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SCIENCE AND IDEOLOGY IN ECONOMIC, POLITICAL AND SOCIAL THOUGHT

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Abstract:

This paper has two sources: One is my own research in three broad areas: business cycles, economic measurement and social choice. In all of these fields I attempted to apply the basic precepts of the scientific method as it is understood in the natural sciences. I found that my effort at using natural science methods in economics was met with little understanding and often considerable hostility. I found economics to be driven less by common sense and empirical evidence, than by various ideologies that exhibited either a political or a methodological bias, or both. This brings me to the second source: Several books have appeared recently that describe in historical terms the ideological forces that have shaped either the direct areas in which I worked, or a broader background. These books taught me that the ideological forces in the social sciences are even stronger than I imagined on the basis of my own experiences.

The scientific method is the antipode to ideology. I feel that the scientific work that I have done on specific, long standing and fundamental problems in economics and political science have given me additional insights into the destructive role of ideology beyond the history of thought orientation of the works I will be discussing.

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1. INTRODUCTION

This paper has two sources: One is my own research in three broad areas: business cycles, economic measurement and social choice. In all of these fields I attempted to apply the basic precepts of the scientific method as it is understood in the natural sciences. I found that my effort at using natural science methods in economics was met with little understanding and often considerable hostility. I found economics to be driven less by common sense and empirical evidence, then by various ideologies that exhibited either a political or a methodological bias, or both. This brings me to the second source: Several books have appeared recently that describe in historical terms the ideological forces that have shaped either the direct areas in which I worked, or a broader background. These books taught me that the ideological forces in the social sciences are even stronger than I imagined on the basis of my own experiences.

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The idea that the scientific method, or more generally rationalism, the unfettered use of human reason, could ban irrationality in human affairs, leading to a sane and prosperous world in which humans would develop their full potential, has occurred several times in history. It was evident in the Golden Age of Greece and again became the moving force of the Enlightenment. But the Enlightenment ended in the totalitarianisms and wars of the Twentieth Century. Following the Second World War each of the competing political systems led by the United States and the Soviet Union held out the promise of a golden future based on the advance of science and a rational social organization, if only the correct model advocated by either side were adopted. Both sides vastly expanded higher education to train armies of scientists and technicians charged with achieving the stated objective.

Following the collapse of communism, capitalist democracy exemplified by the United States and the associated neo-liberal ideology, appeared to have achieved the final triumph. Francis Fukuyama (1992) announced *The End of History*. However, as the present article is being written in mid-2006, the shine is already off the neo-liberal ideology. The principal protagonist of that ideology, the United States, is in comparative decline. The populations of the traditional western industrialized nations have lost the élan that derived from an optimistic view of the future. They look towards the future with reduced expectations and they question the moral and intellectual capacities of their politicians to lead them wisely.

I believe that a key to understanding the situation in which the world presently finds itself lies in an understanding of the evolution and current state of the social sciences. They have experienced an explosive growth in the number of university and other professionals, in the volume of journal and book publications and, particularly in the case of economics, an enormous influence on policy. This expansion was the main vehicle by means of which, it was hoped, a more rational and reasonable social order would be attained. This hope is an example of a failed belief in the ideology of *scientism*. The nature of this ideology and how it evolved in the social sciences is a major topic of this paper.

The key concepts of this paper are ‘science’ and ‘ideology’. The meaning that is often attached to these terms is that ‘science’ is what we do; ‘ideology’ is the opinion of those we disagree with. By ‘ideology’ I mean a rather broad set of beliefs held by a group of people that involves a partial or distorted view of reality, such that the omissions or distortions serve the interests of those adhering to the ideology. An ideology, to be believable, must describe at least a part of reality fairly accurately. Most adherents to an ideology are sincere; they are consciously unaware of the distortions involved or of the self-interested motivation.

The ideologies most often considered are political ideologies and these are central also to the present paper. Political ideologies have existed in great variety, but for many purposes it has proven useful to classify them along a Left/Right ‘political spectrum’. Accordingly, I will also refer to the political Left, or political Right, fully aware of the fact that this is a very crude classification. Generally, the ideologies of the Left and the political parties propagating these ideologies have stood for a strong state actively pursuing redistributive and egalitarian policies. The ideologies of the right have emphasized the importance of individual freedom, limited government and the efficiency of markets. Naturally, the more marginalized segments of society have tended to the Left, the dominant elements to the Right. At times I will use an alternative terminology, by referring to the broad spectrum of the Left as ‘socialism’ of and to that of the Right as ‘neoliberalism’; again recognizing that many on either side of the spectrum would refer to themselves differently.

Another ideology that is central to the present discussion is *scientism*. It has two related aspects: One is an exaggerated faith in the ability of science to solve virtually all individual or social problems and an associated devaluation of all forms of discourse that are not considered to be scientific. At the same time, the conception of science involved is often a highly superficial one, focusing on surface appearances such as the formation of university departments, professional journals, a specialized language or, particularly in the case of economics, the use of mathematics.

To define science, I will use a negative definition due to the philosopher and historian of science Ravetz. He defines immature or ineffective fields of inquiry as those that do not have agreed upon criteria for establishing factual truth. He views the social sciences as falling in this category, an assessment that I share.¹ The implied criterion for a genuine or functioning science is that it does have such criteria and hence is able to acquire a growing stock of factual knowledge.

I have had some hesitation in using the terms ‘political science’ and ‘social science’ since I don’t believe that the fields usually designated in this manner are scientific by any reasonable definition of the term. I decided to continue using them, since it is awkward to change well established terms. I use ‘social science’ in an inclusive sense to refer to all fields that study aspects of society, also economics that is sometimes excluded from this definition.

The paper is unusual both in content and style. I have worked within what I consider to be the economic mainstream, at least from a long run perspective. Within that framework I have tried to make strong hypotheses that explain much on the basis of a few simple assumptions. I was never interested in descriptive detail for its own sake. In this paper I use a similar approach in dealing with the history of ideas. My aim is to identify central tendencies, not to give a full account of the thought of any of the authors mentioned.

Analyzing the role played by ideologies is necessarily an exercise in the history of ideas. When I began my study of economics, the history of economic thought was still a required item in most degree programs in economics; it has since disappeared from these. Quite generally, the social sciences have become ahistoric while their ideological content has increased. The two trends are related; if one does not reflect where current, unreflected assumptions came from, one does not realize their ideological character. I will illustrate this with two examples that are important in the context of this paper:

Today’s students of collective choice learn that preferences are formalized as orderings over alternatives; that the central result obtained is Arrow’s ‘impossibility theorem’, which states that no rational aggregation of preferences is possible; and that a large literature has found no convincing escape from the stark conclusion of that theorem. The ordering

¹ Ravetz (1971, Ch. 14). The distinction between the natural and the social sciences in this respect is not absolute, but rather one of degree. There are no criteria for the unfailing and instantaneous determination of truth. Nevertheless, the extent to which a lasting consensus on truth is established in the natural sciences contrasts sharply with the social sciences.

assumption appears to be even more fundamental than the axioms of Arrow, or of other theorists, since it provides the very formalism by means of which they are expressed. What the student does not learn is that the germ of the assumption can be found in the ideological battle waged by the marginalists against classical economics and its philosophical backdrop of utilitarianism. It was resurrected by a later generation of economists to provide ammunition in their ideological struggle against socialism. Arrow's work, the culmination of this line of argumentation, was formulated at the RAND Corporation in the context of the Cold War ideological battles. Neither is the student told why collective choice theory has attempted to build on the negative result of Arrow rather than on Harsanyi's demonstration of the possibility of a consistent cardinal theory.

My second example is contemporary econometric methodology that has as its central paradigm statistical inference. The task of the econometrician according to this view is to discriminate among alternative hypotheses on the basis of statistical inference. As different tests were developed, there ensued veritable testing orgies: testing for causality, testing for chaos, testing for cointegration and testing for real or monetary shocks, to mention only the most popular ones. This entire program has been singularly unsuccessful; I don't know of any empirical regularity that has been established in this manner. Since the natural sciences do not use statistical testing to any extent,¹ the question arises as to why this should be different in economics. A possible answer is that the large and mathematically sophisticated literature developed around the testing approach became an end in itself. This would be an instance of scientism. While developing the sophisticated testing procedures, economists largely lost interest in understanding how the data to which the tests were applied were constructed. This is the exact opposite of the emphasis that prevails in the natural sciences on a precise knowledge of the data generating process and the resulting properties of the data. In natural science, the testing of theories generally involves no more than the visual comparison of plots of predicted against observed values. The reason why this is possible is that science looks for strong explanations that explain a lot, while assuming little. It would be very difficult to gain any insight into a phenomenon that has many causes, each explaining only a small part. Indeed it is questionable that we would even talk about a specific phenomenon in this case, since with different constellations of causes, quite different observations would result.

While I have tried to make various arguments plausible, it must be recognized that the paper is at a secondary, not a primary level of research. Much of the more detailed evidence on which the arguments are based is found in the cited sources. Can the natural science methodology be applied in the social sciences? Sections 2, 8 and 9 give a number of examples that demonstrate that the answer is "yes". Here I quote a passage from Mancur Olson (1982) that states his methodological credo; it is precisely that of the natural sciences.

We can begin to have confidence in an explanation when a large number of phenomena are explained – that is, when the explanation has "power" – and explained "parsimoniously". Since it is costly to acquire and remember information, parsimonious or concise and simple explanations must, other things equal be preferred; the principle of Ockham's razor-that any inessential premises or complexities ought to be cut out of an argument-has been useful to philosophers, mathematicians, and scientists since the Middle Ages. But when the parsimony of an explanation is taken into account along with its power, it bears also on the likelihood that it is true. For one thing, if the explanation has both power and parsimony it is hard to see how its author could have resorted to unique or distinctive features to explain the outcomes. For another, when a simple explanation explains a great deal-when the ratio of power to parsimony is high-it is improbable that mere chance could explain its success. (p. 12).

The persuasiveness of a theory depends not only on how many facts are explained, but also on how diverse are the kinds of facts explained. Darwin's theory offers insights into the origin and evolution of creatures as diverse as whales and bacteria, and this makes it more convincing than if it could explain only mosquitoes, however many millions of mosquitoes might be satisfactorily explained. If a theory explains facts of quite diverse kinds it has what William Whewell, a nineteenth-century writer on

¹ Statistical testing should not be confused with the use of probability. There are many probabilistic theories in natural science.

scientific method, called “consilience”. Whewell argued that “no example can be pointed out, in the whole history of science, so far as I am aware, in which this consilience...has given testimony in favor of an hypothesis later discovered to be false”. (p.13).

