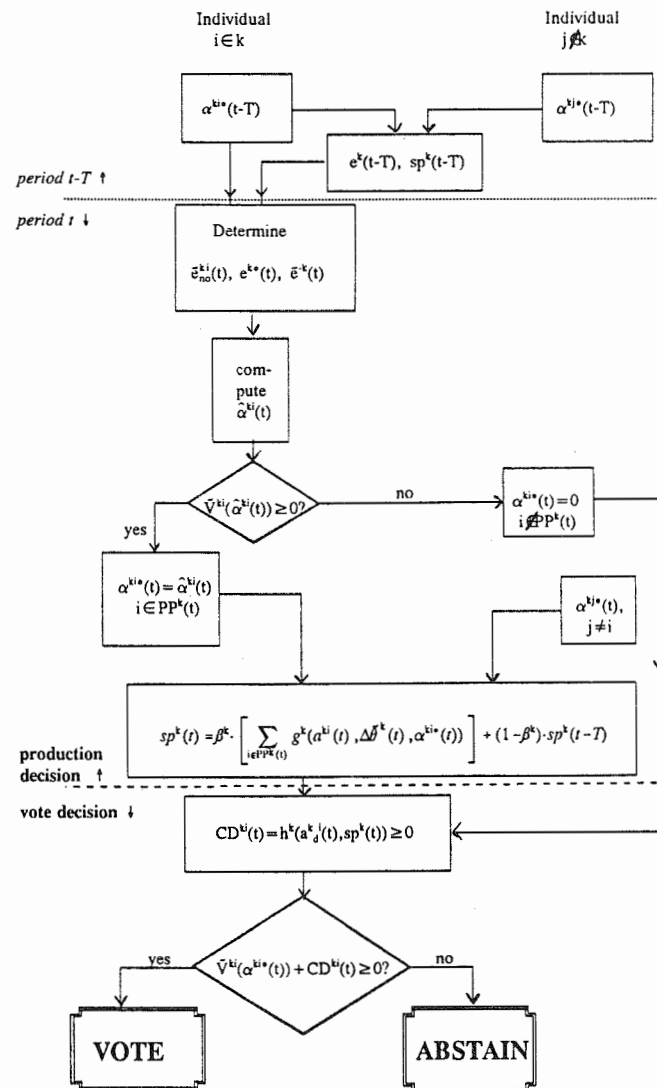
that is, in words, it should hold that

(net benefits of voting) - (costs of voting) + (benefits accruing from the fulfillment of civic duty) ≥ 0 .

This puts our model into the tradition of the Downs-Tullock-Riker and Ordeshook model (see Mueller, 1989, p. 121, or Riker and Ordeshook, 1968, eq. (7)). Interestingly, Tullock (1967, p. 114), also hinted at the importance of social pressure by observing that: "There may be social pressures that make it wise for the individual (to vote)". The crucial difference, however, is that the ad hoc nature of the introduction of 'civic duty' in the traditional model has been remedied by the endogenization of this aspect of social behavior. Though no explanation has been provided for the fact per se that individuals attach utility to giving in to social pressure, it is hoped that the processes leading to the sense of civic duty as described shed new light on the occurrence of this sense. The significance of group interests and processes in this respect has been stressed. Analytically, the decision to vote or abstain in our model may be seen as a two-step procedure: first there is the decision to produce social pressure or not, and then, possibly, the decision to vote due to the consumption of social pressure (for producers both steps have been assumed to coincide - see section 7, however).

An overview of the by now complete model is presented in figure 11.1.

Figure 11.1
The Complete Model



For an example of the consumption of social pressure, the reader is referred to section 6.

5. Elaboration of the model

5.1. Equilibrium analysis

In this section two types of equilibria will be defined for the model: temporary equilibria in a given period t and a dynamic equilibrium for the model as a whole. Both types of equilibrium are characterized by two features:

- 1 each individual behaves optimally, using the information available;
- 2 the resulting behavior of all individuals is jointly consistent in the sense that no individual is withheld from attaining her or his optimum by the actions of others.

In the model at hand, the latter feature requires that somehow the results of the production and consumption decisions are in correspondence with each other. For that purpose it is demanded that expectations regarding the group turnout are realized. Recalling that, by assumption, all individuals of a group k have the same expectations regarding the turnout of other groups, $\tilde{e}^k(t)$, and the same belief concerning the optimal turnout for the group, $e^{k*}(t)$, the actual turnout, $e^k(t)$, of the group depends on the following variables: the distribution of expectations $\tilde{e}_{no}^{k*}(t)$, abilities $\underline{a}^k(t)$, and incomes $\underline{w}^k(t)$ in the group, where

$$\begin{aligned} \underline{\tilde{e}}_{no}^k(t) &\equiv (\tilde{e}_{no}^{k1}(t), \dots, \tilde{e}_{no}^{k|k}(t))' \\ \underline{a}^k(t) &\equiv (a^{k1}(t), \dots, a^{k|k}(t))' \\ \underline{w}^k(t) &\equiv (w^{k1}(t), \dots, w^{k|k}(t))' \end{aligned}$$

First consider a temporary equilibrium for group k in period t . Let $S^k(t)$ denote the set of voters in k (note that the set of producers of social pressure is a subset of the set of voters: $PP^k(t) \subset S^k(t)$).

Definition 1

A weak temporary equilibrium for a group k in period t is a 'state' $(\tilde{e}_{no}^{k*}(t), \underline{a}^k(t), \underline{w}^k(t))$ that satisfies:

$$\tilde{V}^{ki}(t) + CD^{ki}(t) \geq 0, \forall i \in S^k(t); \quad (\text{TE1})$$

$$\tilde{V}^{kj}(t) + CD^{kj}(t) < 0, \forall j \notin S^k(t); \quad (\text{TE2})$$

$$\tilde{e}^{ki}(t) = \tilde{e}^{kj}(t) = e^k(t), \forall i \in S^k(t), \forall j \notin S^k(t), \quad (\text{TE3})$$

where:

$$\tilde{e}^{ki}(t) = \tilde{e}_{no}^{ki}(t) + f^k(a^{ki}(t), \Delta \tilde{\theta}^k(t), \alpha^{ki*}(t))$$

$$\tilde{e}^{kj}(t) = \tilde{e}_{no}^{kj}(t)$$

$$e^k(t) = e^k(\tilde{e}_{no}^k(t), \underline{a}^k(t), \underline{w}^k(t)).$$

A strong temporary equilibrium for a group k in period t is a 'state' $(\tilde{e}_{no}^k(t), \underline{a}^k(t), \underline{w}^k(t), \tilde{e}^k(t))$ which satisfies (TE1)-(TE3) as well as

$$\tilde{e}^k(t) = e^k(t). \quad (\text{TE4}) \blacksquare$$

Note that the conditions (TE1) and (TE2) demand that individuals attaching positive utility to the act of voting vote whereas those with a negative evaluation do not, thus referring to the characteristic of individual optimality mentioned above, whereas (TE3) and (TE4) demand that all individuals' expectations with regard to turnout are fulfilled, and thus concern joint consistency within a group and between groups, respectively. Furthermore, note that for $i \in S^k(t)$ -PP $^k(t)$, $\alpha^{ki*}(t)=0$, implying $\tilde{e}^{ki}(t)=\tilde{e}_{no}^{ki}(t)+1$.

In line with the definition above, a temporary equilibrium for the model as a whole may be defined as a state where each group is in a strong temporary equilibrium.

The following theorem discusses the existence of a weak temporary equilibrium using the function $\alpha^{ki*}(t) = \alpha^{ki*}(\tilde{e}_{no}^{ki}(t))$ (for which it can be shown that the first derivative is negative).

Theorem 3

Suppose there exist distributions of $\tilde{e}_{no}^{ki}(t)$, $a^{ki}(t)$, and $w^{ki}(t)$, satisfying $a^{ki}(t) < 1$ for all $i \in \text{PP}^k(t)$, which determine a group turnout $e^k(t)$ such that for all $i \in \text{PP}^k(t)$ substituting the value 0 for $\tilde{e}_{no}^{ki}(t)$ implies $\tilde{e}^{ki}(t) \leq e^k(t)$. Furthermore, let the conditions of theorem 1 hold.

Then, there exists a state $(\tilde{e}_{no}^k(t), \underline{a}^k(t), \underline{w}^k(t))$ that satisfies the equilibrium conditions (TE1)-(TE3), and yields turnout $e^k(t)$. \blacksquare

Again, the proof of the theorem is given in Schram and van Winden (1991).

The conditions in this theorem, concerning producers' expectations, does not seem to be unduly restrictive as long as $e^k(t)$ is not too small, because it only demands that producers do not believe they can convince more than $e^k(t)$ individuals to cast a vote whilst expecting zero turnout if they abstain. Note that this means that states with a high turnout rate are most likely to be found in equilibrium. To be more precise, the theorem says that any turnout level that fulfills the aforementioned condition and can be supported by a state $(\tilde{e}_{no}^k(t), \underline{a}^k(t), \underline{w}^k(t))$ is feasible as an equilibrium turnout level.

An example of this theorem is provided in section 6.

Now we turn to a dynamic equilibrium analysis of the model. First of all, stationarity of the model is defined as constancy of all variables of the model over time. Next, a dynamic equilibrium for the model is defined:

Definition 2

A dynamic equilibrium for the model is obtained if the model is in a temporary equilibrium in each period t . \blacksquare

The behavior of the model over time is determined by the time paths of $\tilde{e}_{no}^k(t)$, $\underline{a}^k(t)$, $\underline{w}^k(t)$, $\tilde{e}^k(t)$, $\Delta \tilde{\theta}^k(t)$, and $sp^k(t)$ (with $\Delta \tilde{\theta}^k(t)$ a function of $e^k(t-T)$ and $\tilde{e}^k(t)$, and $sp^k(t)$ a function of $sp^k(t-T)$ and the other variables), for all k . As regards the expectation formation processes underlying $\tilde{e}_{no}^{ki}(t)$ and $\tilde{e}^k(t)$ it will only be assumed that these expectations are determined by the results in the previous period (election) if this period produced a temporary equilibrium for the model, implying that expectations were realized. More specifically, if a temporary equilibrium for the model occurred at $t-T$, period t yields the following expectations:

$$\begin{cases} \tilde{e}_{no}^{ki}(t) = e^k(t-T) - \tilde{f}^{ki}(t-T), i \in S^k(t-T) \\ \tilde{e}_{no}^{kj}(t) = e^k(t-T), j \notin S^k(t-T) \\ \tilde{e}^k(t) = e^k(t-T) \end{cases} \quad (24)$$

For simplicity's sake, abilities and incomes will be assumed to be constant. The following theorem (see Schram and van Winden, 1991, for its proof) discusses the existence of a dynamic equilibrium and relates dynamic equilibria to stationarity.

Theorem 4

Let expectations adjust as described in eqs. (24).

- 1 If the conditions of theorem 3 are fulfilled for each group $k=1, \dots, K$, there exists a dynamic equilibrium for the model.
- 2 If the model is in a dynamic equilibrium it is in a stationary state. \blacksquare

This concludes the brief discussion of various types of equilibria that have been defined and investigated for the model. For the model presented an equilibrium is compatible with a positive turnout level. In fact, within the framework of the model zero turnout would be an extreme case, since it demands that no social pressure is produced. The various theorems discussed and the example of section 6 show that the forces generated in the model may be compatible with one another, potentially providing a positive turnout.

5.2. Winner-takes-all elections

We return to the issue of proportional representation versus winner-takes-all situations. In the latter case the winner of an election determines all policies in the forthcoming election term. Even if it is assumed that parties (or candidates) take the preferences of their constituencies into account when determining their policies, proportional representation is only obtained for the voters of the winning party. This case of one party ('partial') proportional representation will be distinguished from the case where party policies are exogenously determined.

Consider a setting with, for simplicity, two groups (1 and 2) and two parties (A and B) to which a representative member of group k attaches utilities U_A^{ki} and U_B^{ki} , $i \in k=1,2$, in case policies are exogenous. For notational convenience, the time index is dropped. If the utility attached to a party is solely determined by the group-specific tax rate it may vary within a group according to income, but every member agrees as to which party is to be preferred. For example, if party A and B offer group 1 a tax rate τ_A^1 and τ_B^1 , respectively, where $\tau_A^1 < \tau_B^1$, then everyone in group 1 is assumed to prefer party A to B, even though the utility attributed to party A within group 1 may vary with income.

In case party preference varies within the group (in the exogenous case each individual prefers the party with the highest U^{ki}), a producer may still profit from a high turnout within her or his group, if the relative preference for the party that (s)he prefers is higher within the group than outside of it. For example, if 60% of the voters in group 1 and 40% of the voters in group 2 prefer party A, then a producer preferring A will profit from a high relative turnout in group 1, in spite of the fact that 40% of the votes in this group will be for B.

In case of partial proportionality the relative number of voters of group k for the winning party, denoted by

$$\theta_w^k \equiv \frac{e_w^k}{e_w^k + e_w^{-k}}, \quad w = A, B,$$

where e_w^k represents the number of votes cast by members of k for the winning party $w=A, B$, determines policies. In this case the model of the previous section is

adapted by assuming that the tax rate is determined by this rate: $\tau^k = \tau^k(\theta_w^k)$, cf. eq. (2).

