

The Makers of Modern Economics

Volume II

Edited by
Arnold Heertje

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Preface

In this book the active pioneers of modern economics describe their development as both scholars and teachers. They discuss those great researchers of the previous generation who helped to shape their careers as economists and all the contributors have provided their own views on the direction that economic theory will take in the future. The book is, therefore, a useful guide for the next generation of economists who, while standing on the shoulders of the present-day makers of modern economics, will find new theorems and new ways of understanding the economic landscape.

I have asked all the contributors to describe their own research and thinking and not to hesitate through fear of appearing a little less modest than one might expect. They have responded to my request in an open and frank manner. The advantage of this for us all is a clear picture of the significance of their research from their own point of view. This may represent the starting-point for others who may be at a point in their careers where a certain stimulus is needed.

I am grateful to all the makers of modern economics for co-operating in such a friendly way.

I also would like to express my gratitude to the publisher Edward Elgar who is doing a great job, both in production and marketing of the book.

Arnold Heertje

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4. Towards a broader and more inspiring economics

Bruno S. Frey*

The driving force of my life as an economist has been the attempt to improve the current state of economics which I regard in various ways as being unsatisfactory and of dubious relevance to real life. My research and teaching has been characterized by opposition, even rebellion, against the prevailing orthodoxy in our field – traditional neoclassical economics. This opposition to orthodox economics refers to four major questions which will be discussed in turn: Economy or Society? (section I); Methodological Narrowness or Interdisciplinary Perspective? (section II); Formalization or Substantive Novel Ideas? (section III); and American or European Influences? (section IV). The final section offers conclusions and presents some thoughts about more and less desirable future developments of economic science.

1. ECONOMY OR SOCIETY?

Orthodox economics, which is generally understood to be neoclassical economics, consists of a narrow view of the economy and often deals with market transactions only. In contrast to this, from the very beginning of my school education (at the Gymnasium) and at university I have been personally much intrigued by politics, sociology, history, psychology, law and art, that is, my interest in society has been a rather general one. This broad outlook was fostered by my academic teachers at the University of Basle, where I took up my studies in economics in 1960 – the very year in which my *alma mater* proudly celebrated its 500th anniversary. While modern economics was appreciated, the context of my studies was more truly academic in the sense that it was not confined to an expert handling of well-defined formal tools, but consisted of an attempt to understand what was going on in the world, including what had happened in the past. My first academic teacher, Edgar Salin (1892–1974) in his younger years fought much against the ‘school of historical economics’ then prevalent in Germany. According to today’s

standards he was, however, strongly committed to viewing economics from a historical perspective. In fact, Salin was as much a historian and *'homme de lettres'* (he even translated some texts by Plato from Greek into German) as an economist of strong humanistic orientation, but coupled with an acute sense of the pressing issues of modern society. He used to remind his students that there exists a distinct difference between an analysis which is logically correct but unimportant (*'richtig, aber nicht wichtig'*), and between one which is 'elegant' but not 'relevant' – distinctions which have remained crucial for how economics is understood today. I greatly admired Salin's erudition in history and ancient languages, but it was clear to me that I could never achieve it myself. I also realized that I had to learn the current economic theory coming mainly from America, but at that time also from Britain.

Fortunately, there were two Professors at the University of Basle who acquainted me with this type of approach. Jacques Stohler (1930–69), while in many ways thinking in terms of the 'new welfare economics' soon recognized the potential of the economic theory of politics. He brought me into contact with the writings of Arrow, Downs, Buchanan and Tullock and therewith exerted a lasting influence on me. My closest academic teacher and mentor has been Gottfried Bombach (born 1919) who was among the leading economic theoreticians in the German-speaking countries. He has outstanding skills in detecting the crucial assumptions and consequences in a given theory and in applying them fruitfully to practical economic policy. He taught me the futility of a 'pure' theory which is not interested in the real world but rather in theoretical models and mathematical structures. (This has recently been echoed by scholars such as Colander (1991) or Mayer (1993).) We have since then shared the view that while scholars should be free to pursue whatever they desire, they should still be prepared to be judged by the standards of the science they are working in. As a consequence, purely mathematical contributions to economics have to meet the standards of the science of mathematics. If formalistic contributions are of little or no relevance to the analysis of economic or social problems, then economics scholars should refer them to their colleagues in mathematics, and not just remain overwhelmed by the mathematical sophistication.

The academic environment in which I grew up was thus committed to an analytically based, but broad, analysis of relevant social problems. Formal modelling was clearly considered secondary. When, in the course of my further education, I was confronted with the currently prevailing concept of economics, which focuses on model-building and is essentially self-contained, I was bewildered, and I still am. The rejection of virtually all knowledge and evidence from the other social sciences (including law, history, sociology and psychology) in modern, 'rigorous', neoclassical theory seems

to me a great loss. It is unlikely (or simply arrogant¹) to assume *a priori* that *all* knowledge is contained within economics as it is and that *no* knowledge can be gained from the neighbouring sciences also dealing with society.

From the very beginning of my studies, I was particularly thrilled by how the economy influences policy and how, in turn, politics influences economic activities. My first effort to transcend orthodox economics was to construct what I called 'politico-economic models' (*Modern Political Economy*, 1977). Instead of taking the economy as a closed sector which is at best influenced by exogenous economic policy, I looked at the interaction between the economic and political sectors of society. In a representative democracy, economic conditions have a major impact on the polity, because when unemployment and inflation are high, and economic growth is low, voters are dissatisfied and tend to vote against the party in power. This relationship is captured by a 'popularity' or 'vote function'. In order to survive, a democratic government must therefore achieve a sufficient share of the vote by undertaking popular economic policies which improve the state of the economy. This re-election constraint is the main determinant of the 'policy function' which describes the government's behaviour. If, on the other hand, the government is confident of winning the elections, it can indulge in ideologically oriented policies. This mutual interdependence between the economy and the polity via the popularity/vote function and the policy function was econometrically estimated for various countries (USA, Britain and Germany) with the help of my assistant and co-worker Friedrich Schneider (1978a, 1978b, 1979). In contrast to the somewhat earlier politico-economic models by Nordhaus (1975) and MacRae (1977) our model was not based on the simplistic assumption that governments maximize votes. Though our results were published in the leading economics journals, the more refined and more realistic assumptions that politicians are not solely interested in votes but – if conditions allow it – also want to pursue policies in line with their ideological views was not taken up in the following literature. For a long time, the formally elegant vote-maximization theory (which dates back to Downs (1957) and Schumpeter (1942)) was applied as a matter of course to an ever growing number of politico-economic models. Only years later was a theory developed in which government policies reflect ideological orientations (in particular Hibbs (1987)) but where the re-election constraint did not matter. The two types of theory co-existed for a considerable time, and it was only recently (see the survey by Nordhaus (1989)) that the politicians' concern for votes *and* for ideology was combined as it was in our papers of the late 1970s.