A final caveat concerns the basic sources on the history of ideas that I have used. In no case have I tried to review these works. Instead I have taken from them what I found useful for the purpose of my argument. I have tried to state the relevant opinions of these authors correctly and also to distinguish them clearly from my own. I have refrained from repeating here the further references given in the sources that I am citing. The reader interested in pursuing a given topic further should consult the given references first.

The paper has the following structure: In Section 2 I relate my own experiences in attempting to apply the scientific method to core problems of economics and politics. Subsequent sections deal in a roughly chronological order with aspects of the history of ideas, with the emphasis always on the extent to which the ideas were shaped by either ideological or empirical considerations.

This question is closely related to how the human agents, intersecting in society and economy, were conceptualized. Accordingly, Section 3 discusses how the enlightenment conception of man narrowed to that of economic man. The principal source for this section is Fonseca (1991).

Section 4 reviews empirical traditions in economics. I conclude that these traditions were generally marginal and tended to relate to established theory with mutual hostility. Empirical and theoretical traditions were briefly joined first in the era of Petty, later in that of Keynes; subsequently the tradition of abstract theorizing always regained dominance. The joining of empirical and theoretical traditions on the basis of equality that was characteristic of the rise of natural science never took place in economics.

Section 5 is devoted to the Chicago School, particularly the concerted effort by Stigler and Friedman to move economics into conformance to the neoliberal ideology. Here the basic reference is Leeson (2000).

Section 6 discusses the concept of representative agent that has become central in much of contemporary economics, particularly macroeconomics and public economics. The theoretical literature on this concept clearly demonstrates that it is flawed in the sense that there are no reasonable assumptions regarding individual agents that allow the existence of a representative agent to be deduced. I conclude that the concept has no scientific validity and that its popularity is due to a confluence of several ideologies. The basic source for this section is Hartley (1977).

A recent working paper by Mankiw (2006), that provides a mainstream perspective on the role of science in macroeconomics, is discussed in Section 7.

Section 8 turns to the cold war origins of rational choice theory at the Rand Corporation. It was part of the effort to develop an ideology supporting capitalist democracy against the claims of socialism. Early applications of rational choice involved the use of game theory to formulate strategy against the Soviet Union and the formulation of the so-called planning-programming-budgeting-system (PPBS) approach to the design of governmental expenditure programs. Intellectually these applications of rational choice were complete failures; institutionally they were hugely successful, in particular, PPBS was first adopted by the Pentagon, then by the rest of the US government and spread from there to the international organizations, particularly the IMF and World Bank. This section is based on Amadae (2003).

Section 9 describes how rational choice moved from the military/government sector to academia, a development greatly facilitated by the Ford Foundation. I concentrate on the single most famous and most influential result in academic rational choice theory, usually referred to as the ‘Arrow paradox’, or ‘Arrow’s impossibility theorem’. This theorem is based on assumptions intended to refute socialism, instead, it refutes capitalist democracy; another Arrow paradox! I argue that the most fundamental assumption used by Arrow, that preferences must be expressed as orderings, is derived from a long chain of ideological battles

and has no foundation in reality. This section is based partly on Amadae (2003) and partly on Hillinger (2005).

This paper contains much evidence of ideology; is there any science to be found in social 'science'? In Section 2 I had described my own efforts at scientific work, particularly in relation to business cycles. Unfortunately, this work directly contradicted the dominant ideological tendencies in macroeconomics and therefore did not motivated independent investigators to test it. The key requirement of independent confirmation is thus still missing. In Section 10 and 11 I discuss two theories that I view as having been confirmed by strict scientific standards: the quantity theory of money and the determinants of happiness.

In Section 12 I discuss why it is that so little confirmed knowledge is generated in the social sciences and why even confirmed knowledge is widely disregarded. I attribute this to a misguided search for novelty and a misunderstanding of the nature of and the need for replication.

In the concluding Section 13 I argue that the problems of the social sciences reflect those of the larger society both with respect to culture and institutions.

2. SCIENCE IN ECONOMICS: A PERSONAL ACCOUNT

2.1. Motivation and Problem Identification

The dominant motivation of my career as an economist has been the application of the scientific method as it is understood in the natural sciences. I had begun my Ph. D. studies at the University of Chicago in 1959 and started to think about a dissertation topic in 1961. I became aware of two problems that from the point of view of the scientific method seemed to be crying for a solution.

At that time it was widely accepted that the macroeconomic fluctuations in the United States were to a large extent *inventory cycles*. It had been observed that inventory disinvestment, i.e., the drawing down of stocks, accounted for between 50 and 100 percent of the typical US recession. Furthermore, it was seen that the typical length of the cycle, including both a phase of negative and of positive inventory investment, was fairly regular at about 3.2 years. The phenomenon had attracted much attention including extensive hearings in the US Senate, chaired by Senator Douglas, a former University of Chicago economics professor. One of the most famous theoreticians in the economics department at Chicago at that time was Lloyd Metzler. He had produced a formal model, that I found very plausible, of how the decisions of producers generated the cycle. In spite of all this activity, I noticed that there was no systematic effort at determining the quantitative characteristics of the inventory cycle, using the best statistical methods available for the purpose. Furthermore, no effort had been made to test Metzler's model against the data. It seemed to me that the scientific method was not being applied.

I decided to make the quantification of inventory cycle stylized facts and the test of Metzler's model against these the subject of my dissertation. I had come to Chicago without any substantial background in mathematics and statistics and these subjects did not have a strong tradition in the Chicago economics department of that time. Outside of Chicago, the dominant style of doing econometrics in macroeconomics was to construct large-scale models involving hundreds of equations. The emphasis was on a good fit, not on the explanation of any independently observed stylized facts. I regarded this as a perversion of the scientific method. At Chicago an entirely different approach to macro-econometric modeling had evolved in the context of monetarism as it was being shaped by Milton Friedman. He believed in a monocausal influence from changes in the stock of money to changes in both nominal and real GDP. Accordingly, in empirical studies a single equation approach was used in which changes in GDP followed, with appropriate lags, from changes in the money stock. The

need for any structural modeling and with it the possibility of an endogenous dynamic of the macro economy was denied.

Thus, from the beginning, my purposes clashed with various commitments on the part of dominant groups within the economics profession. The matter might have rested there with my turning to some other less controversial project, if there had not been a particular development in the department. It was the establishment of a chair in econometrics to which R. L. Baseman was appointed. Baseman shared my interest in the philosophy of science and was equally critical of the econometric mainstream, particularly the practice of building large scale macroeconomic models. His support enabled me to carry out my intended project.¹

The second major unsolved problem for a scientific approach to economics of which I became aware as a graduate student was that of economic measurement. In the natural sciences, most impressively in physics, measurement is intimately tied to theory; if this were not the case the measurements could not be used to test the theories. The most important economic measures are price and quantity indexes; a better terminology is measures of inflation and of real expenditure. There have been three main approaches to theory construction in this area: One is the axiomatic theory of index numbers. Apart from the fact that there has been no agreement on which set of axioms should be adopted, there is a fundamental objection to this approach: it does not provide any economic inference from the measured values of the indexes that obey some set of axioms.² There is also the economic theory of index numbers. It applies to a single utility maximizing consumer with homothetic preferences. There is general agreement that consumer preferences are not homothetic. Moreover, price and quantity indexes that are of interest are computed with aggregate data, even for total GNP that includes business, government and foreign sectors. This theory too I found inapplicable. A third approach is the so-called econometric approach. It involves the estimation of the parameters of a system of aggregate consumer demand curves assumed to be generated by a representative consumer with homothetic preferences. Here the problem is that: **a.** A representative consumer does not exist (see Section 6) and if he did exist he would not have homothetic preferences. **b.** The data employed are not the prices and quantities of individual commodities and services, as consumer theory would require, but rather price and quantity indexes. The entire approach amounts to an invalid theory based on data generated by another invalid theory.

Another important area of measurement is consumer surplus. The formula $\frac{1}{2}(p_0 + p_1)(q_1 - q_0)$, originally obtained by Dupuit is widely employed in cost/benefit analysis. Many economists, among them such famous names as Hicks, Hotelling and Harberger have attempted to give rigorous derivations, but all of their derivations are flawed.³

At the time when I started to be concerned about these measurement problems, the interest of academic economists in them was fading. Several reasons may be adduced: One is that the unsatisfactory nature of the field, after many past efforts, was discouraging and no clear path to progress was apparent. In fact, the intermittent attempts that I made in this area throughout my career produced no real advances. These came only after my retirement when I decided to do more concentrated work in this area. Another reason for the profession's move away from measurement theory was that since statistical and other agencies of government were charged with the task of computing the measures, it was thought that they should also be responsible

¹ A prominent econometrician who has favoured small structural macroeconomic models is Arnold Zellner (2001).

² I once found precisely this criticism made in a book on the philosophy of science; along with the comment that this shows that economists do not understand the scientific method. Unfortunately, I have not been able to recover the reference.

³ Each of the three authors mentioned implicitly assume a constant marginal utility of income. Samuelson has shown that this assumption cannot be valid. The matter is discussed in detail in Hillinger (2001).

for the required theory. This assigned to the agencies a task they could not perform; no bureaucracy can do fundamental scientific research, particularly in a field abandoned by academic researchers.

My third major field of interest is voting theory. Although this interest began early, I turned to it in a concentrated fashion quite late, when I was nearly 10 years into retirement from active teaching. I no longer recall exactly how this interest developed, but I think that there were three different sources: The first was my interest in measurement. Most economic measurements have to do with determining in which of two situations people are better off, i.e. which provides higher levels of utility. Voting also has to do with the determination of which of two or more situation is best in the voters' judgment. In my early paper on voting (Hillinger, 1971), I showed that when parties bundle their stands on several issue, then the platform that combines the most popular stands on individual issues will not in general be the one most preferred by the voters.

Gradually I became aware of other problems connected with voting. The most common voting procedure used in general elections is plurality voting. Two major defects of this method are: **a.** A candidate strongly opposed by a majority of the voters may be elected. This will happen when there are several candidates acceptable to the majority who divide the majority vote among themselves with the consequence that the minority candidate is elected. **b.** In an otherwise fairly even election, a minority candidate may take votes away from the major party candidate that he is ideologically closest to and cause him to lose the election. In the 2000 US election the minority candidate was the consumer advocate Ralph Nader, who was ideologically closer to the Democrat Gore than to the Republican Bush. It is likely that by taking votes from Gore, he tipped the scales in favor of Bush. **c.** The plurality rule provides a strong incentive for strategic voting. If a voter feels that the candidate he favors has no chance of winning, he is likely to regard a vote for that candidate as wasted and will vote in stead for a candidate with a chance of winning. Plurality voting faces the voter with difficult strategic decisions and leads to a vote that does not reflect the true preferences of the voters.