First, the exogenous policy case is investigated in case every group member prefers the same party. Given the expected turnout of the other group (which may be defined stochastically), an optimal group turnout exists which equals zero when it is sure that more members of the other group will vote than the number of members in the group concerned (in that case the election can never be won, and no benefits can be obtained by voting). Examples of the derivation of this optimal value may be found in Palfrey and Rosenthal (1983), Morton (1987, 1991), or Schram and Sonnemans (1992). If group 1 prefers party A and group 2 prefers B, the expected benefit from voting for member i of 1 equals:

$$p' \cdot [Pr_A(\tilde{\theta}_{yes}^{1i}) - Pr_A(\tilde{\theta}_{no}^{1i})] \cdot (U_A^{1i} - U_B^{1i}) - (1-p') \cdot (c^{1i} + q^{1i}), \quad (25)$$

where Pr_A denotes the probability that group 1 is decisive in the election (and, therefore, party A wins the election). The decisiveness is stochastic if the various expectations are uncertain; in case of certainty, the term between brackets [...] in eq. (25) is 1 if $\theta_{yes}^{ki} > 0.5$ and $\theta_{no}^{ki} < 0.5$ (neglecting the possibility of a tie), and 0 otherwise. In any event, Pr may be seen as a function of the expected relative turnout in the group, which is dependent on the number of members one expects to be able to induce to vote. This may give rise to social pressure production. It should, therefore, be clear that the analyzes in the previous sections may straightforwardly be extended to this case.

In case the group members disagree as to the most preferred party, a producer preferring party A will only decide to produce pressure, if the term between brackets [...] in eq. (25) is positive, i.e., if the probability that A wins the election rises as group turnout rises, which is the case if the relative preference for A is greater within the group than in other groups. Of course, the case where a majority of the group prefers a different party than the producer will be rare: in that case the latter can be expected to reconsider her or his reference group.

In case of partial proportionality, the expected benefit is dependent on the fact whether an individual believes (s)he can influence the actual party choice of other individuals (a 'floating vote') or only the choice with respect to casting a vote or not. In the latter case, the expected benefit also depends on the distribution of votes over A and B within the group. An individual in k contemplating the production of social pressure is then faced with the following expected benefits:

$$p' \cdot [Pr_A \cdot [\tau^k(\tilde{\theta}_{A_{no}}^{ki}) - \tau^k(\tilde{\theta}_{A_{yes}}^{ki})] + (1 - Pr_A) \cdot [\tau^k(\tilde{\theta}_{B_{no}}^{ki}) - \tau^k(\tilde{\theta}_{B_{yes}}^{ki})]] \cdot w^k - (1-p') \cdot (c^{ki} + q^{ki}). \quad (26)$$

In case the individual party choice is not affected, Pr_A rises (declines) with $\theta_{yes}^{ki} - \theta_{no}^{ki}$ if the relative preference for A over B is greater (smaller) within group k than outside of it. In case a producer can induce members to vote for party A, Pr_A is an

increasing function of $\theta_{yes}^{ki} - \theta_{no}^{ki}$. In both cases, social pressure production might prove profitable and the general framework of intra-group relationships as described in the model may be retained.

All in all, it appears that the model presented may easily be adapted if the assumption concerning proportional representation is changed to the winner-takes-all situation, where various assumptions concerning the latter may be made.

6. An example

For this example, a setting is chosen with K groups of 50,000 voters each. An election term consists of three periods and a period consists of 2000 working hours. The costs of casting a vote are equal to one hour's wage, so $\gamma = 1/2000 = 0.0005$. The preference for current income relative to the sum of discounted future income over the forthcoming election term is 0.3. The tax rate is assumed to be a linear function of the relative group turnout. Therefore:

$$\left[\begin{array}{l} l^1 = \dots = l^K = 50,000 \\ T = 3 \\ \gamma = 0.0005 \\ p' = 0.7 \ (\Rightarrow p \approx 0.9) \\ \tau^k(t) = \tau_1^k \cdot \theta^k(t) + \tau_0^k, \ \tau_1^k < 0 \\ \theta^k(t) \in \left[0, \frac{50,000}{50,000 + e^{-k}(t)} \right] \end{array} \right.$$

Note that the specification for the tax function implies that an increase in relative turnout by one percentage point decreases the tax rate by $0.01 \cdot \tau_1^k$. This decrease applies to 50,000 people for the forthcoming election term. Given the relative preference for future incomes of 0.7, the total net benefit for the group obtained from a one percentage point increase in relative turnout equals $-0.7 \cdot 50,000 \cdot 0.01 \cdot \tau_1^k \cdot w^k = -350 \cdot \tau_1^k \cdot w^k$ where w^k is the average wage rate in the group for one complete period of 2000 working hours. Needless to say, this benefit can become very large.

Optimal group behavior.

Define:

$$A^k \equiv \left[\frac{1-p}{p-p^T+1} \right] \cdot \left(\frac{\gamma}{l^k} \right) \approx 4.29 \cdot 10^{-9}$$

$$B^k \equiv \frac{-\tau_1^k}{A^k} > 0.$$

First we shall look at the case where an expected value for the turnout in other groups is used for determining the optimal turnout in group k . Assume that 40,000 individuals in each other group are assumed to cast a vote: $e^{-k} = (K-1) \cdot 40,000$. The optimal solution described by eq. (8) then equals:

$$e^k(t) = -40,000 \cdot (K-1) + \sqrt{40,000 \cdot (K-1) \cdot B^k}. \quad (E1)$$

Now assume $\tau_1^k = -0.0008$ (in which case the total net benefit of a percentage point increase in relative turnout is 0.28 times the average wage rate, or the average wage of 560 hours of work). For two groups ($K=2$), (E1) yields an optimal turnout of 46,410.88 (92.8%). For $K=5$ the group optimum is equal to 33,218.36 (66.4%).

Next assume that the optimal turnout rates in various groups are determined simultaneously, using eqs. (10). Assuming an interior solution with $e^{k*}(t) = \hat{e}^k(t)$ (cf. (6)), the set of equations (9) then becomes:

$$e^{k*}(t) = -e^{-k}(t) + \sqrt{B^k \cdot e^{-k}(t)}, \quad k = 1, \dots, K. \quad (E2)$$

Because the model is not defined for $e^k = e^{-k} = 0$, this solution to the set is not a solution for the model. Denoting total turnout by E , this set may be written as:

$$(E(t))^2 = B^k \cdot (E(t) - e^k(t)), \quad k = 1, \dots, K,$$

and because $E \neq 0$, this yields:

$$E(t) = B^k \cdot (1 - \theta^k(t)), \quad k = 1, \dots, K,$$

a linear set of equations yielding the (unique) solution:

$$\begin{aligned} e^k(t) &= D^k \cdot \left[\sum_{j=1}^K \left(\prod_{f=1, f \neq j}^K B^f \right) - (K-1) \cdot \prod_{f=1, f \neq k}^K B^f \right] \\ D^k &\equiv \frac{(K-1) \cdot \prod_{j=1}^K B^j}{\left[\sum_{j=1}^K \left(\prod_{f=1, f \neq j}^K B^f \right) \right]^2} \end{aligned} \quad (E3)$$

If for some k the interior solution does not exist, the relevant corner solution must be inserted and the set must be resolved in order to obtain the optimal solutions $e^{k*}(t)$. The resulting solution may concern positive optimal turnout, depending on the relationship between costs and benefits of voting for various groups, as represented by B^k .

From these equations direct insights are difficult to derive, of course. If groups are identical, (E3) simplifies substantially, however, because B is identical across groups. For $K=2$, the optimal turnout in each group in that case turns out to be $B/4$. For $K=5$, (E2) yields $\hat{e}^k(t) = (4/25)B$.

For $\tau_1^k = -0.0008$, the case of two groups yields an optimal turnout of 46,667 (93.3%), whereas for $K=5$ the optimum is 29,867 (59.7%).

Producers of social pressure. To focus attention on the intra-group relationships, a situation for inter-group relationships is chosen that yields an optimal group turnout of 100%. As can be derived from (E3), $\tau_1^k = -0.05$ yields this solution for $K=2$, if both groups are identical (in which case $B^1 = B^2$, yielding $\hat{e}^1(t) = 0.25 \cdot B^1 \approx 0.25 \cdot 0.05 \cdot 10^9 / 4.29 \gg 50,000$). For this value for τ_1^k a percentage point increase in relative turnout provides a net benefit for a group as a whole of 17.5 times the average yearly wage rate. For an individual in the group, the net benefit of this increase is equal to $-0.7 \cdot 0.01 \cdot (-0.05) \cdot w^{ki}(t) = 0.00035$ times her or his annual wage (which is equal to about 7 hours' wage). The wage is assumed to be equal to 1 for all members of the group.

It is further assumed that there are two identical groups and that group members expect the other group to behave optimally (and, therefore, have a turnout of 100%). For the time being, it is assumed that every group member expects 30,000 members of the group to cast a vote if (s)he abstains. Finally, in the previous election the turnout in the group was 30,000 (60%), whereas the turnout in the other group was 40,000 (80%). Summarizing for group k :

$$\begin{aligned} e^{k*}(t) &= e^{-k*}(t) = \bar{e}^{-k}(t) = 50,000 \quad (\Rightarrow \theta^{k*}(t) = \frac{1}{2}) \\ \bar{e}_{no}^{ki}(t) &= 30,000, \quad \forall i \in k \\ e^k(t-T) &= 30,000, \quad e^{-k}(t-T) = 40,000, \quad (\Rightarrow \theta^k(t-T) = \frac{3}{7}) \\ \Delta \theta(t) &= \theta^k(t-T) - \theta^{k*}(t) = -\frac{1}{14} \\ \tau_1^k &= -0.05, \quad \tau_0^k = 0 \\ w^{ki}(t) &= 1, \quad \forall i \in k \end{aligned}$$

Next, the functions concerning the costs of social pressure production (q^k) and the number of group members one expects to convince (\hat{f}^k) need to be specified. For the former, it is assumed that putting an hour's effort into pressure production costs one tenth of an hour's wage (because of on the job contacts, for example, the opportunity costs of producing pressure are less than an hour's wage). For \hat{f}^k , a function conform eq. (11) needs to be chosen. For simplicity the function is chosen in such a way, that an individual with maximum leadership-ability ($a^{ki}(t)=1$), who spends all of her or his time (2000 hours) on production ($\alpha^{ki}(t)=1$), expects to convince 20,000 members to go and vote, which is exactly equal to $1^k - e_{no}^{ki}$, the number of voters (s)he expects will abstain. Therefore, an interior solution to (11) is assured, and the minimum condition may be dropped. More specifically, the following functions are chosen:

$$\begin{aligned} q^k &= 0.1 \cdot \alpha^{ki}(t) \\ \hat{f}^{ki}(t) &= f^k(a^{ki}(t), \Delta \theta^k(t), \alpha^{ki}(t)) \\ &= 1 - 6261 \cdot a^{ki}(t) \cdot \min\{\Delta \theta^k(t), 0\} \cdot \sqrt{2000 \cdot \alpha^{ki}(t)} \\ &= 1 + 6261 \cdot \left(\frac{1}{14}\right) \cdot \sqrt{2000 \cdot a^{ki}(t) \cdot \alpha^{ki}(t)} \end{aligned} \quad (E4)$$

From the assumptions made the expected net benefits, as represented by eq. (15), can be derived:

$$\begin{aligned} \tilde{V}^{ki}(t) &= 0.7 \cdot (-0.05) \cdot \left[\frac{30,000}{80,000} - \frac{30,000 + 1 + 6261 \cdot \left(\frac{1}{14}\right) \cdot \sqrt{2000 \cdot a^{ki}(t) \cdot \alpha^{ki}(t)}}{80,000 + 1 + 6261 \cdot \left(\frac{1}{14}\right) \cdot \sqrt{2000 \cdot a^{ki}(t) \cdot \alpha^{ki}(t)}} \right] + \\ &\quad - 0.3 \cdot [0.0005 + 0.1 \cdot \alpha^{ki}(t)] \end{aligned} \quad (E5)$$

For these net benefits, the optimal amount of pressure can be derived for various values of $a^{ki}(t)$. Then, one can check whether this optimum yields a positive net

benefit (otherwise, the voter would be better off at this stage by abstaining, and obtaining a net benefit of 0). For the values chosen, it is found that:

$$\bar{V}^{ki}(t) \geq 0 \Rightarrow a^{ki}(t) \geq 0.80 \Rightarrow a^{kp}(t) = 0.8,$$

(cf. theorem 1). Therefore, every individual with an ability greater than or equal to 0.8 will decide to produce social pressure, whereas those with a lower ability will decide not to. The corresponding optimal $\hat{\alpha}^{ki}(t)$'s vary from $3.49 \cdot 10^{-3}$ (6.97 hours) for $a^{ki}(t)=0.8$ to $4.26 \cdot 10^{-3}$ (8.52 hours) for $a^{ki}(t)=1$. For these values, the expected number of people convinced ($\bar{f}^{ki}(t)$ in eq. (E4)) varies from 946 to 1307.