The politico-economic models were later extended to include central bank behaviour (with Schneider, 1981b). We were able to show that one of the most independent central banks of the world, the Deutsche Bundesbank, only

deviated from the government's policy in relatively minor respects. As soon as the Bundesbank's concern for price stability collided with the government's expansionary policy, the Bank gave way to the government's wishes. This does not mean that the independence of central banks is of no value, but rather that its impact is limited. Our, and for that matter all, politico-economic models were constructed and tested for parliamentary democracies, in particular for the (ideally) two-party competition of the United States. Other constitutional setups for democracy were neglected. In order to overcome this shortcoming, my co-workers and I developed a politico-economic model for a direct democracy, namely Switzerland (with Schneider and Pommerehne, 1981c). When the major political decisions are taken by popular referenda (often triggered by initiatives), the relevant opposition is constituted by the voters. The government's position strongly depends on the citizens' approval of referenda, and it therefore has to make a continuous effort to take the voters' wishes into account. Econometric tests supported this view of the direct democratic process.

Recently, I came back to the institutions characterizing direct democracy (1994b). In many respects, at least in between elections, the democratic political process can be characterized as a cartel consisting of all established politicians against the voters and taxpayers. Such a model of the 'political class' deviates from models of exploitative government (Brennan and Buchanan, 1983) because the political class also comprises politicians outside government. As soon as a politician (party) enters parliament, he or she is drawn into the coalition. The members of parliament are a well-defined group jointly reaping rents. Outsiders entering parliament find it difficult, if not impossible, to survive outside the coalition. A whole set of rules and institutions bolsters this coalition (seniority principle, party discipline). Courts, including courts of account, constrain the coalition of politicians only fractionally, since they not only lack the constitutional rights to do so but also have little incentive seriously to oppose the legislators on which they depend in many ways. The institution of popular referenda is a much more effective constraint on the politicians' coalition. Initiatives coming from the voters cannot be controlled by parliament, the agenda-setting power is with the electorate. In Switzerland, it may be seen that many initiatives at all levels of government effectively oppose politicians' rent-seeking and, more importantly, the discretionary power from which the politicians benefit indirectly at voters' expense.

A major consequence of this research on political economy is that the established theory of economic policy is no longer tenable. In that theory, the government is taken to be exogenous, maximizing social welfare, subject to the resource constraint (Tinbergen, 1956; Theil, 1968). Thinking based on politico-economic models suggests, in contrast, that given the parameters of

the system, the economy and polity evolve according to the relationships described by the respective politico-economic model. The government conveniently disregards economic policy advice which does not further its own interests, in particular to stay in power. There are only two ways to influence the course of the system: at the level of the current politico-economic process, information may be offered to the private and governmental decision-makers by economic advisers; and at the constitutional level, the institutional foundations may be changed via a consensus achievable behind the veil of uncertainty (*Democratic Economic Policy*, Frey, 1981a). Thus, for example, economic policy is bound to change if the members of parliament are elected according to the proportionality instead of the majority principle, or if major decisions must be approved in a popular referendum. Thus the first and foremost task of an economic policy adviser must be to suggest, and to help to bring about, changes in constitutional rules. This economic policy concept owes much to Buchanan and Tullock's constitutional approach (1962; see also Brennan and Buchanan, 1983, 1985). While this position is a direct consequence of accepting government to be endogenous, most present day economics has refused to leave the trodden path and to adopt the new view of economic policy: it remains to a large extent committed to the social welfare-optimizing point of view, both with respect to macro- and microeconomic issues (see, for example, the theory of optimal taxation or of optimal public pricing). This is one major reason why the policy advice professed by economists has little effect and is often completely disregarded.

The difference between an exogenous and an endogenous government was also a crucial element in my politico-economic study of the international economy (*International Political Economics*, 1984a). Here, I show how tariffs and other trade restrictions reflect political influences, and that they depend in particular on the activity of pressure groups. Producer interests and trades unions are much better organized than consumers and taxpayers. As a result, governments tend to yield to tax demands for protection of particular branches and industries while the consumer's interest in lower priced imports is treated lightly. This asymmetry of influence also holds for bilateral trade negotiations where consumers are not represented, and producers and trades unions tend to close agreements which benefit the interests present at the cost of burdening the consumers and taxpayers, for example, by promoting 'voluntary export restraints'.