Voting is the central institution of democracy. Given that plurality voting is so highly defective, it seemed to me that there was no more important problem in social science then to devise a better method.

A third source of my interest was the literature on collective choice and in particular Arrow's impossibility theorem that implied that a voting procedure satisfying reasonable conditions could not exist. I felt a very strong resistance to accepting the theorem at face value because I felt that intellectual consistency would then require abandonment of the idea of democracy as a real possibility. Engaging for democracy would then be as foolish as trying to construct a *perpetuum mobile*. I found this alternative emotionally unacceptable. Furthermore, I felt that democracies, imperfect though they were, actually existed and that the task was to improve, not to deny them.

If I was to be right, Arrow had to be wrong. There was clearly nothing wrong with his mathematics that had been subjected to a huge amount of scrutiny. Furthermore, his assumptions, stated as axioms, had also been intensively studied and their modification did not point to a way out of the dilemma. I found the basic idea for the solution in another area of social choice theory. John Harsanyi had shown that a plausible and consistent set of axioms for social choice could be constructed if one adopted a cardinal framework for the description of preferences. Instead of the weaker formalism of orderings, preferences were now represented by numbers obeying the rules of arithmetic, thereby allowing an expression of *intensity* of preference.

Harsanyi's axioms were abstract and his discussion moved on an equally abstract philosophical level. He did not discuss any application. To me the application to voting was clear. The voter must be given a numerical scale such that the numbers on that scale could be used to evaluate the alternatives. The voting scale could be utilized as a normalized utility

scale used to ‘measure’ the utility the voter attached to each alternative. As in any ordinary measurement process, there should be no restriction on how the voter distributes the values permitted by the scale over the alternatives; specifically, he must be free to assign the same value to alternatives that he values equally.

Having come this far, it also became clear to me that this is in fact how preferences are being universally aggregated everywhere except in political voting. Businesses measuring customer satisfaction, or pollsters measuring the popularity of policies or politicians, all proceed in this manner.

The startling conclusion is that the central problem of political theory, described as insoluble in a large theoretical literature, has in fact had a pragmatic solution that is widely practiced everywhere except where it would matter most, in political elections.

In the following I give brief accounts of the principal findings in the three principal fields in which I worked and also of the difficulties encountered in working and attempting to publish against the trend.

2.2. *Swimming Against the Stream*

Having obtained my Ph.D. from the University of Chicago I pursued an academic career in economics, first at US universities and since 1972 at the University of Munich. In the first years of my teaching career I pursued a variety of interests. In addition to the major fields of interest mentioned above, I also published in microeconomics and on the scientific method. All of these publications were isolated in the sense that they were not part of a recognized ongoing research program participated in by other members of the profession. This created obvious difficulties in terms of managing a career, but I simply could not motivate myself to participate in some field that I regarded either as pathological, or as being of lesser importance.

I continued to read widely in philosophy, the philosophy of science as well as in economics and other social sciences. I was also strongly interested in a loose movement that had evolved after the War and went under the two labels of ‘cybernetics’ and ‘general system theory’. Cybernetics was the creation of the mathematician Norbert Wiener who advanced his ideas with both technical and popular writings. General systems theory was founded by the biologist Ludwig von Bertalanffy in association with other natural and social scientists. The most general unifying idea was that complex, evolving systems, regardless of their material and energetic basis, could be modeled in terms of information flows. Cybernetics and general systems theory did not evolve into a kind of meta-science as the protagonists had hoped; however many ideas were imported into different disciplines.

Taken up by engineers, the ideas evolved into mathematical systems theory, equivalently referred to as the state space approach. Major contributions were made by Lotfi Zadeh (of fuzzy sets fame) and Rudolf Kalman (he of the filter). Zadeh and Desoer (1963) was a basic text that helped me understand dynamical systems and apply the theory to the modeling of business cycles. Specifically, I used the theory of modes and their excitation to explain why cycles were sometimes evident in the data and at other times not. Kalman has written about model identification in econometrics. I was not able to penetrate very deeply either his mathematics, or his concepts, but he seemed to express ideas similar to my own. Specifically, he criticized statistical testing in econometrics and argued that a theoretical model should be the simplest one that *exactly* reproduces some stylized facts.¹

Advancing and publishing my work on the inventory cycle proved difficult for several reasons. One is that powerful movements in macroeconomics, with regard to both substance and method moved in diametrically opposite directions to that in which I was moving. Keynesians argued that active stabilization policies made business cycles a phenomenon of

¹ I heard a talk by Kalman on this subject at a conference some years ago. Researching the matter for this paper, I was able to come up only with the abstract given in Kalman (1994):

the past and hence no longer of interest.¹ The monetarists reached a similar conclusion, though from a contrary understanding of macroeconomic reality. They thought that cycles were the result of erratic monetary policies and lacked any interesting own dynamic; they would disappear following the adoption of a steady monetary policy. Similarly, with respect to method, I ran afoul of both Keynesians and Monetarists, though for opposite reasons. Keynesians believed that a macroeconomic model had to consist of hundreds of equations to be considered ‘realistic’; the monetarist, at the other extreme, believed in single-equation monocausal explanations. The sort of small explanatory model that I constructed to explain the inventory cycle was ruled out by both sides.

Another major difficulty was the need to acquire the econometric tools that I felt I needed and that differed sharply from those in common use in macroeconomics. Such tools were needed both for the description of the inventory cycle and for the construction of explanatory models. This was a daunting task, particularly given that I did not have a strong background in either mathematics, or statistics.

The standard tool used in the natural sciences to describe processes that contain both random and periodic components is spectral analysis. Classical spectral analysis, the only kind available at that time, is a nonparametric and hence highly data intensive method. It has been estimated that the precise identification of a cycle requires an observation length that includes about 30 cycles. Not surprisingly, the method has yielded unimpressive results with the short available economic time series. An application to the identification of the inventory cycle that I made was also unimpressive.

The other econometric problem concerned the estimation of explanatory models. Here as elsewhere, the standard econometric practice departs from that used in the natural sciences. There, dynamic models, both in theory and in applications are generally formulated in continuous time. The reason is both that reality is itself continuous, at least at the macro level, and also that continuous models tend to be simpler and more elegant. In macroeconomics, the assumption that the data are generated by a discrete process is unrealistic. Economic agents do not all make a decision at the beginning of a period, be it the quarter or the year; decisions, adjustments and random disturbances affecting these occur more nearly continuously. There exists by now a large literature that demonstrates that the fact that observations are discrete does not obviate the necessity to model the continuous data generating process². An important aspect is the following: Suppose that the data are generated by the continuous process $D\mathbf{x}(t) = \mathbf{A}\mathbf{x}(t)$ and are modeled by $\mathbf{x}_t = \mathbf{B}\mathbf{x}_{t-1}$. Then $\mathbf{B} = \exp \mathbf{A}$ and theoretical restrictions, that apply to \mathbf{A} , particularly zero restrictions on the elements of \mathbf{A} , do not apply to \mathbf{B} . This argument alone shows that structural modeling in macroeconomics in discrete time does not make sense; this is particularly true of large-scale macroeconomic models with hundreds of zero restrictions.

The discussion to this point makes clear that what I had in mind called for a large scale research project, including people with strong backgrounds in econometrics and good computing skills. The fact that I saw no possibility of organizing such a project, combined with the fact that interest in business cycles had vanished within the economics profession led me to essentially abandon the project by 1972.

A strong new impetus to continue the research came in that year in connection with my move to the University of Munich. The city is also the home of the large IFO-Institute for empirical economic research where I gave a talk on my work on the US inventory cycle including the three-equation model of the cycle that I had constructed. A member of the

¹ Even if it had been true, this position was hardly scientific. The explanation of historical cycles would still have remained as an interesting task. Furthermore, the idea that one could neutralize the cyclical forces with active countercyclical policies, without a need to understand the forces themselves, was hardly convincing.

² A survey of the continuous-time approach to macroeconomic modeling is given in Wymer (1996).

institute, Klaus Schüler, decided to fit the model to German data. It turned out that the exact model that I had fitted to the US data gave even better results when fitted to the German data. This was a consequence of the fact that the cycle itself appeared stronger and less erratic in German than in US data.

A few words about the significance of this result are in order. The large-scale models then in fashion were not stable even from one quarterly forecast to the next within the same country. In this case the simplest model that could be devised to replicate the stylized facts was fitted without modification to data for another country, data that I had been unaware of when originally devising the model. I felt that this was a very strong confirmation that I was on the right path, at least from a scientific point of view and I was motivated to resume the research with renewed vigor.

I recommitted myself to a research program on business cycles that was wider both conceptually and empirically than the previous one. I had become aware of a long tradition of work on *investment cycles*, in which the inventory cycle was only one kind, others being the *equipment cycle*, and the *building cycle*. For post-WWII data, I found that genuine building cycles had become localized to metropolitan areas, or regions, and that at the national level a *fixed investment cycle* could be identified that was distinct from the inventory cycle. The other extension was to internationalize the data set so as to include the major industrialized economies.

I continued the strategy of looking to associates for the programming of the required econometric software and for carrying out the empirical work. Initially I had some funding for this purpose. Later I relied on both undergraduate and Ph.D. students from the departments of economics and statistics at the university. As described in the next section, much was accomplished in this manner. However, the process of getting the work done in this manner turned out to be enormously drawn out and often frustrating. I underestimated the difficulty of carrying through an ambitious scientific program with people who did not share my own long term commitments. Looking back, it is clear to me now that I should never have started a project that I could not finish on my own. This is one reason why the publication of the results has fallen far short of what I wanted. After my retirement, when I worked alone first on measurement and then on voting, I was able to progress much more quickly and to have publications that I was more satisfied with. It is one of the difficulties of swimming against the trend that one is left alone not only in regard to purely scientific issues, but also in regard to practical and strategic decisions.

Between the three principal research areas there were substantial differences both with regard to the formulation of research problems and the publication of the results.