Next we assume that there are 10 individuals in the group with $a^{ki}(t)=0.9$, and all others have abilities less than 0.8. Therefore, there will be 10 producers of social pressure in k . For these producers, the amount of effort put into production and the expected number of members convinced can be derived. We find:

$$PP^k(t) = \{i: a^{ki}(t) \geq 0.8\} = \{i: a^{ki}(t) = 0.9\}$$

$$\alpha^{ki*}(t) \approx 3.90 \cdot 10^{-3} \quad (7.79 \text{ hours}), \quad i \in PP^k(t)$$

$$\bar{f}^{ki}(t) \approx 1124, \quad i \in PP^k(t).$$

Thus, each of the 10 producers expects that her or his 7.79 hours of social pressure will induce 1124 members to go and cast a vote.

Consumers of social pressure. For the consumption of the social pressure produced, the total amount of social pressure and the utility attributed to giving in to it need to be determined.

For the production function (eq. (20)), a form similar to eq. (E4) is chosen:

$$\begin{aligned} g^k(a^{ki}(t), \Delta \bar{\theta}^k(t), \alpha^{ki*}(t)) &= -a^{ki}(t) \cdot \Delta \bar{\theta}^k(t) \cdot \sqrt{2000 \cdot \alpha^{ki*}(t)} \\ &= \left(\frac{1}{14} \right) \sqrt{2000 \cdot a^{ki}(t) \cdot \sqrt{\alpha^{ki*}(t)}}, \quad i \in PP^k(t) \end{aligned}$$

$$\Gamma^k(t) = \sum_{i \in PP^k(t)} g^k(a^{ki}(t), \Delta \bar{\theta}^k(t), \alpha^{ki*}(t)) = 10 \cdot \left(\frac{1}{14} \right) \sqrt{2000 \cdot 0.9 \cdot \sqrt{3.9 \cdot 10^{-3}}} \approx 1.79,$$

where account has been taken of the fact that all of the ten producers have ability 0.9.

The production of social pressure in period t adds to the total amount of social pressure that is carried over from the previous election according to a partial adjustment process (cf. the spill-over effect discussed in point 4 of section 4). The

partial adjustment parameter is assumed to be equal to 0.8. In the previous election a social pressure equal to 1 is assumed to have existed:

$$sp^k(t-T) = 1$$

$$sp^k(t) = 0.8 \cdot sp^k(t-T) + 0.2 \cdot \Gamma^k(t) \approx 1.16.$$

Finally, the expected utility derived from giving in to this pressure (the fulfillment of 'civic duty') is specified in accordance with the function h in eq. (21):

$$CD^{ki}(t) = \frac{(0.9 - a^{ki}(t)) \cdot sp^k(t)}{3000}, \quad i \notin PP^k(t).$$

Using $\bar{V}^{ki}(\alpha^{ki}(t)=0) = -1.5 \cdot 10^{-4}$ (cf. eq. (E5)), for the group of non-producers it is obtained that (cf. eq. (22) and theorem 2):

$$i \text{ of } k \text{ will vote} \Leftrightarrow -1.5 \cdot 10^{-4} + \frac{(0.9 - a^{ki}(t)) \cdot 1.16}{3000} \geq 0 \Leftrightarrow a^{ki}(t) \leq 0.51.$$

In this example, anyone with an ability higher than or equal to 0.8 will vote and produce pressure, anybody with an ability lower than or equal to 0.51 will vote out of a sense of civic duty, and anybody with an ability between 0.51 and 0.8 will abstain.

Equilibrium. Finally, a temporary equilibrium for this example is derived (cf. definition 1 and theorem 3). Besides the 10 individuals with $a^{ki}(t)=0.9$, we assume that the non-producers have either $a^{ki}(t)=0.25$ (consumers of social pressure) or $a^{ki}(t)=0.60$ (abstainers). We maintain the assumption that for the producers $e_{no}^{ki}(t)=30,000$. For the consumers, it is assumed that $e_{no}^{ki}(t)=31,123$ and for the abstainers that $e_{no}^{ki}(t)=31,124$. It should be noted, however, that for neither of the latter two groups the change in $e_{no}^{ki}(t)$ will alter their decision not to produce social pressure. These expectations in case of abstention yield the following expectations for group turnout:

$$\begin{aligned} a^{ki}(t) = 0.25 &\Rightarrow \bar{e}^{ki}(t) = e_{no}^{ki}(t) + 1 = 31,124 \quad (i \in S^k(t) - PP^k(t)) \\ a^{ki}(t) = 0.60 &\Rightarrow \bar{e}^{ki}(t) = e_{no}^{ki}(t) = 31,124 \quad (i \notin S^k(t)) \\ a^{ki}(t) = 0.90 &\Rightarrow \bar{e}^{ki}(t) = e_{no}^{ki}(t) + \bar{f}^{ki}(t) = 31,124 \quad (i \in PP^k(t)). \end{aligned}$$

Now assume that besides the 10 producers of pressure, there are 31,114 individuals with $a^{ki}(t)=0.25$ and 18,876 with $a^{ki}(t)=0.6$. The turnout in the group will then be equal to 31,124 (62.2%) and therefore all expectations concerning turnout are fulfilled:

$$\tilde{e}^{ki}(t) = e^k(t) = 31,124, \forall i \in k.$$

An equilibrium is obtained with positive turnout. The equilibrium described is clearly a weak equilibrium for group k (cf. definition 1). A strong equilibrium is obtained if the turnout in the other group turns out to be the expected 100%. In that case, because all expectations are fulfilled, there is no need to adapt these for the next elections (*ceteris paribus*).

7. Concluding discussion

To start this concluding section two important aspects of the model will be discussed that illustrate the shortcomings and achievements of the model, to wit the meaning of the civic duty concept and the interaction between producers of social pressure.

With regard to the former, it was stressed that the model does not claim to explain precisely why individuals attach utility to giving in to social pressure. The aim of the model is to show where the feeling of civic duty originates: from influence attempts by other individuals, referring to common interests. The investigation of the reason(s) why utility is attached to giving in to this pressure is a matter of (economic) psychology which goes beyond the scope of this paper. It is comparable to the micro-micro problem discussed by Leibenstein (1979), who actually discusses the role of 'external pressure' as a determinant of deviations from (traditional) individual optimal behavior. In some cases the incentives to give in to social pressure may be provided by the possibility of sanctions after an election. If opinion leaders can find out who voted and who did not, policies by an elected party may be used to benefit those that did. In the United States, for example, the matter of who voted is a public record. This opens the possibility for sanctions within social groups. One can easily think of other arguments.

At any rate, in our view the description and understanding of the civic duty concept has been brought one step further. In this regard the importance of (socio-economic) groups with common interests is once again stressed. If all individuals were to have common interests in society, it would not really matter who controls

government because everyone's interests are taken account of. It is hard to see why any civic duty concept would come to be in this case, for there is no reason for anybody to try to convince other individuals of the importance of voting. On the other hand, if only purely individual interests were at stake one would never profit from the votes of other individuals (on the contrary, they would diminish one's own -minuscule- influence) and one would certainly not try to persuade others into voting. Once again, it is not clear how any feeling of civic duty would come to be in such a case. Therefore, it is believed that a sense of civic duty as often assumed in the calculus of voting can only be credible in case of common group interests.

As for the interaction between producers of social pressure, one might wonder if there are no free-riding incentives for the various producers. If an individual thinks others will produce a sufficient amount of pressure, (s)he may refrain from doing so. This expectation is reflected in the variable $\tilde{e}_{no}^{ki}(t)$. In the model as presented, the production decision is taken independent of the consumption of social pressure. A considerable number of group members must be persuaded to make production profitable. It may be expected that only for a few opinion leaders (with high abilities a^{ki}) this will be the case, so the group of producers will be small, and every producer's individual contribution may be expected to be considerable. Because the good to be obtained by these producers (a lower tax rate) is 'inclusive', i.e., its supply does not decline when a person 'consumes' it, free-riding incentives may be small or even absent for this group (see Olson, 1965).

The two-step decision process of the model is, of course, a very simplified version of reality. The model may be extended to n steps, however, where in every step the non-producers take the pressure produced in the previous $n-1$ steps into account and then once again decide whether to produce pressure or not. In that case the benefits derived from persuading people to vote need not fully compensate the sunk costs of casting a vote to make voting profitable. These costs are (partly) compensated by the fact that utility is attached to giving in to the social pressure produced in the previous $n-1$ steps. In other words, an optimal effort $\tilde{\alpha}^{ki}(t)$ that was previously not sufficient to provide a positive net benefit to voting and producing pressure (cf. eqs. (15), (16)), might prove worthwhile in case the benefits are updated by taking into account the utility attributed to giving in to the pressure produced in previous steps.

Formally, the set of producers, $PP^k(t)$, the total amount of social pressure produced, $\Gamma^k(t)$, and the difference between individual ability and the average ability of the producers, $\tilde{a}^i(t)$, are adapted in each step n , which is denoted by adding a subscript n (note that all steps are assumed to take place in the same election period t). The group of producers and the amount of pressure produced in each step is determined, in a snowball fashion, as follows:

$$\begin{aligned}
 PP_1^k(t) &= \{i: V^{ki}(\hat{\alpha}^{ki}(t)) \geq 0\} \\
 \Gamma_1^k(t) &= \sum_{i \in PP_1^k(t)} g^k(a^{ki}(t), \Delta \tilde{\theta}^k(t), \alpha^{ki*}(t)) \\
 sp_1^k(t) &= \beta^k \cdot \Gamma_1^k(t) + (1 - \beta^k) \cdot sp_1^k(t-T) \\
 &\vdots \\
 PP_n^k(t) &= \{i: V^{ki}(\hat{\alpha}^{ki}(t)) + h^k(a_{d(n-1)}^{ki}(t), sp_{n-1}^k(t)) \geq 0\} \\
 \Gamma_n^k(t) &= \sum_{i \in PP_n^k(t)} g^k(a^{ki}(t), \Delta \tilde{\theta}^k(t), \alpha^{ki*}(t)) \\
 sp_n^k(t) &= \beta^k \cdot \Gamma_n^k(t) + (1 - \beta^k) \cdot sp_n^k(t-T)
 \end{aligned} \tag{27}$$

Note that in this structure, every individual ends up producing pressure for whom $V^{ki}(\hat{\alpha}^{ki}(t)) + h^k(a_{d(n-1)}^{ki}(t), sp_{n-1}^k(t)) \geq 0$ holds. As the number of steps grows, the number of producers, and the amount of social pressure increases (more precisely, does not decrease), and the number of people needed to be convinced to make production profitable decreases. Of the individuals for whom $V^{ki}(\hat{\alpha}^{ki}(t)) + h^k(a_{d(n-1)}^{ki}(t), sp_{n-1}^k(t)) < 0$, those with $V^{ki}(0) + h^k(a_{d(n-1)}^{ki}(t), sp_{n-1}^k(t)) \geq 0$ will cast a vote (out of a sense of civic duty) without producing pressure, and the others will abstain.

It is noted that in this set-up, producers indirectly try to induce group members in 'lower' steps to produce pressure. They do so by making the (sunk) costs of casting a vote easier to overcome by compensating (part of) them through the sense of civic duty. An interesting extension of the model would be to let pressure have a direct impact on the pressure production of others by having the costs of the production compensated by the social rewards of giving in to 'pressure to produce pressure'. Here we are only dealing with the compensation of the costs of casting a vote. An optimal level of $\hat{\alpha}^{ki}(t)$ that is not sufficient to overcome the sunk costs in the two-step model (because $V^{ki}(\hat{\alpha}^{ki}(t)) < 0$) may yield positive net benefits if positive utility is derived from giving in to pressure produced by other producers.

Olson (1965, p. 60) also discusses the role of social pressure in encouraging group members to contribute to a group goal. The process described in eqs. (27) indicates that if opinion leaders may be assumed to foresee this tree of social relationships, i.e., if they expect their message to be 'passed on', the assumed large number of people expected to be convinced by them in step 1 may be quite realistic. Furthermore, because in each step individuals relate their behavior to that of others in their reference group only, strategic behavior (free riding) is again not very likely. All in all, the two-step procedure assumed in the model is just a very simplified description of the processes believed to exist in reality. The discussion

above is meant to show that the assumed behavior of producers of social pressure may be more realistic than one might perhaps think at first sight.

This concludes the discussion of the strengths and weaknesses of the model. It is hoped that a solution of the as yet unresolved problem of positive voter turnout in large-scale elections is brought nearer by bringing into the picture the importance of social groups with specific common interests, social pressure and (opinion) leadership abilities, showing the possibility of the endogenization of 'civic duty' in a model of voter turnout.

Some empirical support for the model is provided by history. In most democratic societies, suffrage was extended group by group. The pressure developing among the working classes in many European countries at the beginning of the century, urging people to go and vote, is a typical example of the process described in the model of this paper. Other than this, empirical evidence is (as yet) hard to obtain for this model. For some of the variables used, data cannot be collected easily (expectations and abilities, for example, are not measurable in a straightforward way). In Schram (1991) the empirical literature relating voter turnout to individual characteristics is reviewed in the light of the model presented here. Some support for the ideas underlying the model is found.