My work on international political economics got me interested in the behaviour of international organizations (with Gygi, 1991; Frey, 1994c). Despite their great importance for the economic relationships between countries, they have been rarely analysed in economics. At best, on the basis of social welfare considerations, economists have suggested what international organizations *should* do. As is true for economic policy in general, such

advice has little or no effect because the relevant decision-makers in international organizations pursue their own utility which is not identical with global social welfare, and often starkly contradicts it. As is the case for all bureaucracies but in particular for international ones, output cannot unequivocally be defined because the member countries weigh the various activities in different ways, that is, Arrow's impossibility theorem applies. Rather than trying to define the output of an international organization, a process orientation is warranted. The behaviour of an international organization *emerges* from the action of the relevant decision-makers such as the directors and other employees, the national delegates and the national politicians and administrators who directly influence them, once the *rules* are given. Differences in an international organization's rules, or its constitution, taking place over time or existing between various international bodies, lead to differences in behaviour. A striking instance is the relationship between the distribution of votes and the distribution of the financial contributions among the member countries of an international organization. In the United Nations, these nations pay almost 50 per cent of the budget, while about 150 nations pay in the aggregate about 15 per cent of the budget. As all nations have the same vote, a huge majority of them find it advantageous to support higher expenditures because a few nations pay the bill, while they can expect to profit from the expenditure increase. This constitutional setting helps to explain both the continuous financial problems, as well as the inefficiency, of many international organizations. On the other hand, the reason why the UN international financial institutions, the IMF and World Bank, function more efficiently may mainly be attributed to the fact that the vote share is positively associated with the share of financial contributions. Constitutional arrangements may not be improved from outside, but can only be set by the voluntary consent of all member countries. In international politics, no (sovereign) nation can be forced to accept particular rules because no world government exists which can impose them from above. Therefore this situation is particularly apt for an application of the constitutional approach. A unanimous consent about the rules is achievable because each member has to decide behind the veil of ignorance, that is, without knowing for sure in which position it will be in future in international politics. As a consequence, each prospective member is induced to take a more objective stance than short-run self-interest, which helps to found rules which in this sense are advantageous to each of them.

Based on such a constitutional approach, I have proposed rules which, from the point of view of the citizens of the member countries, are likely to improve the performance of international organizations. In addition to 'fiscal equivalence' touched upon above (that nations with larger financial contributions should also have a larger say), other rules serving this goal include the

establishment of competition between international organizations; the possibility of member states to exit partially or totally from the organization if they are dissatisfied with its performance; the partitionability of output (so that member nations can choose between undertakings beneficial and harmful to its citizens), and the popular vote of the board of managers.

In consonance to my work on the economic theory of politics or modern political economy, I have, over the years, been closely attached to the public choice movement both in America and Europe. My stay at the Center for Political Economy at the University of Virginia in 1968 proved to be crucial in this respect. Though James Buchanan and Gordon Tullock had written their pathbreaking *Calculus of Consent* in 1962, the application of economic thinking to politics was either still disregarded among the leading economists, or otherwise thought to be immoral or ridiculous, especially at the 'liberal' leading universities of the eastern United States. While Buchanan, the founder of the above-mentioned Center, was unfortunately on leave when I was there, I spent a most stimulating and thought-provoking time with Gordon Tullock who, at that time, was not yet widely known. I have since then met Buchanan and Tullock at regular intervals at conferences and on similar occasions, and I keep benefiting from their ingenuity. As I was one of the earliest European adherents to modern political economy, I was able to participate in its establishment as a serious branch of economics. In particular, I have from the very beginning been active in setting up the European Public Choice Society which has developed nicely. Its annual meetings in various places all over Europe attract many established and promising young scholars. In contrast to the American Public Choice Society which (unfortunately) seems to have the aura of a right-wing enterprise, no such political slant exists in the European Public Choice Society.

Political economy wetted my appetite to study interactions of the economy with other parts of society, always by applying the economic approach (I collected this work in 1990). Personally, the most satisfying was the excursion into history, namely to explain why prisoners of war are sometimes summarily killed or treated most brutally, and sometimes treated well and freed quickly (with Buhofer, 1988b). In treatises written by historians, sociologists or political scientists the glaring differences in treatment are usually explained by differences in attitudes, in particular religious orientations and national cultures. We offered quite another explanation based on the relative price effect: essentially those prisoners were treated badly and killed who had little value to the captors, while prisoners who were valuable to the captors were sold and released. An essential determinant of a prisoner's value to a captor were the existing property rights. It can be shown by historical evidence that when medieval individual soldiers had a personal property right in the prisoners they took, and if a demand existed for their release (which, of

course, favoured property owners), they were treated well and were ransomed. When weapons technology changed (especially when artillery was used on the battlefield), the property rights in prisoners moved to whole units and then to whole armies, and then to the state as a whole. This reduced the individual soldier's incentive to take prisoners and to save them, which resulted in the brutal battles of the Napoleonic Wars where nobody cared for the prisoners and wounded (which prompted Dunant to found the Red Cross). The relevance of looking at the treatment of prisoners in terms of the economic approach may be illustrated by the Peasants' War where the noblemen butchered the peasants captured (they had no ransom value) while the peasants (who had most serious grievances against the nobility) generally did not kill captured noblemen but released them for a ransom (which was more profitable to them than exerting their wrath by killing their prisoners).

Another great joy is the ongoing collaboration with my long-time co-worker (and former assistant) Werner W. Pommerehne in the economics of the arts (*Muses and Markets*, with Pommerehne, 1989b). A major interest of ours was to calculate rates of return of paintings in order to deal with the often heard claim that investments into paintings (and other antiquities) have a higher monetary yield than investments in financial assets. We collected data on paintings sold at auctions all over the world from 1635 to 1987, certainly the longest data series I have ever worked with. Investment in art is subject to various kinds of risk beyond the uncertainty about future prices, in particular the risk of 'downward' revision of the attribution (for example, from a 'Rembrandt' to the 'school of Rembrandt'), outright fakes and forgeries, as well as the purely material risk of destruction. We calculated an average real rate of return from 9 400 buying and selling operations net of transaction cost of 1.5 per cent per year, with a standard deviation of the rate of return of individual paintings of 5.0. This real return is low in view of the huge sums that have recently been paid for paintings, for example, the \$82.5 mio for Van Gogh's *Portrait du Docteur Gachet* or the \$75.1 mio for Renoir's *Au Moulin de la Galette*. This may be due to the fact that compound interest and inflation tend to be underrated. Compared with investments in financial markets, the real rate of return on paintings of 1.5 per cent is rather low. The long-term real rate of return on financial investment over a comparable period is something like 3 per cent per year, with a standard deviation of only 1.7. Thus investing in paintings instead of financial assets implies a (real) opportunity loss of 1.5 percentage points per year for holders of paintings. This is a sizeable amount: the real return on paintings is half the real return one could get by buying financial assets. In equilibrium, this financial opportunity cost corresponds to the psychic return from owning and enjoying those paintings. Painting collections only seemingly are financially more successful: due to a selection effect only well-chosen collections survive while badly

chosen compilations of paintings are soon broken apart, often by the heirs of the would-be collectors.