Regarding the research on business cycles, I never had any major problem in formulating the research problems and all of the principal decisions that I made were ultimately validated by the empirical results. The reason is the basic ideas were already available; they had to be found and identified, rather than invented from scratch. The basic issue was one of deciding on relevance. Thus I committed early to the scientific method of the natural sciences as against the then current econometric practices. There was also a substantial, empirically oriented tradition of business cycle theory. The main findings of that tradition were summarized in a fine book by R. C. O. Matthews (1959), just before the tradition disappeared from economists' radar screen. My work here was basically one of refinement and formalization as well as the carrying out of the appropriate econometric program. Here three specific methods were essential, all coming from the natural sciences: maximum entropy spectral analysis to determine the business cycle stylized facts; continuous-time econometrics for estimating the explanatory models formulated as differential equations; finally, parametric resonance to explain the aggregation of cycles from the level of firms to the macro-economy.

Publishing the results proved to be very difficult, as far as major journals are concerned, virtually impossible. A number of publications appeared in applied, or more specialized

journals, or in conference proceedings. The most comprehensive publications were the conference proceedings of two conferences that I organized. I was fortunate in being able to secure Oxford UP and Cambridge UP as the publishers¹.

Both with regard to measurement and voting, both my research and publishing experience differed significantly from that encountered in macroeconomics. In all three fields I encountered massive ideological resistance. In macroeconomics I found virtually no understanding or encouragement on the part of the establishment. In the other two fields the attitude was similar on the part of the bulk of the profession; however, in each of these two fields I received a great deal of encouragement and help from the most prominent and senior scholar in the field. In the case of measurement this was Erwin Diewert of the University of British Columbia and in the case of voting theory it was Steven Brams of New York University. This help was of great importance to me particularly since in each of these fields I had to absorb a voluminous and mathematically sophisticated literature.

My best publishing experiences were in relation to measurement. The reason for this is that, on the one hand, the era in which many distinguished economists worked on measurement problems is still relatively recent. Many economists, including editors and referees, are therefore still aware of the fact that these are important problems with a tradition in economic theory. The other aspect is that the subject has migrated from academia to governmental statistical agencies, where the professional staff rarely publishes in academic journals. Hence, there is no cartel attempting to block academic publications they object to. As my ideas in this field progressed, I was able to publish without undue difficulty.

With regard to voting theory the situation is again different. Here I had one simple idea and the justification of this idea did not require any very complex mathematics. The main work involved dealing with the existing theory and showing that various proofs already present in the literature could be modified, or extended to support my proposal of utilitarian voting. Publishing the result in the standard journals dealing with social choice proved to be impossible. The referees typically rejected the paper out of hand, evidently without bothering to read it. I was fortunate in finding sympathetic editors at *Homo Oeconomicus* who guided the paper through a difficult refereeing process and ultimately published it. They told me that some referee comments, received from distinguished scholars in the field, had been so venomous that they were not forwarded to me.

2.3. Results

Following are brief summaries of the principal results in the three major research areas:

Regarding the research on business cycles, a relatively complete summary is Hillinger (2005a). The principal empirical findings, based on several studies of the major industrialized economies are:

- a.** Inventory investment is the most volatile component of GDP, fixed investment the second most volatile. The contributions of these components to the fluctuations of aggregate GDP is substantially greater than their average shares.
- b.** Each type of investment has a periodic as well as a random component, with the random component being more prominent in inventory investment. Roughly, the period length is about 4 years for inventory investment and about 8 years for fixed investment.
- c.** The cycles can be explained by partial adjustment processes that result in overshooting, the excessive accumulation or decumulation of stocks, and ultimately in directional reversals. I have formalized these ideas as *second order accelerator equations*.
- d.** The aggregation of cycles from the level of individual firms to the aggregate level can be explained by means of the theory of *parametric resonance* taken from physics. This theory explains what happens when several mechanism capable of similar oscillations are brought

¹ Hillinger (1992), Barnett et al. (1996).

into contact with each other. If the intrinsic periods of the mechanisms and the initial phases of their fluctuations are sufficiently close to each other, then the cycles will ‘agree’ on an average period and an average phase and the amplitudes of the individual oscillations will increase. The cycles of the firms are coordinated and accentuated in this manner and become visible at the aggregate level.

These empirical findings and explanatory theory are a confirmation of the theory of *investment cycles* that was dominant in the first half of the Twentieth Century. It sharply contradicts contemporary theories that regard the economy as being passively pushed around by unexplained ‘shocks’.

As explained above, in the cases of measurement and of voting the research problem was rather different; instead of identifying patterns in data, the problem here was to find suitable conceptualizations. What is a good conceptualization? There is no mechanical answer, but I think that generally a good conceptualization is one that helps to answer some interesting questions.

The final results of my research on the measurement of inflation and real expenditure is contained in Hillinger (2003a, 2003b). Some of the key argumentation in these papers is rather technical and has to be omitted in this brief summary. Inflation and real expenditure measures are usually employed in contexts that have to do with people’s wellbeing. For example: An increase in the consumer price index signals that people will be worse off at constant nominal incomes, and possibly that they are entitled to some compensation in the form of higher wages or pensions. A rise in real GDP is taken to imply that the society is better off because of increasing current and expected future consumption. If the estimated aggregate consumer surplus associated with a project is greater than the cost, it is inferred that society will benefit from a realization of the project. I asked myself if there is a unified concept that underlies all such inferences.

A hint is provided by the fact that when prices are constant, all of the measures of the change in real expenditure that are in use reduce to the change in money expenditure. When prices are constant, money income or expenditure is how we commonly compare individuals or groups with respect to how well off they are. This suggests that the problem when both prices and quantities change is to decompose the total change into one part that is equivalent to an expenditure change at constant prices and another due to inflation. Conventional measures of the change in real expenditure are computed by multiplying the quantity changes by fixed prices. The vector of fixed prices has been taken as that of the initial period, the final period, or some combination of the two. Depending on this choice, and the functional form employed, many measures have been constructed.

As is often the case in science, a more elegant solution is obtained taking limits and substituting a continuous analysis for the discrete case. For the present problem, such a path had been taken by Divisia and Törnqvist but without taking it to a satisfactory conclusion. I was able to rigorously validate their conclusion and also to show the analytical power of the Törnqvist index in a number of applications. The derivation of the Törnqvist index is too complex to be summarized here; the interested reader can find the details in Hillinger (2003a).

The traditional theory of price and quantity indexes focuses on comparisons between two periods. This has to be extended to the multi-period case. This is done by means of chain indexes. Let $P_{s,t}$ be a price index giving the inflation, i.e. the proportional increase in the price level from period s to period t . Then the chained index over periods $1, 2, \dots, T$ is defined by

$$P_T = 1P_{1,2}P_{2,3}\dots P_{T-1,T}$$

The other extension involves the computation of real expenditures for some aggregate as well as for components of that aggregate. The most important example is real GDP and its components such as real consumption expenditures. In Hillinger (2003b) I argued that the statistical agencies do this incorrectly. The agencies assume that they must compute real expenditures independently for each component as well as for the total. This has the

consequence that the components no longer add up to the total. The agencies used to distribute the discrepancy over the components to restore additivity. This fact was not publicized and generally unknown to the users of the data. From a scientific point of view this was a farce. It had the consequence that the many identities built into macroeconomic models had no basis in reality. Fortunately, the agencies have seen the error of their ways and now report the discrepancies rather than making the adjustments. This is still not an acceptable solution because: **a.** Identities that are essential in building macroeconomic models are not maintained. **b.** The discrepancy tends to grow with time so that meaningful time series of arbitrary length cannot be constructed in this manner. I argue that the only and simple solution to the problem is to deflate GDP and all of its components by the same deflator. I also argued that the deflator should be a chained Törnqvist price index for aggregate consumption.

With respect to voting theory, the matter is much simpler and little needs to be added to what was said above and will also be discussed in Section 8. As already mentioned, I conceptualized the problem of voting as being that of allowing voters to express their judgments on a numerical scale, with aggregation proceeding by addition. For general elections I proposed a simple three-valued scale that can be interpreted as being for, or against, or neutral in relation to some candidate, or issue.

3. FROM THE ENLIGHTENMENT CONCEPTION OF MAN TO ECONOMIC MAN

I had already started on this paper when I chanced across a forgotten, but now highly relevant book in my library; da Fonseca's (1991) *Beliefs in Action: Economic Philosophy and Social Change*. The book is a history of ideas about ideas; their generation, transmission, distortion and finally their social impact. In spite of the obvious importance of the subject and the fact that it has been of concern to philosophers and economists from antiquity to the present, I am aware of no other systematic and historical treatment. My aim here is not to review the book, but rather to extract those findings that are relevant to the present paper. I will also indicate where my interpretation or emphasis differs.

The principal insights that emerge from da Fonseca's book and that he richly documents with quotations from the original works are the following:

a. Before the advent of the neoclassical concept of 'economic man' (EM)¹, philosophers from antiquity through the Enlightenment, most importantly David Hume, Adam Smith and John Stuart Mill, had a different conception of human nature. This conception is most easily explained by contrasting it with the by now more familiar EM concept. Under the EM concept individual preferences are taken as primitive givens, not subject to further scrutiny. The distinction between purely selfish motives and ethical motives for behavior disappears. For the philosophers this distinction was the principal concern. After all, Smith held a chair of moral philosophy. While it is true that adherents to the EM conception have allowed altruistic motives to be part of the individual's preference system, this concession is but a pale ghost of moral philosophy. Specifically, the question of the kind of altruistic elements that *should* be a part of individual preference is not considered. Altruism, along with all other decisions of the individual are regarded as purely personal issues, not subject to any rational debate.

b. Under the EM conception the individual is regarded as the best judge of his own welfare. The philosophers questioned this. They regarded the opinions of the bulk of mankind as being feeble and unstable structures, largely determined by non-rational or even irrational factors. Though neither the philosophers nor da Fonseca used the term, I would refer to many of these opinions as *ideologies*. To give a currently relevant example: An element of the neoliberal ideology that is transported by advertising, the popular media, and the pronouncements of political leaders is that the ever increasing consumption of material goods

¹ Often referred to as *homo oeconomicus*.

is the key to happiness. Modern happiness research sides with the philosophers in refuting this belief.

c. Another major issue that has concerned philosophers and later economists from antiquity to the present is the question of how dominant ideas in a society change and the effect that ideas have on society. Most of the writers considered by da Fonseca both desired and expected their writings to have a positive effect. Accordingly, the prevailing view was that moral philosophers can improve the moral quality of the population, economists the economic understanding and that all of this would contribute to the general improvement of society. This belief system is related to the idealistic tradition in philosophy. Not surprisingly, men of ideas emphasize the autonomy and importance of ideas. This can be contrasted with the views tending in the opposite direction, of men of affairs. In each case an element of ideology is involved, that aggrandizes the own role.