An alternative way to investigate the validity of the model is by experimentation. One can try to simulate an election in a laboratory setting to see whether the processes described in the model occur. This type of research has recently been started (Schram and Sonnemans, 1992). Though the specific role of opinion leaders has not yet been investigated, preliminary experimental results show that pre-play communication helps move behavior from individual-level optima to group-level optima. This supports the distinction made in this paper. A next step will be an investigation into the structure of the communication, to establish whether the production and consumption of pressure takes place as described.

List of Symbols

~ (tilde)	: expected value of a variable
^ (hat)	: maximizing value of a variable
* (star)	: optimal value of a variable

subscripts:

no	: situation where the individual abstains
yes	: situation where the individual votes

superscripts:

- k : group k
 -k : all groups $j \neq k$
 i : individual i

function(value)s/parameters/variables

- a : opinion-leadership ability
 a^p : lower bound for a in group of producers
 a^c : upper bound for a in group of consumers
 a_p : average ability of producers
 a_d : difference between personal ability and a_p
 c : costs of casting a vote
 e : number of voters
 f : number of group members one believes one can convince to cast a vote
 g : individual production function of social pressure
 h : individual consumption function of social pressure
 l : number of individuals
 p, p' : time preference parameters
 q : cost function of social pressure production
 sp : total amount of social pressure
 t : time index for period t
 \bar{w} : real disposable income
 w : real gross income

 CD : utility attached to giving in to pressure
 K : number of groups
 N : total number of individuals
 PP : set of producers
 S : set of voters
 T : time index for length of an election period
 V : benefits

 α : effort put into the production of social pressure
 β : partial adjustment parameter
 γ : fraction of voting costs to income
 θ : turnout relative to total turnout

- τ : tax rate function
 φ : turnout rate

 Γ : total amount of social pressure produced in one period
 Δ : difference between realized value of a variable at previous election and optimal value for the present election

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Chapter 12

How to Avoid Intrapersonal Strategic Conflicts in Game Theory

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Summary

If the same economic agent has to decide at different stages, (s)he may well suffer from her own behavior. This is demonstrated with the help of durable monopoly markets where – in spite of the monopolistic market structure – one may have fierce price competition. Since game theory is not prepared at all for such intrapersonal decision conflicts, it should transform intrapersonal into interpersonal decision conflicts by relying on local players, e.g. by analyzing games in agent normal form. Experimental results, however, indicate that behaviorally such an extreme position is incorrect: human decision makers often are committed by their previous behavior. The conflict between normative or prescriptive game theory and the behavioral theory of game playing, as part of economic psychology, is further illustrated by outside option games to which forward induction is applicable, principal agent models with incomplete information, and repeated games. Throughout the paper we refer to experimental results to contrast normative game theory with real decision making.

Key words

Game theory, experimental economics, durable monopolies, principal agent models, repeated games, behaviorism, forward induction

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Introduction

The main motivation for this study is to explore whether there can be a strategic conflict within the same economic agent deciding at different stages when playing a strategic game. A strategic game is played by at least two independent agents, called players, whose objectives are at least partially contradicting. So what a player will win in a strategic conflict does not only depend on his own decisions, but also on the behavior of his coplayers who try to achieve other goals. Thus every player in a strategic game is confronted with an interpersonal strategic conflict. Usually a player has not only to decide once but several times when playing the game. Of course, a player will always try to maximize his expected utility. But at later stages some aspects cannot be changed anymore whereas he still can influence other determinants of his expected utility. Although the utility function has not changed, the motivation of the same player may therefore depend crucially on the state of the play, i.e. a player may not only face an interpersonal, but also an intrapersonal strategic conflict when playing a strategic game.

Since game theory has been especially designed for resolving interpersonal conflicts, all strategic conflicts should be analyzed as strategic interaction of independent decision makers, i.e. game theory should not allow for intrapersonal strategic conflicts but avoid them. The obvious way for doing so is to transform intrapersonal strategic conflicts into interpersonal ones by relying on an appropriate notion of a player. Whereas more centralized concepts of players, who choose more than just one move, allow for intrapersonal strategic conflicts, purely local players, who decide about a single move, will never encounter such problems. The possibility of intrapersonal strategic conflicts is therefore closely related to the problem of how to define a player in the sense of an independent decision maker (see Güth, 1991).

An extreme, but classical point of view (see von Neumann and Morgenstern, 1944) is that an economic actor has full control over all his future acts, i.e. he can prevent himself from reconsidering later what he intended to do in the future. We will refer to such decision makers as omnipotent players. There seems to be a lot of evidence that such an extreme assumption is empirically wrong. Human decision makers do not always trust their own promises as they do not trust others. But also the other extreme assumption of local players, i.e. that every single move is decided by an independent player, seems to be wrong from a psychological perspective. Human decisions are often influenced by self images and attempts to establish a certain reputation. Although from the normative point of view we strictly favor the notion of local players, it will be much more difficult to develop the behavioristic concept of a player. In our view this will become clearer only

gradually when the behavioral theory of game playing behavior is further developed (for special attempts see Güth, 1993, Selten and Kuon, 1993, and Brandts and Holt, 1993). The progress in understanding game playing behavior will advance the progress of economic psychology, in general, and, more specifically, of the theory of bounded rationality. It certainly would help if more psychologists concentrated on game playing paradigms.

In the following we will try to demonstrate by some examples that the possibility of intrapersonal strategic conflicts depends on the game form, i.e. the way of formally representing a game, as well as on the solution concept determining the individually rational decision behavior. By doing so we try to introduce some of the most important conceptual ideas of modern game theory.

Game theory assumes unlimited analytic and computational abilities so that costs and limitations of cognitive efforts and information processing can be neglected. A player is therefore indefinitely more clever than every human decision maker. Even the most superb master of chess is, for instance, relying on incomplete and probably often inadequate heuristics although chess, as a finite game with perfect information, is rather trivial from a game theoretic point of view. Here we do not confine our discussion of intrapersonal strategic conflicts to the extreme case of individually rational decision makers. Instead we also discuss the behavioral relevance of game theoretic concepts, often by considering experimental results. The results of ultimatum bargaining experiments demonstrate, for instance, that behavior is guided by norms which seem to apply more generally (see Roth, 1994, for the most recent survey).

Our main message is that, contrary to classical game theory, the concept of omnipotent players is normatively wrong since it allows for intrapersonal decision conflicts for which game theory is not well equipped. By assuming local players this problem can be avoided. Concerning the behavioral concept of a player we only indicate open problems and some lines for future research. Thus, this study is more an attempt to provoke research focusing on game playing behavior, both by economic psychologists and experimental economists, than an answer how people decide in strategic environments.

In Section II we consider the durable monopoly game, i.e. a monopolist who can sell his product in more than one market period. Forward induction is shown in Section III to allow for intrapersonal strategic conflicts and confronted with backward induction. A simple principal agent model is used in Section IV to discuss type players which are intermediate to omnipotent and local players. Section V introduces repeated games and how one has tried to justify cooperation of players in such games. A game form which transforms intrapersonal strategic conflicts into interpersonal strategic ones by relying on local players is introduced and illustrated in the final section VI.

The durable monopoly game

On a monopoly market there is only one seller. We assume the standard case of linear demand and constant marginal costs of production which can be normalized (by redefining the unit of quantity as well as the monetary unit) to the demand function

$$X(p) = 1 - p.$$

The price p is the unit profit, i.e. sales price minus constant marginal costs of production, and $X(p)$ is the sales amount at price p . Since $\int_p^1 dv = 1 - p$, one can justify the demand function $X(p)$ by assuming a continuum of consumers with redemption values v which are uniformly distributed on the interval $[0, 1]$. The redemption value of a consumer is the price at which he is indifferent between buying and not buying. It is assumed that every consumer wants to buy at most one unit. If the market were competitive, the sales price would have to equal the constant marginal costs, i.e. the unit profit would be 0. We will therefore say that $p = 0$ is the competitive price. The monopolist's profit

$$\Pi(p) = p(1-p)$$

is maximized by the monopoly price $p^* = 1/2$ which yields $\Pi(p^*) = 1/4$.

The game theoretic justification of the monopoly solution considers not only the monopolist, but also all consumers as players. It is natural to assume that first the monopolist has to state his price p and that, knowing p , then the consumers can decide whether they want to buy one unit or not. Clearly, at every price $p \geq 0$ only consumers with redemption values $v \geq p$ will want to buy whereas all others will refuse to buy. Anticipating this demand behavior yields the demand function $X(p)$ and the profit function $\Pi(p)$ showing that the monopoly solution can be justified as the unique subgame perfect equilibrium point (Selten, 1975) of a simple game with a very natural sequential decision process.

In the durable monopoly game the monopolist does not determine a sales price once but repeatedly. Let $t = 1, 2, 3 \dots$ denote the sales period. In every sales period t the monopolist first states his periodic sales price p_t . Knowing p_t , the consumers then decide whether to buy in period t or not. As before it is assumed that every consumer buys at most one unit, i.e. in period t only those consumers who did not buy before can still buy one unit. We rely on time preferences in the form of constant discount factors. Let δ with $0 \leq \delta \leq 1$ denote the discount factor of all consumers, ρ with $0 \leq \rho \leq 1$ the discount factor of the monopolist, and π_t the profit which the monopolist earns in period t , i.e. π_t is the product of the price p_t and the amount sold in period t . The payoff of the monopolist in the durable monopoly game is

$$\Pi = \sum_t \rho^{t-1} \pi_t,$$

whereas it is $\delta^{t-1}(v-p)$ for a consumer with redemption value v who buys in period t and 0 if a consumer does not buy at all.

Clearly, if for all sales periods t the monopolist sets the monopoly price $p_t = 1/2$, all consumers with $v > 1/2$ will buy immediately so that $\pi = 1/4$, i.e. the monopolist will earn the full monopoly profit.

A game theoretic justification of such a sales policy would have to specify consumers' strategies such that the monopolist's strategy " $p_t = 1/2$ for all t " together with the consumers' strategies is an equilibrium point of the durable monopoly game. Here an equilibrium point is meant to be a strategy vector from which no single player can profitably deviate. Let us simply assume that consumers with $v > 1/2$ will only buy whenever $p_t \leq 1/2$ if they have not bought before and that all other consumers with $v \leq 1/2$ will buy only at a price $p_t = 0$ if they have not bought before.

Clearly, given these consumers' strategies the monopolist cannot earn more than $\Pi = 1/4$ by deviating – if he changes p_1 , he will earn less; if he changes prices p_t with $t \geq 2$, this does not change Π . Furthermore, given the constant sales price of $1/2$, no consumer can gain by changing his buying behavior. We thus have proved

Proposition II.1: The monopoly profit $\Pi = 1/4$ is an equilibrium outcome of the durable monopoly game regardless whether the number of possible sales periods is 1, 2, 3, ...

We want to show that Proposition II.1 describes a rather questionable result of the durable monopoly game if there are at least two sales periods t since it neglects the crucial intrapersonal strategic conflict of the monopolist. Let us assume that there are only two sales periods $t = 1$ and $t = 2$ with sales prices p_1 and p_2 , respectively. A consumer with redemption value v will buy in period 1 if $v > p_1$ and if either $p_2 \geq p_1$ or, in case of $p_1 > p_2$, if

$$v - p_1 > \delta(v - p_2)$$

where the latter inequality is equivalent to

$$v < (p_1 - \delta p_2) / (1 - \delta) = v^*.$$

Thus the sales amount X_1 of period 1 is

$$X_1 = \begin{cases} 1 - (p_1 - \delta p_2) / (1 - \delta) & \text{for } 1 - \delta + \delta p_2 > p_1 > p_2 \\ 0 & \text{otherwise.} \end{cases}$$

Here p_2^* is the price which the consumers expect for period 2. This yields the following demand function $X_2(p_2)$ for period 2:

$$X_2(p_2) = \begin{cases} 1 - X_1 - p_2 = (p_1 - \delta p_2) / (1 - \delta) - p_2 & \text{for } 1 - \delta + \delta p_2 > p_1 \\ 1 - p_2 & \text{otherwise.} \end{cases}$$

Now in period 2 the monopolist only cares for his periodic profit π_2 in period 2 since this is the only variable part of his payoff $\Pi = \pi_1 + \delta \pi_2$. Furthermore, he cannot change anymore the expected price p_2^* for period 2. The profit π_2 depends on p_2 as follows:

$$\Pi_2(p_2) = \begin{cases} [(p_1 - \delta p_2) / (1 - \delta) - p_2] p_2 & \text{for } 1 - \delta + \delta p_2 > p_1 \\ (1 - p_2) p_2 & \text{otherwise.} \end{cases}$$

The optimal price p_2^* is $v^*/2$ with $p_2 = p_2^*$ or $1/2$. Assuming rational expectations, i.e. $p_2^* = p_2^*$, one obtains

$$p_2^* = \begin{cases} p_1/(2-\delta) & \text{for } 1-\delta/2 > p_1 \\ 1/2 & \text{otherwise.} \end{cases}$$

which implies

$$\Pi_2(p_2^*) = \begin{cases} p_1^2(2-\delta)^2 & \text{for } 1-\delta/2 > p_1 \\ 1/4 & \text{otherwise.} \end{cases}$$

