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Essays on Economic Psychology

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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, Tarpy & 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 Schmölders, 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

	Environments (Contexts)				
Classes of Tasks	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. Municas & 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; Wärneryd, 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 Wärneryd 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. Wärneryd 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 Wärneryd: 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 selection. 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 developments. 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 & Smordinsky, 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 Schneiders'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, Elffers, 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, Tarpy & 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-

ticipation. 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 sunken 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, Milgrem, 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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