The most prominent challenge to idealism in social thought is the doctrine of historical determinism due to Hegel and Marx. According to Marx, the material conditions of society determine the ideas that people hold and the dynamics of social change. The ideological aspect of their theories is rather obvious. Hegel, a philosophy professor at the University of Berlin, claimed that history had found its ultimate perfection in the Prussian state. Marx attempted to motivate the proletarian revolution by claiming that the desired outcome was in any case inevitable. Neither Hegel nor Marx was completely consistent, since each was expecting to have an influence with his writings. Marx in fact expected *Das Kapital* to become the “bible of the proletariat” and an important instrument in organizing the revolution.

The relation between ideas and social reality will be a continuing theme of this paper. The question of the priority of one over the other is in my view akin to the question of the priority of the chicken or the egg. A more useful general framework is that of the three worlds of Sir Karl Popper: The first world of material objects; the second world of subjective ideas; the third world of objective interpersonal knowledge. In Popper’s view, that I share, the three worlds interact in a dynamic process; there is no priority of one over the others.¹

The first chapter of da Fonseca’s book, as sketched above, deals in a general way with the role of ideas in relation to society. The remainder of the book narrows the discussion to a specific topic, albeit a very important one, namely the transformation of the concept of man held by Enlightenment philosophers and classical economists, to *homo oeconomicus*. This reduction is itself part of wider intellectual movements that began with ancient Greek philosophy and continued in relation to the rising natural sciences. This is the subject of da Fonseca’s second chapter. The concepts that I would stress in discussing the currents of thought that are involved are: *reductionism, materialism, rationalism and scientism*.² The Ionian philosophers taught that all phenomena resulted from the combinations of the four elements fire, air, water and earth. More radical and closer to modern science was Democritus who taught that all phenomena result from the motions of atoms. He regarded mental phenomena as caused by the motions of particularly fine atoms. Democritus was thus an early representative of materialism, the doctrine that stands in opposition to idealism, which holds that the origin of all phenomena is mental. Both reductionism and materialism have motivated and continue to inspire modern science as well as fields that have merely laid claim to being scientific.

With the rise of science in the Eighteenth Century these philosophical traditions revived, but in a somewhat changed form. Since physics now recognized forces in addition to matter as fundamental entities, the doctrine of materialism was replaced by that of physicalism, according to which physical phenomena are behind all appearances. Rationalism in its pure

¹ See Chapter 38 in Popper (1976).

² Only the second of these terms appears in da Fonseca’s index. All three appear in *The Oxford Dictionary of Philosophy* (Blackburn, 1994) with cross-references to related terms.

form persisted in philosophy, but there also developed the related doctrine of scientism according to which the methods of the natural sciences, above all those of physics, were appropriate in all areas of inquiry.¹ Particularly important as a forerunner of the later economic man was the French physician and philosopher de la Mettrie who in 1747 published *L'Homme Machine*. Da Fonseca, starting with a quotation from de la Mettrie describes his philosophy as follows:

'Man is a machine, and there is nothing in the entire universe but a single substance diversely modified'. The basic idea is to see human beings and their actions as objects and events in the physical world, that is there to be described and accounted for, like all the rest, in purely objective terms; in terms in which moral thinking and mental processes in general – the agent's subjective experiences – have no place. (p. 26).

The rise of the EM concept is described in da Fonseca's Chapter 3. This development was both gradual and controversial. da Fonseca points out that the Eighteenth Century founders of economics, Quesnay and Smith, both rejected the mechanical model of human behavior, arguing instead for the reality of human freedom and the importance of moral choices. A tendency to limit the discussion to motives that we would today call economic was first evident in the work of Bentham and was subsequently elaborated by Ricardo. Ricardo's work also involved a substantial increase in the degree of formalism, an important aspect of scientism. da Fonseca points out that this reductionism to purely economic motives was controversial. Thus, J. S. Mill, in his mature work distanced himself from the reductionism of both Bentham and Ricardo and refused to draw a sharp line between economic and non-economic motives. The most prominent economist of the early Twentieth Century, George Marshall, still expressed the (failed) hope that economists would pay increasing attention to non-economic motives.

The conquest of economics by the EM concept began in the early Nineteenth Century with the advent of Marginalism on the continent and with the work of Jevons and a little later of Edgeworth in England. The basic conceptualization in terms of the decision problem facing a utility maximizing agent was the same in both cases, but the English school is more interesting in the present context because of their emphasis on the use of mathematics, specifically the calculus, for the analysis of the problem. With the work of these economists, economics assumed a semblance of physics. This was fully intended. Edgeworth (quoted by da Fonseca p. 45) wrote:

'Mecanique Sociale' may one day take her place along with 'Mecanique Celeste', throned each upon the double-sided height of one maximum principle, the supreme pinnacle of moral as of physical science. As the movements of each particle, constrained or loose, in a material cosmos are continually subordinated to one maximum sum-total of accumulated energy, so the movements of each soul, whether selfishly isolated or linked sympathetically, may continually be realizing the maximum energy of pleasure, the Divine love of the universe.

At the end of the chapter da Fonseca reaches the following conclusion:

So, the mechanization of the economic picture advanced by Jevons, Edgeworth and some of their contemporaries can be seen as combining two divergent tendencies:

1. There is the simplification of economic action so as to make it independent of moral ends. The central feature of the metamorphosis of economic agents into 'pleasure-machines' is that they cease being moral persons and become players, each one computing his way – always within the legal framework – towards the means that shall enable him to fulfill his desires and commitments once he steps out of the purely economic sphere of action.

2 There is the metamorphosis of the economic agents into 'pleasure-machines' as constituting in itself an end. Economic actions, the argument runs, ought to be purely instrumental rather than ends in themselves. And it is a good thing, given the nature and properties of a pure market economy, that agents should suspend their moral beliefs and opinions as they try to amass at lowest cost the maximum of the means necessary for the satisfaction of their highest preferences. The disengagement of the economic life from morality turns out to be not only a promising starting-point for abstract analysis, it becomes a moral end-point too, that is, a desirable state of affairs.

¹ Callahan (2005) gives an interesting account of the rise and continuing impact of scientism.

This section has so far been based on the first three chapters of da Fonseca's book. The remaining chapters are not as directly relevant to my purposes and I omit a consideration of them. At this point I give my own interpretation of what has been discussed, an interpretation that I hope to deepen in the following sections. My interpretation differs significantly both from that of da Fonseca as well as of most other critics of the economic mainstream.

I do completely agree with da Fonseca that the concerns of the enlightenment philosophers with moral judgments as well as with the general uncertainty and limited rationality of beliefs is essential to any broad understanding of social issues. Where I disagree is in evaluating the analytical separation of economic and non-economic motives and their consequences. I believe that the separation was natural and essential for the development of economics. Smith himself largely separated the analysis, treating the one part in his *Theory of Moral Sentiments* and the other in *Wealth of Nations*. Let us look at Smith's central analytical argument concerning the 'invisible hand'. He argued that under conditions that we would today refer to as 'perfect competition' those goods that are most desired by consumers would be produced. If this were not the case, then some producers could bid resources away from competitors producing the less desired goods and produce the more desired ones. This argument is purely economic and analyses the implications of given tastes and technologies. Adding moral issues or uncertainty complicates the analysis without adding to our understanding. Later in this paper I will argue that as contemporary economists are turning in large numbers to the themes neglected in neoclassical economics they are also abandoning the solution of long standing problems of economics.

Subsequent to Adam Smith the subject matter of the *Wealth of Nations* was steadily elaborated and evolved into the respected academic discipline of economics. Moral philosophy, the subject of *The Theory of Moral Sentiments* largely stagnated. Some aspects of the social thought of the enlightenment philosophers were elaborated in sociology, as well as in Marxism and other non-orthodox strands of economics. But this did not lead to a complex and codified structure of thought as in orthodox economics. That economics was able to impress more with its sophistication was one reason for the difference in social acceptance and prestige. Another reason is that economics could be more easily aligned with conservative ideologies, sociology with revolutionary ones. Where revolutions seemed possible or actually took place, sociological thought dominated; with the world-wide collapse of revolutionary regimes and their ideologies economics began to rule supreme. An ideological fixation on isolated aspects of reality at the expense of other equally important aspects has prevented the emergence of a comprehensive and balanced system of social thought.

4. THE EMPIRICAL TRADITION IN ECONOMICS

4.1. The Empirical Tradition at the Birth of Economics

Ideology and empiricism may be thought of as opposites; empiricism is the earnest study of reality to find out what it is actually like; ideology is the attempt to make believe that reality is as we would like it to be. Of course, this is a bold, but I believe illuminating assertion. It provides the motive of the present section. I argue that the source of the strength of ideology in social thought is the weakness of the empiricist tradition. More specifically I will argue that the train of empiricism was derailed at the very beginning of modern economics, the dominant social discipline.

I begin the discussion by looking at the key ideas contributed by three founding fathers of modern economics and their subsequent fate. The founding fathers are Sir William Petty, François Quesnay and Adam Smith. The extensive writings of the three authors on what may be termed social philosophy are not the subject here. Instead, I focus on the one idea that each author is primarily identified with and that has retained its relevance to the present day.

Petty was a typical universal genius of the Enlightenment era. Rising from a humble background he became at a young age a doctor in physics, a professor of anatomy and a professor of ‘music’, a term designating at that time a wide field of cultural studies. Drawn to practical pursuits, Petty soon terminated his academic career and enlisted as a physician with Cromwell’s army that invaded Ireland. He was given the task of surveying the land and assessing the riches some of which were to be distributed among the victors. As a consequence of these endeavors he published in 1672 *The Political Anatomy of Ireland*. In this and in subsequent publications he estimated population, national income and national wealth for Ireland and England. He is thus the father of quantitative economics, or to put it differently, of measurement in economics. The title of his last, posthumously published book *Discourse on Political Arithmetic* contained the title he gave to this new subject of study.

Petty, a founding member of the *Royal Society* was personally involved in the scientific revolution occurring in his day. He believed in the methodological unity of the natural and the social sciences and took them to rest on a foundation of measurement. In the introduction to the *Discourse* he expressed his famous credo:

The Method I take to do this, is not yet very usual; for instead of using only comparative and superlative Words, and intellectual Arguments, I have taken the course (as a Specimen of the Political Arithmetick I have long aimed at) to express my self in Terms of Number, Weight, or Measure; to use only Arguments of Sense, and to consider only such Causes, as have visible Foundations in Nature; leaving those that depend upon the mutable Minds, Opinions, Appetites, and Passions of particular Men, to the Consideration of others.