Anticipating these results for period 2, one can now express $\Pi = \pi_1 + \delta\pi_2$ as a function of p_1 alone:

$$\Pi = \pi_1 + \delta\pi_2 = \begin{cases} p_1(2-\delta-2p_1)/(2-\delta) + \delta p_1^2/(2-\delta)^2 & \text{for } 1-\delta/2 > p_1 \\ \rho/4 & \text{otherwise.} \end{cases}$$

Since the upper value on the right hand side above is maximized by $p_1 = (2-\delta)^2/2(4-2\delta-\rho)$, the maximal payoff Π^* of the monopolist is given by

$$\Pi^* = \begin{cases} (2-\delta)^2/4(4-2\delta-\rho) & \text{for } 2 \geq \delta+\rho \\ \rho/4 & \text{otherwise.} \end{cases}$$

Since $2 \geq \delta+\rho$ is always true, the profit $\Pi^* = (2-\delta)^2/4(4-2\delta-\rho)$ can be realized by the pricing behavior

$$p_t^* = \begin{cases} (2-\delta)^2/2(4-2\delta-\rho) & \text{for } t = 1 \\ (2-\delta)/2(4-2\delta-\rho) & \text{for } t = 2 \end{cases}$$

In game theoretic terminology, we have solved the durable monopoly game with two sales periods. The durable monopoly game has several subgames resulting after some initial moves. More specifically, every sequence of previous moves ending with the price decision of the monopolist or the demand decisions of the consumers, which did not yield an end of play, constitutes a proper subgame of the durable monopoly game. Subgame perfect equilibria (Selten, 1975) require that a strategy vector is not only an equilibrium point of the game itself but that it also implies strategy combinations for the subgames which are equilibria in these subgames. Backward induction means to solve first the smallest subgames, i.e. those subgames with the longest sequence of previous moves before they are reached, then the second smallest subgames, etc. till one reaches the first move of the game. For the durable monopoly game with two sales periods we have proved by backward induction

Proposition II.2: Every subgame perfect equilibrium point of the durable monopoly game with two sales periods implies the sales prices $p_1^* = (2-\delta)^2/2(4-2\delta-\rho)$ and $p_2^* = (2-\delta)/2(4-2\delta-\rho)$ which yield the payoff $\Pi^* = (2-\delta)^2/4(4-2\delta-\rho)$ for the monopolist.

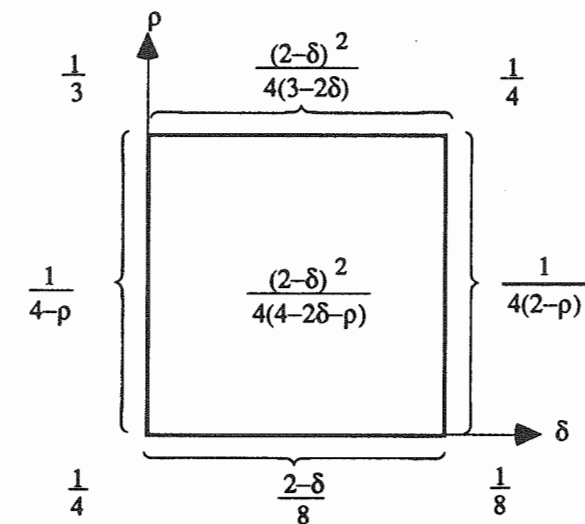
According to Proposition II.2, the monopolist can only achieve the monopoly profit $1/4$ in the special cases $\delta = 0 = \rho$ and $\delta = 1 = \rho$. In case $\delta = 0 = \rho$, there is

actually no durable monopoly since nobody wants to wait whereas in case of $\delta = 1 = \rho$ nobody minds waiting.

In Figure II.1 we illustrate graphically for the δ, ρ -unit square how the profit Π^* of the monopolist depends on both discount factors, namely the discount factor δ of the consumers and the discount factor ρ of the monopolist. Whereas Π^* is equal to $1/4$ in the two opposite corners $\delta = 0 = \rho$ and $\delta = 1 = \rho$, the result for the other two corners is very different. In $\delta = 0$ and $\rho = 1$ the profit $\Pi^* = 1/3$ results from price discrimination:

Figure 12.1 (II.1)

The Monopoly Profit for Two Possible Sales Periods



Since no consumer wants to wait, the monopolist first exploits consumers with high redemption values and sells to consumers with low redemption values later. Here we are more interested in the result for the remaining corner $\delta = 1$ and $\rho = 0$ for which the monopoly profit Π^* is only $1/8$, i.e. only half of the monopoly profit.

The intrapersonal strategic conflict of the monopolist is the essential reason for reducing the monopolist's payoff from $1/4$ to $1/8$ for $\delta = 1$ and $\rho = 0$. The "monopolists" of the second sales period, who are confronted with a given price p_1 , do not care at all about π_1 whereas the monopolist of the first sales period cares for $\Pi = \pi_1 + \delta\pi_2$ and therefore has a different motivation. This competition of the monopolist of the first sales period and those of the second one can be most easily illustrated by considering the limiting case $\delta = 1$ and $\rho = 0$. For $\delta \rightarrow 1$ both prices, p_1^* and p_2^* , converge, e.g. to $1/4$ for $\rho = 0$, which indicates how the monopolist of the first sales period and the one of the second sales period are compet-

ing for demand. Since consumers do not mind waiting, we envisage here a form of price competition on a homogeneous market which became known as Bertrand-price competition (Bertrand, 1883, see also Allen and Hellwig, 1986, for a more recent study) and which is the most severe form of price competition.

As illustrated by the durable monopoly game with two sales periods, the notion of an equilibrium point allows for omnipotent players who can determine all their future acts in advance even if such an act is rather irrational given the future situation. So the result of Proposition II.1 relies on the threat of the monopolist not to decrease his price although it would be profitable for him to do so when period 2 is reached. The more refined concept of subgame perfect equilibria has ruled out such an omnipotent monopolist since it requires optimality of p_2 with respect to π_2 , i.e. by purely looking at the second sales period. As will be shown in the next sections, the concept of subgame perfect equilibria is, however, not sufficient to rule out omnipotent players.

The durable monopoly game has been proposed by Coase (1972) who conjectured that in case of infinitely many sales periods, the monopolist will earn only the competitive profit of 0 which has been rigorously proved by Gul, Sonnenschein, and Wilson (1986). One way to approach infinitely many sales periods is to assume a given trading time, split up in sales periods of equal length, and fixed discount factors for this given time interval. Increasing the number of trading periods therefore implies a smaller time difference between two neighboring trading periods and corresponds to larger discount factors relating the profits of two neighboring trading periods. Thus the assumption of more trading periods in a given physical time interval corresponds to an increase of the discount factors in our model with just two sales periods. Güth and Ritzberger (1992) have shown that for a finite time horizon the Coase-conjecture is wrong since the monopolist can always delay sales to the latest point in time. Also for infinitely many sales periods of a given time length the Coase-conjecture is generally false according to Güth and Ritzberger (1992). It usually requires that the seller is much more impatient than his potential customers. Our discussion assumed that the utility level v of a consumer can be enjoyed only in the period when the product is bought. Other versions of the durable monopoly game allow v to be enjoyed continuously after buying (see Tirole, 1988, Section 1.5 for references).

Although the implications of Proposition II.2 for discount factors $\delta = 1$ and $\rho = 0$ may appear counterintuitive at first sight, there seems to be quite some evidence for behavioral analogues. A publisher, being the monopolist by owning the copyright, often sells first an expensive 'hard cover' version and later a much cheaper 'paper back'. Both versions, furthermore, often become cheaper as time goes by. If such a pricing behavior is anticipated, the publisher seriously restricts his sales amounts in the first sales periods.

Güth, Ockenfels, and Ritzberger (1993) have performed experiments with durable monopoly markets differing in the number of possible sales periods (two or three)

as well as in the constellation (δ, ρ) of discount factors $((.05, .95), (.5, .5), (.95, .05))$. The participants of one session first learned the theory of durable monopoly markets before actually playing them. In the other session participants were completely unfamiliar with such markets and with game theory, in general, but allowed to play the market again. Although experience and even more the theoretical instruction improved the quality of results, the game theoretic prediction was hardly ever supported. The qualitative effects observed for the different markets differed quite often from those predicted.

Many market institutions like notaries and other institutions of private law, which help us to restrict our future behavior, are based on the experience that people cannot rely on others, nor on themselves. In essence such institutions allow us to cut bridges behind, i.e. to commit oneself to a mode of behavior which is suboptimal when actually facing the decision problem. The durable monopoly game shows that one can gain by such commitments. Consequently there are clear economic incentives for developing institutions which can provide the required commitment power.

From a psychological point of view it is interesting to ask when a decision maker prefers external commitment, e.g. a legally enforceable one, and when he can rely on selfcommitment due to internalized or socially controlled moral norms. By the example of durable monopolies it should have become clear that it does not matter whether others or the one, who commits himself, is interested in keeping the promise. Societies where breaking a promise is seriously punished, e.g. by isolation, should have fewer institutions providing external enforcement of future behavior than societies where promises are more or less considered as cheap talk. In general, it seems justified to say that intrapersonal strategic conflicts, as exemplified for the monopolist in the durable monopoly game, are commonly known and often anticipated in everyday life. Thus in actual life, people are used to thinking about their future ego as a different decision maker, i.e. also from a behavioral point of view omnipotent players are not very appealing.

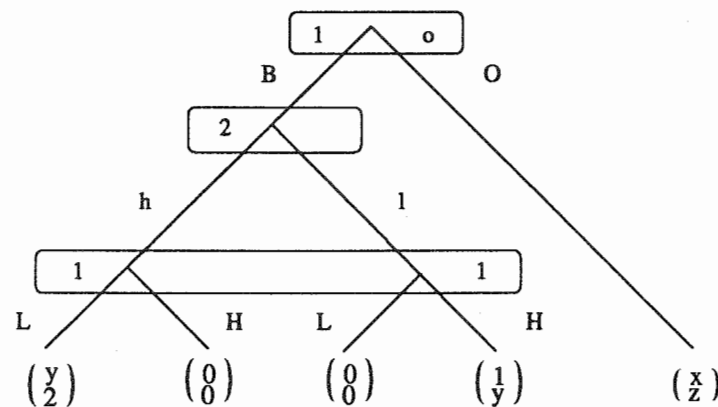
Forward versus backward induction

In the durable monopoly game, we could avoid the omnipotent monopolist by applying the concept of subgame perfect equilibria by which the monopolist is forced to set optimal prices for the given residual demand although he would have preferred not to do so. In the following we want to show that the restriction to subgame perfect equilibria does not rule out omnipotent players. More specifically, we want to demonstrate that the game theoretic concept of forward induction preassumes omnipotent players although forward induction solutions are subgame perfect equilibria.

Unlike backward induction, which solves every subgame by only considering its strategic aspects and which therefore neglects how the subgame is embedded in a larger game context, forward induction tries to interpret earlier moves in a game as signals of future behavioral intentions. Consequently, the solution of a subgame may well depend on how it is embedded in a larger game context. If an earlier move is a sure hint of future strategic actions, one has to assume that the earlier move and the future actions are decisions of the same decision maker. This indicates that forward induction relies on omniscient players who can determine all their future moves.

Forward induction is a concept to select one of several subgame perfect equilibria as the solution in outside option games. Although Van Damme (1987) gives some hints that forward induction is related to the generally applicable stability concept of Kohlberg and Mertens (1986), it therefore is only an ad hoc-selection concept for a very special class of games. An outside option is an alternative way of earning a certain profit instead of interacting strategically with the other players. So a firm may produce something else and thereby avoid bargaining with its customer over the price of a special delivery.

Figure 12.2 (III.1)
An Outside Option Game



A simple game like this is illustrated in Figure III.1 with payoff parameters satisfying $1 > x > y > 0$. Player 1 has an outside option 0 which yields the payoff x for him and an arbitrary fixed payoff z for player 2. The game starts at the origin o (the top decision node in Figure III.1) with player 1's decision for 0 or for B where B stands for bargaining with player 2. In case of 1's choice for B, players 1 and 2 bargain in the form of exchanging demands where both players can either choose a high (indicated by 'H' and 'h') or a low (indicated by 'L' and 'l') demand. If both

players choose a high or a low demand, no agreement has been reached and both players receive their conflict payoff of 0. In the other two cases both demands are consistent and an agreement has been achieved. In case of (L,h) player 1's (2's) agreement payoff is y (2) whereas the agreement payoffs are 1 and y , respectively, in case of (l,H). By encircling the two decision nodes in Figure III.1 where 1 has to decide between L and H it is indicated that 1 does not know whether he is at the right or left decision node, i.e. 1 is not informed about 2's previous decision for h or l. A set of encircled decision nodes is called an information set.