The arts appear to me to be a particularly fruitful area in which to apply economic reasoning, not least because art is closely connected to material aspects but the sciences concerned with art (art history, sociology of the arts) are badly equipped to deal with the corresponding issues, such as the income of artists, the effect of various institutions on artistic performance (for example, the quality and quantity of artistic output of public versus private theatres and opera houses), or the public support and subsidization of the arts. Another important issue for a political economist is whether high quality art can subsist in a democracy. The research undertaken suggests to me that this is in fact the case. In Switzerland, for instance, the voters are prepared to support the arts financially to a surprisingly large extent (not less than when an 'élite' decides). An impressive example was in Basle where the voters in 1967 approved buying two paintings by Picasso – according to him the first time in history that the population directly decided on, and approved of, contemporary art.

II. METHODOLOGICAL NARROWNESS OR INTER-DISCIPLINARY PERSPECTIVE?

Economists use a well-defined approach based on rational individuals who maximize their own utility subject to resource and time constraints (see, for example, Stigler and Becker, 1977). This model of human behaviour has proved to be most useful to enhance my understanding of the areas I studied outside the economy. However, the fact that the model is so well defined and coherent has the disadvantage that influences from other social sciences tend to be rejected. Economists are proud of being able to explain nearly everything by resorting to rational man balancing the benefits and costs of his actions. In particular, psychology is often claimed to be unnecessary to understand behaviour. This is, in general, a sensible procedure because 'psychological' explanations (or what economists understand as such) are often superficial and not amenable to empirical tests. But as so often, neoclassical orthodoxy overstates its case. I am convinced that we can gain important insights from psychology, especially from its cognitive experimental social branch, which can greatly improve our knowledge of human behaviour.

Integrating economics and psychology is a difficult task. Merely superimposing a psychological theory on the economic model of behaviour is bound to lead to logical contradictions, and thus destroys the economic model's strength. Bearing this in mind, I nevertheless tried to take into account specific psychological aspects when they seemed to me to be so obvious that

they just had to be integrated. In this endeavour, I was helped greatly by a small group of social scientists who, over the last 15 years, have regularly met on a private basis in order to discuss interdisciplinary issues. It has become an intellectual circle of friends, composed of the philosopher Hans Albert, the sociologist Karl-Dieter Opp, the three psychologists Kurt Stapf, Klaus Foppa and Wolfgang Stroebe, and another economist, Willi Meyer.

One of the efforts for using psychological concepts to improve economic analysis takes issue with the neoclassical presumption that an individual's knowledge of his or her possibility set is restricted only by the cost of information. In my view, in addition to the objective and subjective (that is, the individual's information about the objective) possibility set, there is another quite unconnected one, which Klaus Foppa and I have termed *ipsative* (with Foppa, 1986a; Frey, 1988a) and which relates exclusively to one's own person. An example will clarify the difference: people are subjectively quite well informed about the objective probability of divorce (about 50 per cent in Western cities), but when it comes to their *own* marriage, almost all believe in a far lower probability. The fact that ipsative probabilities deviate systematically and strongly from subjective and objective ones has been empirically established for many aspects of life (for example, Weinstein, 1980). Interestingly enough, the ipsative possibility space is sometimes larger and sometimes smaller than the subjective and objective one, be it by design or by nature. This has interesting implications, for example, the behaviour of firms (with Heggli, 1989a), as well as for individuals. It helps to explain why persons against all odds decide to enter professions with (average) low incomes but with some exceedingly high income earners (Adam Smith noted this behaviour for the case of lawyers and considered it to be a case of irrationality). Instances of systematically upward braced success expectations occur regularly for artists: what young dancer would not strongly believe that she will be a famous prima ballerina – and in fact at best ends up as an unknown dancer in the *corps de ballet* and, even more likely, gives up the career without ever having been employed, but with enormous investments of time, money and effort, and moreover lasting health problems? Such behaviour *can* be accounted for by traditional neoclassics – indeed, *all* behaviour can be embraced by it in some way or other because it is highly flexible while still maintaining its overall consistency. The question is, however, whether the standard account makes sense and provides deeper insights than we already have, or whether it is not more efficient to consider what other social sciences have to contribute.

Another attempt to profit from psychology has been provoked by the observation that economists do not, in important instances, behave according to their own theoretical view. It seems to me that most academic economists not only claim to be, but indeed are, strongly affected by intrinsic motivation:

they do research because they like this activity, and many would still do it even if they were paid much less. So the main incentive for research is 'idle curiosity' and not monetary gain. However, when economists analyse the labour market, they take it for granted that in order to induce people to work more or better, they should be rewarded either by a monetary incentive or be monitored more intensively (for example, Milgrom and Roberts, 1992). Thus standard economics assumes as a matter of course that, 'if people were not paid they would not work' (Lane, 1991, p. 349). Monetary and other material incentives may well result in a *reduction* of effort because the person's intrinsic motivation is negatively affected. In psychology, this effect is known as 'the hidden cost of reward' (for example, Deci and Ryan, 1985), but in my view it also extends to the effects of outside controls, in particular to regulations. This 'crowding out' of intrinsic by extrinsic motivation is relevant for a great many economic activities and may, for instance, negatively affect work morale or environmental ethics (Frey, 1992b, c; 1993b, 1994d).

Crowding-out intrinsic motivation by monetary rewards constitutes one instance of the limit to use prices for the allocation of resources. I find this subject of great importance because present day (neoclassical) economics tends to favour the application of prices in many new areas (for example, in the form of tradable permits and effluent charges to reduce environmental degradation, of road pricing to regulate traffic, or of auctioning to ration a fixed number of immigrants), but non-economists, other scientists and non-academics often tend to oppose strongly the application of these measures (Frey, 1986b). One of the reasons may be that non-economists are aware of the possible destructive effect on intrinsic motivation (for example, on environmental ethics); another reason may be that they consider the price mechanism to be unfair. An extensive survey carried out in Switzerland and Germany (with Pommerehne, 1993c) found, indeed, that even in a situation of a clearly defined excess demand for a good, people strongly prefer an allocation according to a traditional principle ('first come, first served') and, according to an administrative principle (allocation by public servants), to a rationing via a price increase.