Petty’s work inspired some further work in political arithmetic particularly by Charles Davenant who supplied the essentially modern definitions of national income and product.

Quesnay was like Petty a physician, in fact the court physician to Louis XV. Under the label of physiocracy he established a school of social and economic thought. Much of physiocratic thought is antiquated, particularly the idea that all wealth originates in agriculture. The idea that Quesnay is famous for and that has retained its relevance is that of a circular flow of goods and services through the sectors of an economy that could be measured in terms of the associated money flows in the opposite direction; an idea that suggested itself to the physician in analogy to the circulation of the blood. He gave a numerical example of such a system in his *Tableau Economique* (1758), a forerunner of the modern national income and product accounts.

With the work of Petty and Quesnay the basic ideas on which quantitative macroeconomics is based were in place. Thereafter the subject languished until the early Twentieth Century when the rise of socialism and of the econometric movement generated a strong interest in social and economic statistics. The modern equivalents of Petty’s and Quesnay’s measures, the national income and product statistics are generally regarded as the most important of all economic statistics and enter the national economic and political debates on an almost daily basis.

Adam Smith, like the other two authors, fully participated in the spirit of the enlightenment and of the beginning scientific and industrial revolution. The structure of his two principal works was in fact inspired by Newton’s theory of universal gravitation. Just as Newton had postulated a single force to explain, among other phenomena, the motions of the heavenly bodies, so Smith postulated in each of his works a single force to explain an aspect of human behavior. In the 1759 *Theory of Moral Sentiments* this force was sympathy; derived from an empathic concern for the feelings of others. In the 1776 *An Inquiry into the Nature and Causes of the Wealth of Nations* he postulated self interest as the driving economic force.

Smith differs from Petty and Quesnay in having never been active in a natural science. Instead, his principal occupation was as a professor of moral philosophy. Nor did Smith share the interest in measurement and quantitative analysis of the other two. Famously he declared “I have no great faith in political arithmetic”. Undoubtedly he was expressing a valid

skepticism relative to the very rough and uncertain estimates of the political arithmeticians; mere skepticism however does not help a field to advance.

The academic economic mainstream as it evolved subsequent to Smith remained essentially in the mold of philosophy, reasoning from intuitively perceived first principles. This was true of classical economics, of the marginalists, of the French mathematical school and, as I shall argue, also of contemporary economics. It is instructive to contrast the beginning of economics with the beginning of the natural sciences. The universities, of the early Renaissance, dominated by their theological faculties, were hostile to the rise of science and particularly to the experimentalists who generally came from the class of artisans of lower social standing. The early organization of science took place in scientific organizations outside the universities. It was only when the prestige of science began to replace that of religion that scientists established themselves at the universities, with experimentalists and theorists enjoying equal prestige. The marriage of empiricism with speculative theory, so characteristic of natural science, never occurred in economics or later social sciences.

4.2. The Empirical Tradition from Smith to the Early Twentieth Century

The evolution of economics after Smith illustrates the clash of various ideologies with empiricism. Broadly speaking, academic economics concerned itself with the elaboration of the properties of a competitive equilibrium, at first primarily verbally and conceptually, later increasingly in mathematical terms. Since the principal result is the optimality of the competitive equilibrium, there developed a natural affinity between economics and a conservative laissez faire political ideology. The analysis of what happens outside of equilibrium was both difficult and would have stood in the way of this conservative alignment of the profession. Ideologies tend to extreme positions; in this case Jean-Baptiste Say's law of markets, which ruled out on theoretical grounds the very possibility of a general overproduction.

Irrespective of Say's law, periods of economic depression and evidently lacking aggregate demand were a fairly common experience of the rising industrial nations. They were labeled at first as 'crises' and later, when a certain regularity of the occurrence was noted, as 'cycles'. The question of the possibility of a general glut due to insufficient demand was the subject of a long debate between Ricardo and Malthus. Ricardo, the abstract theoretician represented the economic mainstream, upholding the validity of Say's law. The empiricist Malthus took the modern position that income could be hoarded with the consequence that demand would be insufficient to absorb capacity output. On this and other issues the two could in the end only agree to disagree, but Ricardo's views remained the standard of academic economics, while those of Malthus, as observed by Keynes, persisted only in an economic "underground".

It is relevant in the present context that Malthus was the founder of demography as an essentially empirical discipline. Demography evolved subsequently outside of economics, to the great loss of the latter.

A great divide developed between the academic mainstream and those who wrote about economic fluctuations, referred to by the earlier writers as 'crises' and later as the 'trade cycle', or business cycle. Prior to Keynes, writers on this subject were, almost without exception, not academic economists. They were ignored not only by their academic contemporaries, but also by later and contemporary writers on the history of economic thought. Of the half dozen books on the subject in my library, none has an explicit treatment of the subject. Surprisingly, this is true even of Schumpeter's history; even though he had written a book on the business cycle. The treatment of the subject in Spiegel (1971) is typical. He has a short section on Schumpeter that contains the following passage:

Bunches of innovation, reinforced by imitators and speculators, would make for cyclical movements, with the economy pulsating to the threefold rhythm of the three-year Kitchins, nine-year Juglars, and fifty-five-year Kondratieffs, so named by Schumpeter after their discoverers.

The first two authors are not mentioned elsewhere in Spiegel's book. Kondratieff is briefly mentioned elsewhere under the topics 'Soviet Economics' and 'Econometrics'. The book contains no section explicitly devoted to business cycles.

From entries in *The New Palgrave* it can be learned that Kitchin was a journalist and businessman. Juglar was by training a physician who similarly to Quesnay drew an analogy to human physiology, discovering in the business cycle the 'heartbeat' of the economy. Kondratieff, finally, was an economist and statistician in the Marxist tradition.

The most extensive discussion of business cycle theories of which I am aware is Gottfried Haberler's *Prosperity and Depression*. Originally commissioned by the League of Nations, the first edition appeared in 1937. Haberler, himself a prominent academic, focuses on contribution to theory, mainly empirical work such as that of Kitchin or Juglar is not mentioned. Also, he focuses on contributions that were recent at the time of his writing; a time with more academic contributions than earlier periods, since the Great Depression could not be ignored even by the academic mainstream. Given this *caveat*, I will use the book to convey an idea of the main contributors and their ideas. A valid remark that Haberler makes prior to introducing his classification of theories is that they are not mutually exclusive; rather each emphasizes a particular aspect without necessarily denying the others.

Under the heading of *purely monetary theories of the cycle* only one author is prominently mentioned: R. G. Hawtrey, who for most of his career was an official at the UK Treasury. In this theory the cycle is caused by fluctuation in the supply of money that are in turn caused by fluctuations in the willingness of banks to extend credit.

A broad range of theories are discussed under the heading of *over-investment theories*:

The central theme of all these theories is the over-development of industries which produce producers' goods or capital goods in relation to industries producing consumers' goods. They all start from the universally admitted fact that the capital-goods industries are much more severely affected by the business cycle than industries which produce for current consumption. During the upward phase of the cycle the output of producers' goods rises much more, and during the downward phase is much more curtailed, than the output of perishable consumers' goods. Durable consumers' goods, such as houses and automobiles, are in a special position approximating to that of capital goods.

Two sub-groups of theories are distinguished. The first group is that of *monetary over-investment theories*. The authors discussed under this heading are: F. A. Hayek, F. Machlup, L. Mises, L. Robbins, W. Röpke and R. Strigl. Under the heading of *non-monetary over-investment theories* the principal authors are A. Spiethoff and G. Cassel whose line of thought can be traced back to Tugan-Baranowski and Marx. Prominent in these lists are authors from the German speaking area, not associated with the classical to marginalist mainstream.

The following chapter in Haberler's book is titled: *Changes in cost, horizontal maladjustments and over-indebtedness as causes of crises and depressions*. I limit myself to listing some of the prominent authors listed under this heading: W. C. Mitchell, A. C. Pigou, F. W. Taussig, Sir W. Beveridge, I. Fisher and others.

Under-consumption theories are the subject of Ch. 5. I quote from the first two paragraphs:

The under-consumption theories have a long history. In fact, they are almost as old as the science of economics itself. Lord Lauderdale, Malthus, and Sismondi are prominent among the early adherents of this school of thought. The authors who have done most in recent times to re-state and propagate the under-consumption theory in a scientific way are Mr. J. A. Hobson in England, Messrs. W. T. Foster and W. Catchings in the United States, and Professor Emil Lederer in Germany. The cruder versions of the theory, which exist in innumerable varieties in all countries, will not be considered here, as their fallacy has been clearly demonstrated on various occasions.

It is difficult to summarize these theories because, with some notable exceptions, their scientific standard is lower than the standard of those reviewed earlier in this volume. They cannot be reviewed as systematically as the over-investment and monetary explanations, for it is only in regard to certain phases of the cycle that these theories have anything original to contribute. The under-consumption theory is a theory of the crisis and depression rather than a theory of the cycle.

The discussion thus far has covered the principal theories and authors contributing to what may be called *traditional business cycle theories*. It was a largely empirical tradition that

existed apart from the economic mainstream and observed a phenomenon that the mainstream asserted could not exist. The situation changed as a result of the Great Depression that even the mainstream could not ignore. But, the perception changed with the rise of Keynesianism. Keynes thought that economic conditions had altered fundamentally so that instead of traditional business cycles there would now be a permanently depressed situation due to insufficient demand. Ironically, he was inspired by the under-consumptionists whom Haberler above characterized as more primitive than other business cycle theorists.

Keynes was clearly wrong in assuming that permanent depression instead of business cycles would henceforth characterize the capitalist economies. His prestige largely contributed to the disappearance of the traditional business cycle theory. It is an example of the pathological nature of the transmission, or non-transmission, of ideas in economics.

4.3. *The Early Econometric Movement*

Early econometrics, even before that term came into use with the founding of the Econometric Society in 1930, had the analysis of business cycles as its principal object of study. The early studies were essentially time series analyses with the purpose of determining periodicities.¹ These studies were not successful for several reasons. **a.** The periodogram analysis originally employed is not a good method and was later abandoned in favor of spectral analysis which involves a smoothing of the periodogram. **b.** The statistical material available was very limited, particularly in relation to investment, which is the key variable in the business cycle theories discussed above. **c.** Uninterrupted economic time series tend to be short, whereas the traditional methods of time series analysis require lengthy series.²

I turn to the econometric efforts at macroeconomic modeling, specifically with the purpose of explaining business cycles. In this connection I need to discuss a methodological issue. In the natural sciences, when it is said that the predictions of a theory have been confirmed, what is meant is that a characteristic pattern implied by the theory has been observed. In the natural sciences such patterns are referred to as *empirical regularities* in economics as *stylized facts*. In econometrics, prediction is taken to mean predicting the future values of some variables, which is entirely different. A further difference between natural science methodology and that of econometrics is the emphasis in the latter on statistical testing which plays hardly any role in natural science.³ To illustrate: One of the most famous episodes in the history of science is Newton's deduction, from the postulate of universal gravitation, of Kepler's laws of planetary motion, in particular, that the planets follow elliptical orbits. The determination of Kepler's laws required complex three dimensional geometry, but no statistical testing. That the orbits were nearly elliptical was, given the proper calculations, obvious so that a statistical test was superfluous. A statistical test though would have rejected the hypothesis since the orbits are not perfectly elliptical.