In game theoretic terminology, Figure III.1 is a simple game in *extensive* form consisting of a game tree, the player partition (indicated by attaching player numbers to decision nodes), the information partition (indicated by its information sets), the move partition (indicated by attaching the same move symbol to one move at every decision node in the same information set), and the payoff function which assigns a payoff vector to every endpoint (lowest node in Figure III.1) of the game tree. In general, an extensive form game may also involve chance moves so that one also may need a probability assignment specifying a probability distribution for every chance move.

The forward induction solution for the game in Figure III.1 is the subgame perfect equilibrium point $s^f = ((B,H),l)$ where (B,H) is the strategy of player 1 and l the one of player 2. The intuitive justification for the forward induction solution s^f is as follows: if player 2 observes that 1 has chosen B and believes that 1 is a rational decision maker, he must conclude that 1 expects more than x from choosing B since 1 can guarantee himself the payoff x by choosing his outside option 0. This implies that player 1 will choose H since he expects the equilibrium point (H,l) for the subgame after B which yields the payoff 1 ($> x$) for player 1. But if player 2 expects 1 to use H, his only best response is to choose l so that one obtains s^f as the solution of the game. Observe that according to this reasoning, player 2 expects an omniscient player 1 since 2 thinks that player 1 determines his final move H already when choosing B.

A more rigorous justification for the forward induction solution s^f relies on repeated elimination of dominated strategies. A strategy of a given player i , which is a complete behavioral plan in the sense that it specifies a choice for all possible decision problems of the player, is dominated if player i has another strategy which is never worse but at least for one possible behavior of i 's coplayers better. Eliminating dominated strategies might yield new dominated strategies so that one can eliminate dominated strategies repeatedly. For the game in Figure III.1, the strategy (B,L) of player 1 is dominated since this strategy yields at most y whereas 1 can guarantee himself x ($> y$) by choosing 0. But if (B,L) is eliminated, player 2's strategy h becomes dominated. After eliminating h, too, the remaining game is the trivial game with one player, namely player 1, who can choose between B yielding 1 for him and strategies prescribing 0 which yield only x (< 1). So one

has to eliminate the strategies prescribing 0 and receives $s^f = ((B,H),l)$ as the solution of the game in Figure III.1 by repeated elimination of dominated strategies.

Proposition III.1: Repeated elimination of dominated strategies yields a unique solution for the game in Figure III.1, namely the forward induction solution $s^f = ((B,H),l)$.

If one cannot be completely sure that the other players will avoid certain choices, dominated strategies will not be chosen by rational decision makers. If, furthermore, all players know that all players avoid dominated strategies, one can eliminate dominated strategies repeatedly. But what is rather debatable is whether one should eliminate repeatedly dominated strategies of omnipotent players or only of local decision makers. The game in Figure III.1 has three local decision makers, namely player 1 deciding between 0 and B, player 1 deciding between H and L, and player 2, and none of them has a dominated strategy or move, respectively. So if one relies on local players, the concept of repeated elimination of dominated strategies has no bite at all for the game in Figure III.1.

Unlike forward induction, backward induction requires solving the subgame after B in Figure III.1 by purely considering the strategic aspects of this subgame. This subgame has two equilibria, namely (L,h) and (H,l). Clearly, if one wants to select one of these as the solution of the subgame, one must select (L,h) since both equilibria yield the same payoff for one player but (L,h) a higher payoff than (H,l) for the other player. Actually, one can give a simple axiomatic justification for selecting (L,h) as the subgame solution. Symmetry invariance demands that neither (L,h) nor (H,l) can be the solution if (L,h) would imply the payoff level 1 instead of 2 for player 2 since in this case the subgame would be completely symmetric. Monotonicity, as proposed by Harsanyi and Selten (1988), requires then that the increase of player 2's payoff for (L,h) from 1 to 2 is sufficient to select (L,h) as the subgame solution since the incentive for selecting (L,h) instead of (H,l) has been increased. But if (L,h) is the subgame solution, then player 1 must choose 0 so that the backward induction solution of the game in Figure III.1 is $s^b = ((O,L),h)$.

Both solutions, the forward induction solution s^f and the backward induction solution s^b , are subgame perfect equilibria of the game in Figure III.1. Thus unlike in the durable monopoly game, subgame perfectness does not rule out omnipotent players. To do so we also have to rely on local players who determine their behavior by only considering the strategic problem at hand and not how it is embedded in a large game context, i.e. to solve the game in the spirit of backward induction.

A series of experiments (see Abdalla, Cooper, Dejong, Forsythe & Ross, 1989, Balkenborg, 1989, Gardner, Güth, and Ockenfels, 1990, and Van Huyck, Battalio and Beil, 1993) has tried to explore the predictive power of forward induction. Partly the experimental results are more in line with forward induction, partly they

support backward induction. In our view, forward induction is an important behavioral principle but not in the narrow sense of game theory where it is restricted to select among subgame perfect equilibria in a special class of games. Forward induction as a general behavioral principle means that later moves are made because of considerations underlying earlier moves, i.e. when deciding later, one feels obliged not to contradict previous decisions, e.g. due to an egodefensive attitude. For instance, many firms continue to invest into products which they have produced before even if other products or financial investments yield a higher profit. Partnerships, both of private and economic nature, continue to exist purely because one feels obliged to stick to one's previous decisions. The fact that we are warned 'not to throw good money after bad money' reveals that there is a severe danger to do so.

Unfortunately, most of the experimental studies do not consider forward induction in such a general sense (only Gardner, Güth, and Ockenfels, 1990, also consider situations where forward induction is not restricted to selecting among subgame perfect equilibria and provide some evidence for considerations of forward induction in the broader sense).

Completely unrelated experiments might also yield results indicating the promise or limitations of forward or backward induction. So Neelin, Sonnenschein, and Spiegel (1988) performed bargaining experiments where players 1 and 2 can bargain how to divide a monetary amount which decreases when the agreement is reached later. Bargaining starts with player 1 proposing how to divide the cake c_1 of the first bargaining period. If player 2 accepts this proposal, the game is over and c_1 is divided as proposed by player 1. If not, player 2 can propose how to divide the monetary amount c_2 of the second period, etc. until the final round is reached where non-acceptance means conflict with 0-payoffs for both players. Every such game has a unique subgame perfect equilibrium point that can be computed by backward induction. In the last round T the demanding player can ask for nearly all of c_T since the other player will accept any proposal yielding a positive payoff for him. So in the second to last round one can demand $c_{T-1} - c_T$ which is positive since the cake c_t is shrinking with time t. Neelin, Sonnenschein, and Spiegel found that in the first round most players 1 demand $c_1 - c_2$ for themselves. For $T = 2$ this is the opening demand of the backward induction solution as shown above. But this behavior was also observed in games with $T = 3$ and $T = 5$ where it is quite different from the game theoretic opening demand. Thus already in slightly complex situations people avoid the cumbersome backward induction procedure and use a more simple approach for determining their opening demand by asking: What can I safely demand in period 1 without having to fear a rejection by player 2? Certainly $c_1 - c_2$, since by rejecting player 2 can earn at most c_2 . Thus backward induction is questionable from a behavioral point of view although it is normatively very convincing.

Incomplete information

When exploring the possibility of intrapersonal strategic conflicts, games with incomplete information are crucial since there is no clear notion of a player in such games. Due to private information, in general, only the player himself knows his own true type whereas his coplayers may expect a large variety of types which will be defined more formally below. On the one hand a player may be the actual decision maker, i.e. the given true type. On the other hand the same player, as expected by his coplayers, may be of a much more complex nature (in the sense of various possible types) involving intrapersonal strategic conflicts which do not exist for the actual decision maker.

Incomplete information means that the rules of the game are not commonly known. If the game is given in extensive form, one may, for instance, not know the game tree, the player, information, or move partition, the probability assignment, or the payoff function. Usually one considers only incomplete information about the payoff function since other information deficits can be represented by strategically equivalent games whose payoff function is unknown.

Incomplete information about the payoff function means that at least one player does not know for sure the payoff function of at least one of his coplayers, i.e. for at least one player i the coplayers expect more than one payoff function for i with positive probability. The possible payoff functions for such a player i are called the types of player i . Whereas i knows his own type, at least one other player is not sure about i 's true type, i.e. payoff function.

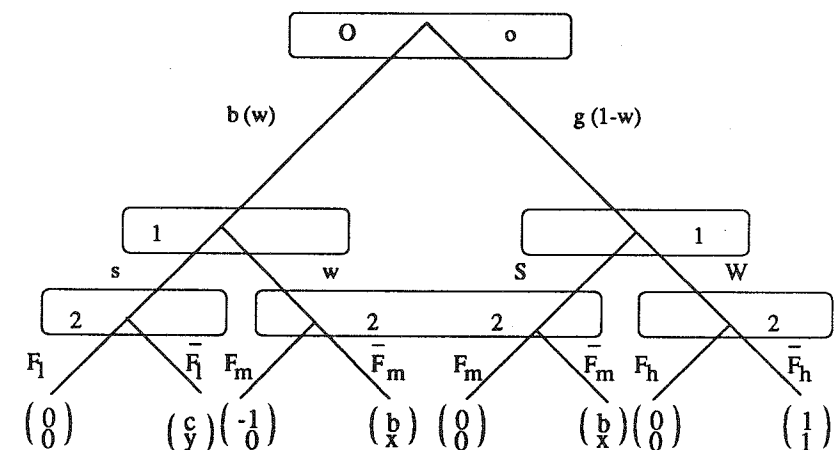
The essential trick to capture the uncertainty about other players' types and to have a nevertheless informationally closed and thereby solvable game is to introduce a fictitious initial chance move determining the types of all players. The probabilities of this chance move are given by the subjective probabilities of all players i concerning the type constellations of their coplayers. Its result is only partially revealed to induce the correct beliefs about the types of all players. Since this fictitious initial chance move is assumed to be commonly known, i.e. every rational player will transform the game by including the fictitious chance move, one has a game with complete information which can be solved in the usual way. Thus the essential trick, due to Harsanyi (1967/68), is to transform incomplete information about other players' types into stochastic uncertainty resulting from partly unobservable chance moves.

Instead of discussing in rather abstract terms the impact this may have for the player notion and the possibility of intrapersonal strategic conflicts, we prefer to illustrate this by a simple principal-agent model. Whereas the agent (player 1) knows more about the productivity of the firm, the principal (player 2) is neither sure about the productivity level nor about the effort of his agent who may or may not be shirking. The game is graphically represented in Figure IV.1. We impose the conditions $0 < \omega < 1$, $1 > x > 0 > y$, and $1 > b > c > 0$.

The game starts at the top decision node o with the fictitious initial chance move (by chance player 0). b stands for bad productivity, which results with probability w , and g for good productivity with the complementary probability $1 - w$. Knowing which productivity level has been chosen, player 1 can shirk (s in case of bad and S in case of good productivity) or work (w , respectively W). Shirking in the case of bad productivity leads to a very low output and reveals that the agent has been shirking, i.e. after the moves b and s the principal/player 2 observes the output level and is therefore completely informed about the previous moves as indicated by encircling his decision node where he chooses between F_1 and \bar{F}_1 . Similarly, working in the case of good productivity yields such a high output level that player 2 knows that g and W have been chosen before.

But when player 2 observes an intermediate output level, he does not know whether it is due to bad productivity or to shirking in the case of good productivity. This is graphically indicated by encircling the two decision nodes of player 2 after the moves b and w as well as g and S where player 2 has to decide between F_m and \bar{F}_m . The final decision 'F' of player 2 stands for firing the agent, whereas ' \bar{F} ' means that player 2 continues to rely on his agent. Of course, when deciding whether to fire his agent, player 2 knows whether the output level has been low (ow), at a medium level, or high (igh). He therefore can condition his decision on the observed output level as indicated by the three different choice symbols F_1 , F_m , F_h and \bar{F}_1 , \bar{F}_m , \bar{F}_h , respectively.

Figure 12.3 (IV.1)
Principal-agent Model



The payoff function is described in the usual way by attaching payoff vectors to every endpoint where the upper (lower) number is player 1's (2's) payoff for the corresponding play. The parameter condition $1 > b > c > 0$ means that a higher

output induces a higher wage and that the wages must be positive. If player 2 does not fire his agent, his worst payoff y results in the case of shirking and bad productivity, whereas his best payoff is 1 after observing the high output level. For the intermediate output level continuation of the employment contract yields x with $1 > x > 0$ for player 2. If the employment contract is not extended, both players receive 0 – payoffs except for the endpoint after b , w , and F_m when player 1 receives only -1 .

Observe that the actual situation may be such that player 1 knows that the productivity is bad. So we also have to include the good productivity part of the game tree since player 2 falsely believes that the productivity could be good. Although player 1 knows that b is true, by the notion of a strategy he nevertheless has to decide whether to shirk or to work in the case of good productivity. This indicates that in the case of incomplete information one has to analyze a much more complex situation than the actual situation as perceived by at least some players. The game in Figure IV.1 has two proper subgames with the trivial solutions F_l , due to $y < 0$, and \bar{F}_h . Anticipating this, player 1 prefers W over S since W yields for him the highest possible payoff. Thus in any equilibrium 1 must choose w and 2 his move \bar{F}_m so that the signaling equilibrium $s^s = ((w, W), (F_l, \bar{F}_m, \bar{F}_h))$ is the solution of the game due to our parameter restrictions. Signaling here means that, knowing the strategy vector, the principal can infer which type of agent, the one with bad or the one with high productivity, he encounters.