Cognitive experimental psychologists (for example, Kahneman, Slovic and Tversky, 1982) as well as some economists (for example, Thaler, 1992) have convincingly established that individuals do not follow the principles of rationality as established by Von Neumann and Morgenstern (1944), and therefore systematically violate the economic model of decision-making under uncertainty. People do not maximize subjective expected utility. As usual, orthodox neoclassical economists either rejected or simply disregarded these findings (the model of subjective expected utility maximization is still used as a matter of course in models involving uncertainty). Those scholars who do take it seriously (for example, Machina, 1987) seek to

integrate the anomalies into economic theory by generalizing the individual utility function to be maximized, so that the previously 'anomalous' behaviour becomes normality. In joint work with my assistant and co-worker Reiner Eichenberger (1989c, d, 1992a, 1994a), we pursue a different line of research and argue that individuals react to anomalies, for instance by imposing rules upon themselves and, more importantly, by establishing institutions which prevent them from falling prey to anomalies. An example are people who play excessively in casinos: many of them who are aware of their weakness of will guard themselves by taking a limited amount of money with them. They may also rationally favour laws prohibiting excessive players to enter casinos. We thus postulate that individuals are not fully rational at each moment of time, but that they are sufficiently rational to try to take measures against their irrationalities and weak spots. Individuals are in this sense seen to be 'superrational'. The effort to guard oneself against one's weaknesses constitutes a so far neglected reason for institutions to emerge. Moreover, it suggests that empirical observations are not sufficient. It may well be that no anomalies are observed in real life, but the reason is that institutions of various sorts prevent their occurrence. If the institutions did not exist – if they were suppressed – anomalies of behaviour would become apparent. An equally important aspect of anomalies is that there exists an incentive for firms to exploit them which tends to increase their occurrence. It follows that anomalies are likely not to be erased by a learning process of their victims, but rather that there is a mutual interaction (or game) between the victims and the exploiters of anomalies. This analysis of behavioural anomalies differs basically from how they are treated in the current literature. Some economists focus on the type of anomalies identified (mostly) by experimental psychologists and consider their consequences for economic decisions. We argue that such anomalies are not directly relevant to real life because they are transformed by endogenously emerging institutions as well as by the actions of profit-seeking exploiters. The emphasis shifts away from the 'psychological anomalies' as such and concentrates on the behaviour of individuals and firms who react to such irrationalities with the result that they take an often completely different form, and in a few instances (for example, in a perfectly competitive environment in which only fully rational actors can survive) they disappear altogether. Our approach also contrasts with the axiomatic approach because the latter includes in the generalized utility function of the actors all those effects which are due to both the emergence of institutions and the activities of profit-seeking exploiters. Such an integration into the actor's utility function disregards individuals' and firms' reaction to anomalies or, alternatively, assumes that the institutions which help to deal with the anomalies, as well as the behaviour of exploiters, are

in an unchanged equilibrium, which means that what has to be explained is assumed as given.

III. FORMALIZATION OR SUBSTANTIVE NOVEL IDEAS?

The use of mathematics has surely become sacrosanct in economics; almost no one is willing to argue against it, at best concern is raised about its excesses. This should be compared, for instance, with the use of historical knowledge: no economist is blamed if he or she is unaware of historical precedents. In many branches of economic theory, empirical facts or the real world are irrelevant because the research is immanent to the existing theory; it is self-referential. What matters is solely whether a phenomenon is consistent with standard theory, and what moves the field is exclusively its internal dynamics.

Formal rigour has thus become a value for itself in orthodox economics instead of being a way to provide additional insights. The results are all too often purely self-contained exercises around some abstract model and usually make a minor or, more often, no contribution at all, to our knowledge about society. This evaluation is shared by many senior economists who, towards the end of their careers as professionals, become aware that the insights provided by the formal theory which they pursued and supported at a younger age, are rather minor. A recent example is Franklin Fisher (1989, p. 123) who bluntly writes: 'There is a strong tendency for even the best practitioners to concentrate on the analytically interesting questions rather than on the ones that really matter... The result is often a perfectly fascinating piece of analysis. But so long as that tendency continues, those analyses will remain merely games economists play'. Similar statements have been made by many insightful economists, for example, by Kolm (1988), Colander (1991) and Mayer (1993). They are often taken as evidence that excessive formalization will regress, and that economists will again tackle real-world issues instead of theoretical riddles and niceties. Though I obviously share the view that economic formalism has gone astray, according to my analysis (see the concluding section of this paper) this is but wishful thinking. The market for academic economists signals clearly that formal theory matters foremost. The views of today's senior economists which are revealingly uttered on special occasions, particularly formal presentations as presidents of some professional organizations, are of little or no relevance for younger, career-oriented economists. They listen to such exhortations with due respect and some amusement – and put them aside immediately. Young scholars who want to be successful in the theory-prone American market are well aware that for-

malism counts, and not the real world. This has been clearly revealed by a survey among graduate students in top US economics programmes (Klamer and Colander, 1990, p. 18). Only 3 per cent of them believe, 'having a thorough knowledge of the economy' to be 'very important' for professional success, while 65 per cent think, 'being smart in the sense of being good at problem-solving' is what matters, and 57 per cent state that, 'excellence in mathematics' is very important.