An economic example can be given from the business cycle stylized facts. Short-run macroeconomic fluctuations are to a very large extent fluctuations of inventory investment or disinvestment. Many studies made at my institute and elsewhere, using a wide variety of different methods have found that the typical short-run fluctuation in aggregate output is accounted for to an extent of between 50 and 100 percent by fluctuations in inventory investment. Given that *average* inventory investment is only 1 or 2 percent of GDP, this is a startling and robust result. This is the type of stylized fact that any business cycle model

¹ Excerpts from some of these are reprinted in Part II: *Early Time-Series Analysis*, of Hendry and Morgan (1995).

² Maximum entropy spectral analysis is a modern method that can be employed with much shorter series. For a discussion of the application of this method to business cycle analysis see Hillinger (2005).

³ I do not mean to imply that the many applications of statistics are in some sense unscientific. Instead, I mean that the typical application of statistics does not involve the testing of scientific laws. This point has also been made by Heckman (2001).

should be expected to replicate. Again, this stylized fact is so massive and robust that statistical testing is superfluous. Such a test would also be difficult since the share of inventory investment in fluctuations, while large, fluctuates substantially itself.¹

The methodological position that I have just described will now be used to evaluate the evolution of econometrics and of Keynesian economics. The question is: To what extent did these developments advance our empirical knowledge.

'Cobweb' models of cycles in the prices and quantities of agricultural commodities began to appear around 1930.² Inspired by these Tinbergen (1959 [1931]) made the first application to an industrial cycle, namely the 'shipbuilding cycle' involving freight rates and shipping capacity for the British, American and German merchant marines. The paper begins with a figure showing graphs of these variables as deviations from trend. A cycle is evident, especially clearly in freight rates. This is the stylized fact that the model is intended to explain. The assumptions incorporated into the model are plausible in terms of our general understanding of how market function as well as of characteristics of shipbuilding that can be verified by talking to the people involved. The basic assumptions are:

- a. When shipping capacity is low/high, freight rates are high/low.
- b. When freight rates are high/low, the rate of orders for new ships will be high/low and so will be the increase in tonnage or ship launchings θ years later, θ being the gestation lag. The fundamental equation obtained by Tinbergen is

$$f'(t) = \alpha f(t - \theta),$$

where $f(t - \theta)$ is the tonnage θ years ago and $f'(t)$ is current investment (increase in tonnage). He specifies the gestation lag for building a ship as two years and shows that, depending on the adjustment speed, a variety of cyclical and non-cyclical solutions are possible. Specifically, he obtains an 8 year cycle that he takes to be typical.

Both Frisch (1933) and Kalecki (1933) took the decisive next step of constructing business cycle models meant to be applicable to a national economy rather than an isolated market. The key variable of their models is the volume of fixed business investment.

The basic assumptions entering Frisch's model are as follows:

- a. Capital is required in fixed proportion for the production of consumer and producer goods.
- b. Orders for new capital goods have two components, a replacement component assumed proportional to total production and an acceleration component proportional to the rate of change of the production of consumption goods.
- c. Following an order, investment takes place at a constant rate over an interval ε , the gestation lag, until the completion of the capital good.
- d. Consumption is a constant, modified by a relationship between the fixed money supply and the transaction demand for money.

Frisch demonstrates that for plausible parameter values his model produces two cycles, regarding which he writes:

The primary cycle of 8.57 years corresponds nearly exactly to the well-known long business cycle...Furthermore; the secondary cycle obtained is 3.50 years, which corresponds nearly exactly to the short business cycle.

The cyclical properties of the model are shown to depend sensitively on only one model parameter, the gestation lag ε .

¹ The business cycle research conducted at my institute did produce a statistical test of business cycle stylized facts reported in Reiter and Woitek (1999). Their conclusion:

...the data support the predictions of the classical writers: fixed investment tends to have more spectral mass in the frequency range of 7-10 years, inventory behavior in the range of 3-5 years. Looking at each country separately, these regularities are often not significant in the statistical sense, which is not too surprising given the shortness of the available time series. The significance comes mainly from the similarity of results across countries.

² For a discussion see Pashigian (1987). The most prominent of these, the hog cycle is apparently still alive and well; see Stearns and Petry (1996).

In part V of his paper, entitled 'Erratic shocks as a source of energy in maintaining oscillations', he gives the first precise definition of the type of irregular cyclical movements which I refer to as 'quasi-cycles'. He gives the example of a damped (frictional) pendulum which can be modeled by differential equations with complex roots, giving the period and damping of the oscillations. If the pendulum is subject to erratic shocks, they can be added to the deterministic equation. After analyzing the effect of the shocks, he concludes that:

The result of this analysis is...a curve that is moving more or less regularly in cycles, the length of the period and also the amplitude being to some extent variable, their variations taking place, however, within such limits that it is reasonable to speak of an average period and average amplitude. In other words, there is created just the kind of curves which we know from actual statistical observations.

Unfortunately, much of the discussion of economic fluctuations to the present day suffers from a failure to use this clear-cut concept of a quasi-cycle.

In contrast to Frisch, who made no further contribution to this subject, macroeconomics was the main interest of Kalecki throughout his professional career. Despite his many articles and books on economic cycles, beginning in 1933 in Polish and after 1936 in English, his basic approach and model did not change very much. The basic formulation is contained in his 1933 paper¹. In that paper Kalecki cites no references, and it is likely that he was unaware of the cobweb models mentioned earlier. After moving to England, he published in 1935 a more elaborate paper in *Econometrica* in which he cited Frisch and Tinbergen and used their solution methods to analyze a highly sophisticated and carefully specified lag structure of the investment process.

Kalecki also makes a considerable effort to obtain range estimates of the parameters. He shows that, for plausible parameter values, the model generates cycles which lie in the observed range of 8-12 years. In subsequent publications (particularly Kalecki 1954) he tested his model against different data-sets and obtained generally plausible results.

Regarding the investment process, Kalecki distinguishes between orders for investment goods, actual investment, which takes place subsequently for the duration of the gestation lag, and additions to the capital stock, which occur at the end of the gestation period.

The rest of Kalecki's model is rather unsatisfactory. He assumes that workers do not save and that they consume a constant amount. Capitalists base their investment decision on their gross profit without an accelerator effect or the consideration of capacity utilization. Gross profits in turn are in his model a simple accounting consequence of the level of investment.

Each of the three early econometricians whose work I have just discussed attempted in his own way to use the methodology of the natural sciences to explain macroeconomic fluctuations. Their attempts remained isolated; their work was not followed up either individually or collectively. Part of the reason is the rise of Keynesianism that will be discussed next, but that is not the only reason. Some other aspects will be mentioned here.

The only one of the three authors who had a substantial impact on the subsequent evolution of macro-econometrics is Tinbergen. In turning from the original paper on the shipbuilding cycle to general macro-econometric modeling Tinbergen abandoned his original methodology. He now concentrated on the statistical testing of the individual equations entering his models, rather than on the dynamic properties of the complete model. This became the style of subsequent macro-econometrics. I don't know why he changed his methodology in this manner.

In the case of Frisch, his style was to make an important contribution to some problem and then to move on to other problems. His model of the interaction of the long and short cycles remained an isolated effort.

Kalecki, a Polish Jew and a Marxist, without any formal training in economics was the consummate outsider, a position that he retained after returning to Poland to participate in

¹ Translated as 'Essay on business cycle theory' in Kalecki (1966).

economic planning. It is not surprising that he was eclipsed by Keynes, the consummate insider.

An important element in the neglect of the work of the three authors is the difficulty of the mathematics that they used: mixed difference-differential equations. The mathematics of these is substantially more difficult than that of pure difference or differential equations. I surmise that at the time the three authors were the only ones in economics to understand the mathematics; nor was this ever a topic to engage economists subsequently. An ironic aspect of the situation is that there was no plausible reason for adopting the mathematics of Tinbergen's shipbuilding paper to macroeconomic models. The many different firms in the economy will have gestation lags of different length and they will start capital projects at different times; consequently, aggregate investment will be more nearly continuous than discrete.

Above all, I believe that the lack of response to this work is an instance of a more general problem, namely, the failure to establish in economics the natural science tradition of intensively studying stylized fact so as to determine them with a maximum of precision and to secure a collective agreement with regard to them. This theme will come up again in Section 9, in relation to the quantity theory of money.

4.4. Keynesian Economics

A vast literature deals with the 'Keynesian Revolution' most of it devoted to doctrinal controversies. I will emphasize here aspects that have received little discussion.

The evolution of classical and neoclassical economics had been driven by the desire to elaborate the formal apparatus of the theory of markets in perfect competitive equilibrium. This required no empirical input. Empiricists were attracted to the more negative aspects of competitive markets: crises, business cycles, social inequality and the cultural impoverishment of the working class. These problems were marginalized by the representatives of the mainstream. Keynes changed all that. His 1936 *The General Theory of Employment Interest and Money* (GT) was directly inspired by the Great Depression, the dominant empirical phenomenon of his time. He believed that depression due to insufficient aggregate demand would hence force be characteristic for advanced capitalist economies unless compensated by government expenditure. He regarded structurally determined insufficient demand as an essentially static problem with the consequence that the main part of the GT, intended to deal with the problem, is a static theory. Had Keynes, the empiricist, lived longer into the postwar era he would undoubtedly have realized that the basic assumption underlying his GT, the indefinite continuation of the conditions of the Great Depression, was invalid. What kind of theory is Keynes likely to have produced, given that the rhythm of the business cycle, with its alternation of periods of prosperity and recession reestablished itself. At first glance the question seems absurd; how can anyone tell what Keynes would have thought had he lived much longer and maintained his intellectual vigor? In fact, a reasoned answer can be given because it is outlined in neglected chapters of the GT.