Proposition IV.1: For the parameter restrictions $0 < \omega < 1$, $1 > x > 0 > y$, and $1 > b > c > 0$ the solution of the game in Figure IV.1 is the signaling equilibrium $s^s = ((w, W), (F_l, \bar{F}_m, \bar{F}_h))$.

The assumption $1 > b > c$ is, of course, rather questionable since the wage is not the only determinant of player 1's payoff. Working means to sacrifice the pleasure of shirking which might justify the assumption $c > b > 1$ for which the game has two subgame perfect equilibria, namely $s^o = ((s, W), (F_l, F_m, \bar{F}_h))$ and $s^p = ((w, S), (F_l, \bar{F}_m, \bar{F}_h))$. But since every strategy prescribing the move F_m is dominated due to $x > 0$, s^p is the only reasonable solution for the game with $c > b > 1$ instead of $1 > b > c > 0$. Since both agent types induce the intermediate output level m , s^p is a pooling equilibrium which does not allow the principal to infer whether productivity is bad or good. Whereas according to our initial assumption the agent always avoids shirking, this happens only in the case of low productivity according to our new assumption which accounts for the pleasure of shirking.

Proposition IV.2: For the parameter restrictions $0 < \omega < 1$, $1 > x > 0 > y$, and $c > b > 1$ the solution of the game in Figure IV.1 is the pooling equilibrium $s^p = ((w, S), (F_l, \bar{F}_m, \bar{F}_h))$.

Here we are not primarily interested in interpreting the results for the game of Figure IV.1 and how they change from parameter region to parameter region, but in discussing the player notion for games with incomplete information. In the

game of Figure IV.1 player 1, the agent, can be both: either the agent knowing that productivity is bad, or the other agent who knows that productivity is good. But since the initial chance move is purely fictitious, there is no real life analogue to player 1 of Figure IV.1 who represents both these agents. This player 1 is purely a game theoretic construct in order to reflect the principal's uncertainty about the productivity of the firm. To assume that this player 1 of Figure IV.1 can control all his moves would therefore mean to introduce a completely new decision maker with no real life analogue. The most centralized concept of a player in games with incomplete information is therefore the type of player, i.e. in Figure IV.1 we would have to distinguish two agent players, namely the one who knows that productivity is bad, and the one who knows that productivity is good. Of course, the game of Figure IV.1 could also be interpreted as a stochastic game with the interpretation that productivity is stochastic and determined by an actual chance move whose result is revealed only to player 1, i.e. in this game there exists a real life analogue to the central player 1 who may envisage bad or good productivity. Now allowing for an omnipotent player 1 when the chance move is real, and splitting up into type players in the case of a fictitious chance move means to sacrifice the unified treatment of games with and without incomplete information which, in our view, is one of the greatest achievements in game theory. One certainly does not want the result of a game to depend on the interpretation of its chance moves. Thus games with incomplete information tell us to give up the notion of omnipotent players. The most centralized concept of a player is therefore the type player (see Harsanyi and Selten, 1988, who rely on type players).

Observe that the concept of type players is an intermediate concept as compared to omnipotent and local players. This is most clearly indicated by our previous examples, the durable monopoly and the outside option game in Figure III.1, which did not contain any chance moves so that type players coincide with omnipotent players. Thus also type players will encounter intrapersonal strategic conflicts as demonstrated for the monopolist in the durable monopoly game and for player 1 in the outside option game. If one wants to avoid intrapersonal strategic conflicts, the concept of type players as supposed, for instance, by Harsanyi and Selten (1988), is only a half hearted and insufficient step away from the notion of omnipotent players.

Game theory assumes that rationality of all players is common knowledge (see, for instance, Binmore, 1987) in the sense that all players are rational, and that all players know that all players are rational, and that all players know that all players know ..., Now human decision makers can satisfy at most bounded rationality, i.e. employ cognitive methods which are in line with their limited computational and analytic abilities. Boundedly rational decision making, in general, tries to determine goal oriented behavior which requires only cognitive abilities which a well trained human decision maker can satisfy. It will differ not only from rational behavior due to cognitive limitations but also from real behavior which often is

influenced by irrelevant environmental aspects which, for a boundedly rational decision maker, should not matter (see, for instance, the framing effect discussed by Kahneman and Tversky, 1984, and the presentation effect in decomposed prisoners' dilemma games as explored by Pruitt, 1970). Since perfect rationality is the exception rather than the rule, practically all real games are games with incomplete information since we usually do not know how rational our opponents are in pursuing their interests. Especially all game playing experiments are therefore experiments of games with incomplete information where we mostly do not control well enough the beliefs concerning the types of other players.

Thus the usual practice of experimental game theory (see, for instance, Güth and Tietz, 1990) to compare the experimental results with the game theoretic solution of the game, when monetary payoffs are assumed to represent payoffs and when rationality of all players is common knowledge, is very dubious. In our view, it would be a promising field of cooperation between experimental game theorists and psychologists to develop experimental techniques which allow for a better control of what subjects believe about the rules of the game, in general, and more specifically about the types of their coplayers. One might, for instance, try to find out which motivational forces a player attributes to all his coplayers.

If all game playing experiments are viewed as experiments of games with incomplete information, one may ask why it is important to perform experiments based on games of incomplete information (see, for instance, the large number of auction experiments surveyed by Kagel, 1994, and the experiment of Mitzkewitz and Nagel, 1993 and Güth, Huck, and Ockenfels, 1993). In our view, such experiments only introduce additional uncertainties as usual chance moves whose results are only partly revealed. One therefore does not need a specific behavioral theory for games with incomplete information. One only has to enrich the spectrum of institutional details for which one explores the game playing behavior by all kinds of uncertainties, including such chance moves by which game theory tries to capture incomplete information.

On repeated games

Let G denote a given game, for example, in extensive form. In a repeated game with base game G this game is not played once but repeatedly. The repeated game is not completely defined by G and the number T saying how often G is played in sequence. One also has to specify what the players learn about the previous playings of G and how they evaluate a given sequence of T playings of G . Here we will simply assume that all previous moves are commonly known, i.e. any start of a playing of G defines a subgame of the repeated game which is also a repeated game of G with a smaller repetition number. If T is finite, we assume that

the payoff in the repeated game is the average payoff of the T plays of G . In the case of $T = \infty$, this payoff is defined as the limes inferior of the periodic payoffs. Let Π_i^t be player i 's payoff in the t -th play of G . The sequence $(\Pi_i^1, \Pi_i^2, \dots)$ of periodic payoffs with $T = \infty$ is evaluated by the limes inferior $u_i = \lim_{t \rightarrow \infty} \inf \{\Pi_i^\tau : \tau \geq t, \tau \in \mathbb{N}\}$,

i.e. essentially as the lowest periodic payoff occurring infinitely often. \mathbb{N} denotes the set of natural numbers. This defines the rules of repeated games with finite or infinite repetition numbers. In the case of $T = \infty$, the repeated game of G is often called the supergame of the base game G .

For the sake of simplicity we will rely on base games G in normal form. A game in normal form is described by the vector $G = (S_1, \dots, S_n; \Pi)$ where $n (\geq 2)$ is the number of players, S_i for player $i = 1, \dots, n$ the set of strategies s_i , and Π the payoff function assigning a payoff vector $\Pi(s) = (\Pi_1(s), \dots, \Pi_n(s))$ to every strategy vector $s = (s_1, \dots, s_n)$. To avoid that one has to distinguish between local and omnipotent players in G we assume G to be structured in such a way that both notions coincide, i.e. for $i = 1, \dots, n$ the strategy s_i prescribes only one move, i.e. every player $i = 1, \dots, n$ has exactly one information set.

In the case of $n = 2$, the normal form can be described as a bimatrix game with rows representing player 1's strategies s_1 and columns representing player 2's strategies s_2 . For every matrix field $s = (s_1, s_2)$ the double entry is the payoff vector $\Pi(s) = (\Pi_1(s), \Pi_2(s))$ for the strategy vector s .

Famous examples of such games are given below.

Table 12.1 (V.1)

s_1^2	s_2^1	s_2^2
s_1^1	(β, b)	(δ, a)
s_1^2	(α, d)	(γ, c)

$$\alpha > \beta > \gamma > \delta, a > b > c > d$$

Table 12.2 (V.2)

s_1^2	s_2^1	s_2^2
s_1^1	(α, a)	$(0, 0)$
s_1^2	$(0, 0)$	(β, b)

$$\alpha > \beta > 0, b > a > 0$$

Table 12.3 (V.3)

s_1^2	s_2^1	s_2^2
s_1^1	1	-1
s_1^2	-1	1

Table V.1 depicts the 2-person prisoners' dilemma game: Both players have only one undominated strategy, namely s_1^2 , but the only equilibrium point $s^2 = (s_1^2, s_2^2)$ is payoff dominated by $s^1 = (s_1^1, s_2^1)$ in the sense of $\Pi_i(s^1) > \Pi_i(s^2)$ for both

players i . Another well-known example is the battle of sexes-game in Table V.2 with the two equilibrium points $s^1 = (s_1^1, s_2^1)$ and $s^2 = (s_1^2, s_2^2)$ where player 1 prefers s^1 and player 2 the other equilibrium s^2 . In the case of $\Pi_1(s) + \dots + \Pi_n(s) = 0$ for all strategy vectors s one says that game G is a zero-sum game. Due to $\Pi_1(s) = -\Pi_2(s)$ for all strategy vectors s , a 2-person zero-sum game can be represented by a matrix game with entries $\Pi_1(s)$. Table V.3 is commonly known as the matching of pennies-game with a unique equilibrium according to which both players use both their strategies with probability $1/2$. One usually refers to probability distributions over S_i as mixed strategies of player i , whereas probability distributions which put all the weight on one strategy $s_i \in S_i$ are called pure strategies. If every move by player i is chosen with positive probability according to a mixed strategy of player i , this strategy is said to be completely mixed. The game in Table V.2 has an equilibrium point in completely mixed strategies in addition to s^1 and s^2 according to which player 1(2) uses his strategy $s_1^1(s_2^2)$ with probability $b/(a+b)$ and $\beta(\alpha+\beta)$, respectively.

Consider a repeated game with the base game G being the prisoners' dilemma game in Table V.1 and assume $T < \infty$, i.e. all players know that they are going to play G exactly T times in sequence. Backward induction tells us to solve the repeated game by solving first the last round of playing G since the subgames starting in the last round T are the smallest subgames. Clearly, such games have the unique equilibrium s^2 like the base game G itself. If now it is anticipated in the second to last round $T-1$ that s^2 will be played in all subgames starting in round T , all strategies prescribing the move s_1^1 for round $T-1$ are dominated strategies in all subgames starting in round $T-1$ since the only justification for using s_1^1 in round $T-1$ is that s^1 induces cooperation later whereas s^2 prevents it. Thus all solutions of such subgames prescribe $s^2 = (s_1^2, s_2^2)$ as initial moves in round $T-1$. Proceeding in the same way, one can show that the game has a unique subgame perfect equilibrium point according to which both players i always choose their second strategy s_i^2 :

Proposition V.1: The finitely repeated prisoners' dilemma game in Table V.1 has a unique subgame perfect equilibrium point according to which both players i will always rely on their undominated strategy s_i^2 of the base game G in Table V.1.

There is an impressive body of experimental evidence (see, for instance, Axelrod, 1981, Smale, 1980, Stöcker, 1978 and 1980, Selten and Stöcker, 1986, for references) contradicting the game theoretic prediction that players will always rely on their undominated strategy s_i^2 in finitely often repeated prisoners' dilemma games. What one observes instead is that players cooperate in the sense of choosing s_i^1 till shortly before the end of the game where they typically defect from cooperation, a phenomenon called termination effect. As a consequence, participants of repeated prisoners' dilemma experiments earn in average much more than predicted by game

theory. Furthermore, these prospects from mutual cooperation except for the two or three last rounds seem to explain why subjects do not rely on backward induction although, as indicated by the termination effect, they seem to be aware of it.

Somebody, who understands that one should not cooperate at the end and that this provides an incentive not to cooperate earlier and who nevertheless starts by playing cooperatively, apparently does not accept the logics of backward induction (see the impressive evidence of Güth, Ockenfels, and Wendel, 1993, as well as McKelvey and Palfrey, 1992, for such behavior). This can be explained if one allows for decision behavior which is not only guided by its own monetary incentives but also by what is in the mutual interest of all interacting individuals. In a repeated prisoners' dilemma game players can earn far more by cooperating than by obeying the game theoretic prescription. From a psychological point of view it is therefore interesting to explore conditions for which cooperation, although it is individually non-optimal, can nevertheless be a stable phenomenon. The experimental results indicate, for instance, that cooperation is quite stable if cooperation is much more profitable than competition.