It is not easy to judge how much harm full mathematization has done to economics because it is always handy to argue that a precisely formulated argument is superior to a loose one. But consider the following argument: 'Coase refrained from stating a general 'theorem' underlying his examples. Quite wisely ... Coase chose to present the profession with a 'Coasian insight' and left formal details to be worked out by others. Anyone who only reads about the Coase theorem without reading Coase's insightful discussions of examples has missed the best of the story' (Bergstrom, 1989, p. 1157). I am even prepared to go a step further. The emphasis on rigour tends to trivialize or even kill important novel ideas, or as Lord Kelvin put it, they may 'trap the mind'. There may even be value in ambiguity because it works against a tunnel vision (see Mayer, 1993, p. 124). A case in point is Simon's (1957) concept of 'bounded rationality'. Orthodox economists were quick to reformulate it simply as 'utility maximization subject to an (additional) formal constraint'. Such an interpretation is not fruitful, as it suggests that Simon is unaware of utility maximization. Indeed, he made it clear (Simon, 1978) that bounded rationality has to do with cognitive limitations of the human brain, which is quite different from simple informational constraints. He further argues that the concept of outcome-oriented rationality has to be substituted by procedural rationality. This constitutes a completely different point of view focusing on the rules and conditions under which a decision is taken.

In my work (see, for example, Frey, 1990) I have endeavoured to emphasize substantive ideas, not least because I feel that formal models are produced by many economists, while ideas seem to be scarcer. I have also been motivated by the conviction that important aspects of society, and in particular relevant economic policy issues, can be dealt with by skilfully using the basic principles of economics – above all that a price increase reduces the demand for goods and activities, and increases its supply, that is, the relative price effect. I am not the only one to feel this way. Herbert Stein, successful Chairman of the (American) Council of Economic Advisers, stated that, 'most of the economics that is usable for advising on public policy is at about the level of the introductory undergraduate course' (see Hamilton, 1992, p. 62). Everyone who follows the pieces of advice proffered by economists to meet one of the most important challenges of today, the development of the

ex-communist countries, must be well aware that this view holds more generally. This does not imply that economics has nothing to contribute; I believe the opposite is true. The simple economic ideas, in particular the relative price effect, are surprisingly little known, little accepted and widely misunderstood in the general population, among politicians and scholars of other sciences. What I argue is that much of 'advanced' economic theory contributes little to economic policy-making (not even in the longer run), and that economists would do a better job by showing how the simple principles of their science can improve the human lot. Nor are such applications trivial – again the opposite is true. Indeed many economists who excel in formal theory are unable to apply *any* economics to real-world issues. Critics such as Leontief or Galbraith claim that, 'departments of economics are graduating ... *idiots savants*, brilliant at esoteric mathematics yet innocent of actual economic life' (Colander, 1991, p. 41). This is often visible when such scholars are confronted with issues of the economics of the family, the economics of crime or the economics of art. All too often they forget in these instances all economics, and argue as complete laypersons would.

IV. AMERICAN OR EUROPEAN INFLUENCES?

My life as an economist is quite typical for a European of my own and the following generations (I was born in 1941). I am a frequent visitor to the United States. After my doctorate (which I took at the University of Basle in 1965) I spent two years in the United States, first as a Visiting Lecturer at the University of Pennsylvania's Wharton School, then as a Visiting Scholar at Princeton and Stanford Universities. Later stays as a Visiting Scholar took me to Yale, Michigan, Colorado, Berkeley (with Oliver Williamson) and the Hoover Institution, but the culmination was my stay as a Visiting Research Professor of Economics at the Graduate School of Business at the University of Chicago in 1990. The atmosphere there is pure intellectual delight, and I have profited enormously from frequent discussions with George Stigler (who was my host), Gary Becker, Harold Demsetz (who was also visiting at the time) and James Coleman (the rational choice sociologist), as well as others such as Sam Peltzman, Ronald Coase and Richard Posner. The first paper I gave at Stigler's famous seminar was not well received (only later did I find out that almost *no* paper is well received!), but I learned a lot from the discussion. The second paper, in Becker and Coleman's seminar, was more successful. Chicago provided me with the most lively and rewarding academic surroundings one can wish for. I was and am deeply impressed, especially by Gary Becker's fabulous mind, and I owe a great deal to his support regarding my efforts to introduce psychological elements into economics.

Another 'home base' in the States has been the Public Choice Center led by James Buchanan and Gordon Tullock and located consecutively at the University of Virginia, the Virginia Polytechnic Institute and the George Mason University. Over the years, I have also had close contact with Mancur Olson and Dennis Mueller, now both teaching at the University of Maryland. They also had a strong and continuous influence on my thinking.

From what has been said it becomes clear that I greatly admire American economists for their professionalism, engagement and culture of exchange and discussion – all aspects that used to be a characteristic of European universities, but are now sadly lacking at most academic institutions. I also appreciate the great hospitality and helpfulness I encountered virtually everywhere in the United States.

I am, nevertheless, irritated about some aspects of American economics. One is that American scholars cannot completely abstain from what Lipset (1992, p. 2) describes in the following way: 'American ... ideological conflicts are conducted as battles between good and evil, and such conflicts engender demands for correctness'. Needless to say that the 'ideology' I think of is orthodox neoclassics. I recognize that there are some groups of American economists who are heterodox, but neoclassical economics in a rather narrow sense has almost completely captured the leading US universities and is simply identified as the only 'good' economics. Any new idea or any new aspect only has a chance of being heard, let alone of being integrated, if it is formulated in a neoclassical mode, including its terminology, and if it is cast in terms of a mathematical model. While this gives a kind of formal coherence to economics, it prevents innovation and flexibility. It moreover drives economics increasingly away from considering empirical issues of the real world.

Another of my irritations with American economists is connected with the fact that they rarely take into account works by Europeans, even if they are, or could be, aware of them. This neglect is certainly not a matter of bad will, but the result of the peculiar environment in which American scholars live. A young scholar who wants to pursue his or her career in America, does, in general, well to refer to American economists because that shows competence and is part of an exchange process of mutual quotations. Moreover, as there exists a more or less informal network among leading American universities where virtually all interesting papers are presented much before they are published, the transaction cost of quoting such indigenous papers is much lower than having to search for non-American work in the particular area. An unfortunate result of this situation is that ideas and concepts well known in other parts of the world seem to be unknown to Americans, until all of a sudden they reinvent them without being aware that elsewhere they are well known. An example is the concern for 'reputation' which in the 1980s flooded

American economics, a phenomenon which in societies less geared to short-run monetary exchanges and evaluations than America, has always been of central importance.

A third irritation is with European scholars: instead of drawing on their own (often very valuable) traditions, experiences and institutional conditions, they all too often mindlessly copy the Americans, the result being pale imitations. Many academic economists over here have been so strongly influenced by American economics (they have been raised with their textbooks, and their only foreign stay for professional purposes has been in the United States) that they scarcely know the conditions in their country but nevertheless directly apply the newest fads current in America. These are often (at best) relevant only for the institutional conditions obtaining in that country. Not few German economists, for example, know more about the Federal Reserve's than about the German Bundesbank monetary policy, though the latter is of central importance not only for Germany but also for the European Union.

These observations and irritations provoked my interest in the analysis of economics as a science. With my co-workers Werner Pommerehne, Friedrich Schneider and Hannelore Weck-Hannemann (1982) and Pommerehne, Schneider and Gilbert (1984b, c), I carried out an extensive survey inquiring as to how far and in what respect American economists' opinions on theoretical and applied issues deviate from European economists' opinions. We did find systematic and interesting differences, one being that Americans tend to follow the 'textbook mode' prescribed by orthodox neoclassics more closely. The French and Austrian economists, on the other hand, who are influenced by a long tradition of government intervention into economic affairs, are rather sceptical about standard neoclassical theory. They do not share the basic distrust of government which is typical of orthodox American economists. (It should be noted that the writings of such French and Austrian scholars are almost completely unknown in American-dominated 'international' economics. What is known are the few economists in France and Austria who follow the path of standard theory.)

Reiner Eichenberger and I have developed a theory (1992a, 1993a) where we postulate that the large and highly competitive market for economic research existing in North America induces scholars to pursue abstract and non-institution-specific work, while the often highly protected national market for economists in Europe induces more institution-oriented, applied research. Americans have clearly dominated research and citations (with Pommerehne, 1988c). According to the number of citations in the *Social Science Citation Index* over the period 1972-83, the United States and Canada provide 72 per cent of all eminent economists while only 25 per cent are Europeans (not to speak of other continents!). The Nobel Prize reveals a

similarly massive American dominance: between 1969 and 1993, 23 out of 34 (or 68 per cent) of Nobel Prize winners were living in America. As European professors of economics are much less pressed by the need to publish and to be cited, they have more incentives to embark on a political career than their American colleagues. They have often been very successful, having a large number of presidents, prime ministers and other members of government among their ranks. In Europe, the top rank of a Prime Minister was, after World War II, for instance achieved by the following *Professors of Economics*: Raymond Barre in France, Viggo Kempmann in Denmark, Ludwig Erhard in Germany, Andreas Papandreu in Greece, Jelle Zijlstra in the Netherlands, Kare Willoch in Norway, Antonio Salazar and Anibal Cavaco Silva in Portugal, Harold Wilson in the UK and Tansu Ciller in Turkey. In many European countries, it is quite common for professors of economics to be government ministers. To give an example for a particular country and date: In December 1991 in the Netherlands Jacob E. Andriessen (University of Amsterdam) was Minister of Economic Affairs, Jo Ritzen (University of Rotterdam) was Minister of Education and Science, and Jan P. Pronk (University of Amsterdam) was Minister for Development and Cooperation; moreover Wim Duisenberg (University of Amsterdam) was President of the Dutch Central Bank. Indeed, many professors of economics in Europe have high positions at central banks and at the level of states. In Germany, for example, in December 1991 Herwig Haase, Reimut Joachimsen, Hans-Jürgen Krupp and Georg Milbradt were ministers in German *Länder*, while Helmut Hesse, Norbert Klöten and Kurt Nemetz were Presidents of the central banks of particular *Länder*, and Otmar Issing was a member of the directorate of the Bundesbank. It is noteworthy that (with few exceptions) these politically most successful professors of economics are not even mentioned in Blaug's (1986) *Who's Who in Economics* which is based on the number of citations, that is, an evaluation geared to the American scholarly environment. Despite the fact that these European professors of economics are not considered among the 'prominent' economists according to American standards, they have exerted much influence on policy-making. It may well be that they have had more impact on society than their more academically oriented American colleagues.

My effort to understand better the relationship between American and European economics has not been confined to the abstract research level. I have instead actively tried to counterbalance the American influence by visiting as many European universities as possible and to experience what kind of economics is being pursued there. Right after taking my doctorate, I spent six months at Christ's College in Cambridge under the guidance of James Meade, Joan Robinson and Nicholas Kaldor, who were at that time the most dominant figures at Cambridge. They impressed me personally, but their kind of

theorizing did not exert any lasting influence on my own thinking. I also visited 'the other place' (Oxford) for extended periods as a Visiting Scholar at Nuffield College. The highlight was my appointment as a Visiting Fellow at the venerable All Souls College, where I greatly enjoyed the scholarship in the almost medieval atmosphere (as well as the good food and the excellent wines!). I have also had the opportunity to stay as a Visiting Professor at various European Universities outside the Anglo-Saxon area, for instance at the Institute for International Economics, at the Institute for Advanced Studies in Vienna, and the Universities of Valencia and Groningen and at the Institut de Science Politique de Paris.

My mother tongue being German (or rather Swiss German) and my academic teachers Salin and Bombach both originating from Germany, I have had close links with the economic profession of that country. My first full professorship (1970–77) – in public finance – was at the newly founded University of Konstanz, which in those years was a most lively and enjoyable place. I was but a few years older than my assistants and students, because I was only 30 when I was elected (which is rather young for German standards). I was soon able to assemble a group where we intensively discussed modern economics and, in particular, political economy and other applications of economic reasoning to social problems. The then members of this group were of outstanding quality: virtually all of them later became full Professors of Economics in different countries: Charles B. Blankart (Berlin), Werner W. Pommerehne (Saarbrücken), Friedrich Schneider (Linz), Gebhard Kirchgässner and Manfred Gärtner (both St. Gallen) and Jürgen Backhaus (Limburg). The contact within this group has remained close; not only do we meet regularly at conferences, we also do joint research resulting in co-authored publications. My connection with German academic institutions was strengthened by my stay at the Institute for Advanced Studies (Wissenschaftskolleg) in Berlin, which proved to be a particularly fruitful time for interdisciplinary research, especially with psychology.