Not all game theorists consider game theory as a purely normative discipline but want it to be also a descriptive theory. For such scholars, the experimental results of finitely repeated prisoners' dilemma games are truly troublesome and there have been many attempts to find an escape (see, for instance, Radner, 1980, Kreps and Wilson, 1982a, Milgrom and Roberts, 1982, and Kreps, Milgrom, Roberts, and Wilson, 1982). Here we want to discuss briefly one approach to justify cooperation in finitely repeated prisoners' dilemma games which is in the spirit of Kreps, Milgrom, Roberts, and Wilson (1982) and which is in line with our fundamental assertion that all game playing experiments involve games with incomplete information.

The essential idea of Kreps et al. is to introduce incomplete information of a very specific nature. Assume, for instance, that both players expect their opponent with a very low but positive probability to be a cooperator in the sense that for him the strategy s_1^1 instead of s_1^2 is the only undominated strategy. In the terminology of types, both players are thus expected to have payoff functions or types satisfying the parameter restriction in Table V.1 with very high probabilities and, with low probabilities, payoff functions or types satisfying $\beta > \alpha$, $\delta > \gamma$ and $b > a$, $d > c$, respectively. Due to the fictitious initial chance move, which adequately takes into account the incomplete information and whose result is only partly revealed so that both players are only sure about their own type, the finitely repeated prisoners' dilemma game does not have any subgames (a subgame is informationally closed, i.e. all players deciding in the subgame know that they are in the subgame). Thus by definition, every equilibrium is subgame perfect. To avoid the concept of type players in such games, one therefore has to apply a stronger solution concept than that of subgame perfect equilibria, e.g. the one of perfect equilibria (Selten, 1975) or of sequential equilibria (Kreps and Wilson, 1982b).

A sequential equilibrium is a pair containing a strategy vector and a belief function which defines for every information set a probability distribution over its decision nodes. We refer to such a probability distribution as to the beliefs of this local player concerning the likelihood of the various nodes in his information set. For such a pair to be a sequential equilibrium, every move must be a best reply to the strategy component given the beliefs of the local player. Furthermore, the strategy component and the beliefs have to be consistent. Consistency means that one can find vectors of completely mixed strategies which converge to the strategy component and which, by Bayes' rule, induce beliefs converging to the belief component of the sequential equilibrium.

Similar to Kreps et al. (1982), one can prove for our game model that there exists a sequential equilibrium according to which a player can build up a reputation to be a cooperator in the sense that he uses s_1^1 in the initial rounds as to increase his opponent's subjective probability that he is the type for whom s_1^1 , and not s_2^1 , is undominated. Typically this reputation is built up in the first rounds and exploited by defection in the final rounds. The typical equilibrium playing therefore closely resembles the typical playing of finitely repeated prisoners' dilemma experiments according to which one cooperates until the termination effect takes over.

Due to the incomplete information about the opponent's type, the most centrally deciding player in such a model is, of course, the type player, i.e. there are at least four different decision makers, namely the two types of both players. But the sequential equilibrium does not postulate global optimality in the sense that no single type player can gain by deviating but local optimality in the sense that, given his beliefs, no local player can profitably deviate. In this sense, applying the concept of sequential equilibria when solving games in extensive form is an important step to avoid intrapersonal strategic conflicts by relying on local players.

Nevertheless, this normative justification of cooperation in finitely repeated prisoners' dilemma games has serious flaws. First of all, incomplete information is assumed in a very tailored way forcing the desired result. One can very well envisage other possible types so that one needs to argue why the very specific kind of incomplete information is natural. Secondly, the sequential equilibrium only requires consistency of the beliefs so that formation of beliefs is quite arbitrary. Thus the analysis suffers mainly from two ad hoc assumptions, namely the tailored kind of incomplete information and the ad hoc-specification of beliefs.

Unlike for the finitely repeated prisoners' dilemma games, continuous cooperation by both players in the sense of playing s^1 in all rounds $t = 1, 2, \dots$ can be implied by subgame perfect equilibria of the infinitely often repeated prisoners' dilemma game with $T = \infty$. So, for instance, both players i can use their so-called grim strategy of the supergame which tells them to play cooperatively (choose s_1^1) as long as no player has deviated from the mutual cooperation s^1 before and to use their undominated move s_2^1 forever if there has been such a deviation. The vector

of these grim strategies implies the payoff vector $u = (u_1, u_2) = (\beta, b)$ according to the limes inferior definition. Clearly, any deviation from mutual cooperation will hurt the deviating player. Since, furthermore, playing s^2 always is also a subgame perfect equilibrium point of the supergame, punishing deviations from mutual cooperation is consistent with subgame perfect equilibrium behavior. This proves

Proposition V.2: In the supergame with the base game G of Table V.1, both the continuous play of s^2 as well as the continuous play of s^1 are implied by subgame perfect equilibria.

Proposition V.2 describes a special implication of the so-called Folk Theorem (see Güth, Leininger, and Stephan, 1990, for references) saying that every possible payoff vector of the base game G , which yields for both players what they can guarantee themselves (in the case of Table V.1 this is γ for player 1 and c for player 2), is implied by a subgame perfect equilibrium point of the supergame.

Since there can be no experiment testing the behavior in supergames due to $T = \infty$, one could simply ignore the result of Proposition V.2. Although we do not want to state and prove formally the more general result, one should mention that a statement similar to Proposition V.1 holds if T is stochastically determined as long as there exists a finite upper bound for T which is common knowledge. Thus, given the finite life expectation of human decision makers, only Proposition V.1 and not Proposition V.2 seems to be of practical relevance. Nevertheless, one often justifies cooperation in situations of repeated strategic interaction by Folk Theorem-like arguments. We therefore want to discuss the conceptual basis of Folk Theorems, especially since they seem to rely on omnipotent players which may encounter intrapersonal strategic conflicts.

To demonstrate the omnipotent player notion underlying Folk Theorems, consider the grim-strategy which has been used to prove Proposition V.2. If there has been a deviation from mutual cooperation, such a strategy prescribes the choice of s_2^1 whereas player i has to use s_1^1 if no such deviation occurred. But why should player i behave differently after these two sequences of previous moves? For $T = \infty$, each round $t = 1, 2, \dots$ is the starting period of subgames with exactly the same rules as the supergame itself. This is true since in any finite round t both players still expect to play the base game infinitely often and since previous moves have no impact for the rules of future strategic interaction. So why should player i choose different strategies in strategically identical subgames? This illustrates that grim-strategies and other strategies needed for proving Folk Theorems ask players to react to previous moves although these moves have no influence on the future game situation. We thus encounter a similar phenomenon as in the case of forward induction, namely that local decisions are not justified by the local strategic situation but by the great strategic plans of omnipotent players, which, of course, induces the possibility of intrapersonal strategic conflicts.

An obvious way to avoid such omnipotent players and thereby Folk Theorems is to require consistency in the sense that strategically equivalent games should have the same solution. Clearly, grim strategies are inconsistent like the other strategies used for proving Folk Theorems.

An even more convincing way to avoid Folk Theorems is to consider the supergame of the base game G purely as the limit of repeated games with $T < \infty$ when $T \rightarrow \infty$ and to accept only the asymptotically convergent subgame perfect equilibria of the supergame which can be approached by subgame perfect equilibria of finitely repeated games when $T \rightarrow \infty$. Due to Proposition V.1, the only asymptotically convergent subgame perfect equilibrium of the supergame with the base game G in Figure V.1 is the supergame strategy vector according to which both players always rely on their undominated strategy s_1^2 . Thus neither in the finitely, nor in the infinitely often repeated game do players ever cooperate. Clearly, asymptotically convergent subgame perfect equilibria are inspired by the idea of backward induction. Although the set of consistent and asymptotically convergent subgame perfect equilibria coincide for supergames, as defined here, this is not generally true. In dynamic games, where past moves can influence the future strategic situation by changing the state variables of the dynamic system, the two notions may differ since consistency allows to condition the behavior more or less arbitrarily on the value of state variables (for an example see Hoel, 1992). For asymptotically convergent subgame perfect equilibria, such a behavior is not possible. The two concepts coincide in supergames since the set of state variables is empty in supergames.

A game form avoiding intrapersonal conflicts

Usually the possibility of intrapersonal strategic conflicts due to more centralized notions of players depends on both the game form and the solution concept which one applies to solve the game. Often one can induce local players and avoid intrapersonal conflicts by applying a more refined equilibrium concept even if the game form as such allows for omnipotent players. So we could avoid the omnipotent monopolist in the durable monopoly game by relying on subgame perfect equilibrium points instead of assuming just the equilibrium property. Similarly, substituting subgame perfect equilibria by asymptotically convergent or consistent subgame perfect equilibria avoids Folk Theorems. Nevertheless one might ask whether there exists a game form which as such excludes all centralized player notions and thereby intrapersonal strategic conflicts. Fortunately, the answer is yes: Games in agent normal form rely on local players and exclude intrapersonal strategic conflicts.

Consider a game in extensive form with players $i = 1, \dots, n$ and let, furthermore, $m_i (\geq 1)$ be the number of information sets which player $i = 1, \dots, n$ has in this game. The agent normal form of this game is described by the vector

$$G = (S_1, \dots, S_N; \Pi)$$

where the player number N of G is the number of information sets of personal players, i.e. $N = m_1 + \dots + m_n$. Thus for every move of the extensive game, there is one player of its agent normal form who is responsible for it, i.e. the strategy sets S_i of the agent normal form are the move sets of the extensive form. The payoff function Π assigns a payoff vector

$$\Pi(s) = (\pi_1(s), \dots, \pi_N(s))$$

to every strategy vector $s = (s_1, \dots, s_N)$ where the evaluation of s by player i coincides with the evaluation of this strategy vector by the player of the extensive game whose move he determines. Each player i of the extensive form has therefore m_i agents who are choosing the m_i moves in the m_i information sets and who all evaluate the final result in the same way as player i does. Here one, of course, has to keep in mind that, in spite of the same evaluation of final results, an agent only cares for those results which he can influence by his decision, i.e. an agent will optimize the conditional payoff expectation given that his information set has been reached.

In our view, solving extensive games via solving their agent normal form is the most straightforward and adequate way to avoid omnipotent players and thereby intrapersonal strategic conflicts. Of course, for the agent normal form one cannot apply rationality requirements as, for instance, subgame perfectness since a normal form game by definition has no subgames (all players are assumed to decide independently, i.e. each player has only one information set). But one can impose stronger requirements as sequentiality or perfectness (Selten, 1975) which imply subgame perfectness. Furthermore, the notion of a cell (Harsanyi and Selten, 1988) generalizes the one of a subgame and is well defined for games in (agent) normal form.

For the durable monopoly game, the agent normal form would have many 'monopolist agents' since every possible sequence of previous market decisions ending with consumers' decisions is followed by a different information set of the monopolist. Given the multiplicity of monopolist agents, it may not be so surprising that the result of the durable monopoly game can be very close to the competitive outcome since the various monopolist agents have different payoff interests: Only the monopolist agent of the first sales period cares for the profit π_1 of the first sales period; only the monopolist agents of the second sales period and the one of the first sales period care for π_2 etc. Given the special payoff interests of two monopolist agents of different sales periods, their competition is more understandable.

In case of the outside option game in Figure III.1, the agent normal form would be a 3-person game since player 1 is substituted by two agents, one deciding between B and O and one choosing among L and H. And there is no way to tell the agent deciding between L and H what to do. When deciding, this agent knows that the outside option O has not been chosen. So his decision will try to optimize his payoff given that B is realized. But as argued above, there is no way to justify the solution (H,L), predicted by forward induction, when only the strategic structure of the subgame after B matters.

The agent normal form of Figure IV.1, which can be interpreted as a principal-agent model, has the two type players representing the principal's uncertainty about the firm's productivity, and the three agents of the principal as players although the principal is just one type player. In general, a type player splits up in several agents if he has to decide about more than just one move. Clearly, the two agents of the principal who decide between F_l or \bar{F}_l and F_h or \bar{F}_h , respectively, will have to choose their dominant strategy whenever they think that their decision matters. This shows how considerations of perfectness (Selten, 1975) or sequentiality imply subgame perfectness.

Like for other repeated games, the agent normal form of a finitely repeated prisoners' dilemma game is a multi-person game where the number of players depends on the repetition number T . Every perfect equilibrium (Selten, 1975) of this game will predict the general choice of s^2 by both players and will therefore conform to Proposition V.1. In the case of incomplete information, as suggested by Kreps et al. (1982), one, of course, cannot apply the concept of sequential equilibria, but has to rely on perfectness which implies sequentiality. Actually, the concept of perfectness preceded the one of sequentiality. The agent normal form of a supergame has, of course, infinitely many players showing how unrealistic such a model is. Similar to the durable monopoly game, an agent, furthermore, will only care for the strategic aspects of the subgame in which he is an active player. Thus there is no reason for an agent to condition his behavior on previous results as predicted by Folk Theorems. Consequently, the strategic plans must correspond to the only consistent or asymptotically convergent subgame perfect equilibrium.

By this study we hope to have illustrated that the agent normal form avoids intrapersonal strategic conflicts in game theory. Since one relies on local players, intrapersonal strategic conflicts become interpersonal strategic conflicts for which game theory has been developed. But doing so means to question some celebrated ideas and results of game theory as, for instance, forward induction and Folk Theorems.

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