My professional relationship with Switzerland, my native country, has, by necessity, been less close than is the case with economics professors mainly working in Switzerland. I come from a Swiss family whose origins can be traced back to the 15th century and whose male line has always resided in the German-speaking part of the Jura. While my grandfather was a poor peasant, my father was a true entrepreneur who moved to the city of Basle and became quite well-to-do by hard work, a skill which was conveyed to us children (I have a younger sister and an older brother) as a matter of course. My mother gave us a keen sense of internationality, especially for the French culture, as well as of the importance of education. My brother René was the first member of the family ever to go to university. With a lag of one semester I followed his example to study economics. René is today a Professor of

Economics at the University of Basle, specializing in research on environmental and regional economics. Since 1970, we have jointly been editing the academic journal *Kyklos*, which, with an edition of nearly 4000 copies and a good quality rating, developed well to our satisfaction. While René has concentrated his work on Switzerland, I have oriented myself more internationally. My feelings about my country are somewhat mixed: on the one hand I consider it an excellent home base with good working conditions, which is very valuable to me; on the other hand I don't approve of its tendency to isolate itself from larger currents; also, many academics and students are rather complacent and consider the Swiss way of life – at least implicitly – as quite superior to any other.

When, in 1977, I left Konstanz to take over a Chair of Economics (especially on the theory of economic policy) at the University of Zurich, I was fortunate enough that Werner W. Pommerehne, Friedrich Schneider and Gebhard Kirchgässner joined me. After they had left to take up Chairs of their own I was able to assemble a new group of young scholars interested in academic endeavours and non-market economics. Among them are Hannelore Weck-Hannemann, who also became a Professor of Economics, Beat Gygi and Angel Serna, who both became Economics Editors with the leading Swiss newspaper *Neue Zürcher Zeitung*, and Reiner Eichenberger, with whom I have jointly worked and published on issues in psychological economics and the analysis of economics as a science.

Over the years, my thinking has been strongly influenced by a conference which took place once a year, the seminar on 'Analysis and Ideology' in Interlaken, Switzerland. It was initiated and organized by Karl Brunner, a Swiss economist who has gained fame in the United States as one of the founders of 'monetarism'. He taught me the value of hard discussions and academic competitiveness, and it was there that I first came into contact with the comparative analysis of institutions and its leading proponents, such as Armen Alchian, Harold Demsetz, Serge-Christophe Kolm and Siegwart Lindenbergh. Unfortunately the Interlaken conference, which had a very beneficial impact on many younger European scholars, was discontinued in 1992. The Wallerfangen conference on 'New Institutionalism' run by Rudolf Richter and Eirik Furubotn has now taken the role of the Interlaken conference. The papers presented, and perhaps even more so the discussions taking place, at this conference still influence my thinking considerably. This is not least due to the excellent participants, among them James Buchanan, Ronald Coase, Oliver Williamson, Richard Posner and Douglass North.

V. OUTLOOK

My dissatisfaction with the general state of economics has become obvious. I criticize economics because I am fond of it and because I believe that it can contribute much to improve the world we live in. Moreover, there are certainly many interesting contributions within economics, but it seems to me that empty formalism, irrelevance and simply boring articles have become increasingly prominent compared to exciting work introducing novel ideas in a palatable way. An increasing number of economists seem to take it for granted that all that counts is to refine further some existing model. They do not desire nor attempt a better understanding of the society we live in, nor develop suggestions for its improvement. This retreat seemingly has an advantage: 'By applying their analysis to issues no one except economists care about, economists avoid upsetting people' (Colander, 1991, p. 5). Economics has moved a large step away from what motivated most earlier economists – and to what I subscribe myself – namely a curiosity about the world we live in (and not only in purely formal relationships) and a passion to improve upon it. If our analysis of the market for economics and economists (with Eichenberger, 1993a) is correct, this tendency will grow, and not decrease, over the coming years. The main reason is the extension of the market from a national to a European one and from there to a global scale favouring abstract and formal work and discouraging problem-oriented and institutionally applied work.

Despite my dissatisfaction with much of modern economics, my feelings about it are much more positive when I compare economics to other social sciences, in particular to traditional (that is, non-rational choice) sociology and political science. To me, these sciences largely lack a systematic approach; analysis is too often substituted by verbosity. By taking part in a considerable number of conferences among sociologists and political scientists, I have learned that there is an unfortunate tendency to pontificate on one's views instead of having a direct, well-pointed discussion of controversial issues as among economists. This shortcoming has, however, its good sides, too. The atmosphere is open enough to admit all kinds of views and approaches, including the economic (rational choice) one. As a result, many of the most imaginative economists are now located in the neighbouring social sciences and not in departments of economics proper. Examples are, among many others, Ronald Coase in a School of Law or Oliver Williamson in a Business School. Moreover, much of the most fascinating work in economics is now being carried out by sociologists (for example, James Coleman or Jon Elster), political scientists (for example, Barry Weingast or Douglas Hibbs), psychologists (for example, Herbert Simon, Amos Tversky, Daniel Kahneman and Robin Dawes), lawyers (for example, Richard Posner)

and by economists with joint appointments (Gary Becker, for instance, is also a Professor of Sociology, and Gordon Tullock is also a Professor of Political Science). It should be added that although economic orthodoxy is extremely well established and still growing, there has always been room for scholars expressing deviating views, such as Albert Hirschman or Tibor Scitovsky of the older, and Robert Frank or Richard Thaler of the younger, generation. As long as these options remain open, economics remains a fascinating social science to which I am happy and grateful to belong.

NOTES

- * I am grateful to Iris Bohnet, Reiner Eichenberger, Claudia Frey, René L. Frey and Hannelore Weck-Hannemann for helpful comments.
1. See McCloskey (1991), pp. 145-9, and many other present day critics of economics.

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