I have stressed that the GT was motivated by Keynes's vision of a post-Depression world of mature capitalism, characterized by stagnation and chronically deficient demand. Most of the GT elaborates a model of such an economy. However, in Chapter 22, 'Notes on the Trade Cycle', Keynes cast a backward glance at a world which he thought had ceased to exist. He attempted a brief explanation of economic cycles which he explicitly described as a "nineteenth-century phenomenon".

The building blocks for Keynes' dynamic theory are contained in Chapters 5 and 12 on short- and long-run expectations. The entrepreneur is described as making his current decisions on the basis of his expectations regarding the future. Future expectations are based on extrapolations from the past, but these may be strongly influenced by irrational or volatile factors of individual or mass psychology. Keynes discusses two fundamental decisions of the firm. In Chapter 5 it is the decision on how much to produce, which is related to short-term

expectations regarding the demand for the product and also to current inventory levels. Chapter 12 is devoted to the firm's decision to invest in fixed capital. Here the decisive consideration is the relationship between the current cost of capital and its expected long-term yield. The current cost depends on the price of capital goods and the rate of interest, the expected long-run evolution of market demand and on costs of producing with old or new capital.

Keynes' attempt in Chapter 22 to construct an endogenous dynamic theory of economic cycles, based on these building blocks, has remained rudimentary. The main reason appears to be that he devoted little effort to the task, since he regarded the chapter as no more than an historical aside. Also relevant is the fact that a logically tight description of the dynamics of an oscillatory process is virtually precluded by the rather diffuse verbal style of Keynes and earlier writers on economic cycles.¹

Considering the three chapters together, Keynes made a substantial contribution to the explanation of economic fluctuations that was firmly in the tradition of the theories of investment cycles. Keynes' reflections on business cycles were a major influence on my own work.

Keynesian economics, particularly in the IS-LM formalization given to it by Hicks, dominated academic economics for several decades. The exaggerated claims connected with latter day Keynesianism, would hardly have been supported by Keynes himself. This is particularly true of the claim that the large-scale macroeconomic models would enable policy makers to ban the business cycle and usher in the age of perpetual equilibrium growth². The theory of equilibrium growth became a prime occupation of macro theorists. The turn away from Keynesianism towards monetarism and subsequently real business cycle theory and representative agent models will be the subject of subsequent sections.

The turn away from Keynesian theory in academia cannot alter the profound and lasting impact that Keynes has had on macroeconomics. He played a key role in the creation of the national income and product accounts.³ The structure of the accounts reflects that of the GT both in terms of the definition of the major sectors of the economy as well as in the emphasis on flows rather than on stocks. In the postwar period efforts were made to expand the accounts so as to integrate real and financial stocks with the flow accounts. As these efforts lacked a patron with the influence of Keynes, and as the economics profession generally moved away from an interest in measurement, the efforts remained in limbo.

4.5 Measurement

The above narrative touched on measurement at many points. The importance of measurement in science and the exceedingly strange history of measurement first in economics, later also in other social sciences, make it advisable to pull the threads together in this subsection.

The crucial event in this story is the invention of the utility concept by the Utilitarians. They stressed that utility is a subjective magnitude, different from money income. In this they were correct. The importance of the distinction is confirmed by modern happiness research, reviewed in Section 9, where it is demonstrated that above a certain minimum income, factors other than money income become important determinants of happiness. Equally important was the Utilitarians idea that the goal of economic policy should be the maximization of aggregate utility. This marked the change from mercantilism, where the aim of economic policy was seen as the maximization of the ruler's treasure, to the modern conception that the purpose of economic activity is to benefit the society.

¹ I have provided a more detailed discussion of Keynes' views on business cycles in Hillinger (1987).

² Keynes expressed skepticism, in my opinion justified, regarding the work of Tinbergen that set the style for subsequent work in macro-econometrics. See Section VI, The Tinbergen Debate, in Hendry and Morgan (1995).

³ This is discussed in Stone (1978) and more briefly in Sir Richard's 1984 Nobel Lecture.

The difficulty with the utilitarian position was that they did not know how to measure utility, though they were convinced that it was measurable in principle. They also thought that when a way to measure utility was found, it would be done as in the natural sciences by means of a cardinal scale. This would allow the total utility to be determined by simple addition of the numerical values obtained for the individual utilities. Lacking both a mathematical model of the economy as well as precise quantitative measurements, the utilitarian maxim of ‘the greatest happiness for the greatest number’ remained a vague exhortation rather than a precise guide to action.

In the utilitarian analysis, a consumer’s optimum is defined by the condition that the expenditure of one monetary unit on any commodity would bring the same increment of utility.

The nature and implication of utility as a subjective magnitude has remained the central and unresolved problem of economics throughout its history. Since the economic approach came to dominate collective choice theory, it has also become the central and unresolved problem of political theory.

The marginalists, in their endeavor to replace classical economics, rejected the utilitarian analytical apparatus. They showed that the consumer’s optimum is defined by the condition that the rates of substitution between commodities that keep utility constant is equal to the inverse of the ratio of their prices. In this formulation there is no need to consider any utility increments. They argued further that such increments could not be observed and that they were therefore a useless metaphysical construct. This pointed the way towards utility as an ordinal concept, with indifference curves indicating rates of substitution, and utility levels regarded as irrelevant.

This argumentation on the part of the early marginalists was not entirely fair, since they were able to do without cardinal utility only because they ignored the problem for the solution of which cardinal utility was invented: the determination of a social optimum. With the rise of general equilibrium theory this problem came again to the fore. It was solved in the spirit of marginalism by Pareto. He showed that the equilibrium that results under perfect competition has the property that not all individuals can be made better off. This condition of ‘Pareto optimality’ also makes no use of any utility increments. The Pareto optimum demonstrates the efficiency of a competitive economy. It is weaker than the *optimum optimorum* envisaged by the Utilitarians, since the income distribution generated by a competitive economy may be unacceptable. On this the Pareto principle is silent.

The next crucial development was the publication in 1932 of Lionel Robbins *An Essay on the Nature and Significance of Economic Science*. The essay was directed against the socialist claim that economic and social policy could and should be based on science. Robbins’ argument was based on the marginalist position according to which interpersonal utility comparisons are impossible. Actually, Robbins made the more modest claim that such comparisons lacked a scientific basis. Since the determination of a social optimum apparently requires such comparisons, it follows that policy aiming at the attainment of such a state cannot be entirely scientific.

What Robbins actually said should not have been very controversial. A reasoned reply came from Myrdal (1969) who argued that those who propose a policy should make their values explicit. Unfortunately, Robbins’ essay did not lead to a reasoned debate among economists. Instead, the stronger assertion that ‘interpersonal comparisons of utility are impossible!’ usually preceded by ‘Of course!’ became a mantra by means of which economists signaled that they were members in good standing of the academic economic mainstream. This strong form of impossibility assumption became the foundation of Arrow’s work and, as discussed in Sections 2 and 8, of modern collective choice theory generally.

The conviction of the impossibility of interpersonal utility comparisons also impacted economic measurement. The pervasive economic measure is money. The Utilitarians had

made the distinction between money and utility and had given priority to the latter. This amounted to a de-emphasis of the money measure. As already described, the first half of the Twentieth Century saw a great expansion of social and economic statistics, produced with the aim of promoting a scientific approach to the solution of social problems. Many of these measures were expressed in monetary form, most importantly GDP. For economic theorists there arose the problem of relating the monetary measures to changes in welfare that in turn had to be defined in relation to utility.

For the GDP, the solution was sought via the Pareto criterion and various compensation measures. It was argued that an increase of GDP signified that the winners could compensate the losers, so that, at least in principle, the Pareto criterion would signify an improvement.

The other measure that theorists paid much attention too was Dupuit's measure of consumer surplus. It was typically used to calculate if the benefit of a project that would result in a lower price to consumers for some service would result in a greater benefit than the cost involved.

Around 1950, Paul Samuelson demonstrated that all of these attempts were irreparably flawed. The consequence was that economic theorists largely abandoned problems of measurement. This did not in any way reduce their practical importance; instead, theory and practice simply parted ways.

It is highly ironic that cardinal measurement was abandoned by theorists in economics and political science, precisely the two fields that have natural cardinal measures in the form of money and the ballot. The opposite path was taken in psychology. Here central concepts such as intelligence, or various emotions, have no natural metric, but immense efforts were made to construct such metrics. Sophisticated statistical methods such as factor analysis and multi-dimensional scaling were developed for the purpose. Where scientism in economics concentrated on the construction of mathematical models, in psychology it concentrated on measurement.

As already described, my own work on measurement and on voting essentially involved the rejection of the self-defeating and ideologically motivated commitment to ordinalism. Instead, I elaborated the theory of the naturally available cardinal measures.

5. FREE MARKETEERS: THE CHICAGO SCHOOL

5.1 The Intellectual Background

Immediately following The Second World War and during the following decades, socialism appeared to be the wave of the future. This was certainly the view of almost all intellectuals, of artists and of the rising elites of the newly liberated Third World countries. Concerted efforts were made at two American institutions to advance a counter ideology of unrestrained markets and limited democratic government. The earlier effort centered on the Rand Corporation, another was started about two decades later at the University of Chicago. Each of these efforts is described in a recent book; the Rand story is covered in Amadae (2003), that relating to Chicago in Leeson (2000). In this section I deal with the Chicago story because it is the logical continuation of the discussion of Keynesianism and early econometrics. First though I discuss the intellectual atmosphere that motivated the counterattacks organized at Rand and at Chicago. Amadae describes it as follows:

...during the 1930s and 1940s there was a pervasive sense of dismay and defeat among the intellectuals of the West over what they took to be the inevitability of the triumph, both internal and external, of fascist or communist alternatives to democratic capitalism. Marxism – forged during the early days of mass production, mass warfare, and mass democracy – appeared to be the inevitable, if not yet victorious, structural principle that eventually would govern world affairs. (p. 2)

The most prominent defenders of political and economic freedom in the Interwar Period were themselves pessimistic:

Schumpeter's *Capitalism, Socialism, and Democracy* (1943), Hayek's *The Road to Serfdom* (1944), and Popper's *The Open Society and Its Enemies* (1945) convey the tenuous foothold of Western institutions of democracy, capitalism, and science before the rational choice revolution. These three widely read books, each expressing grave apprehension over the future viability of these sacred institutions, outlined the philosophical crises at the heart of Western civilization. Schumpeter and Popper devoted much of their works to analyzing Marx's critique of capitalism; Hayek turned his attention to the ills of socialism and collectivism more generally. Each liberal theorist, to an extent unimaginable from our perspective following the collapse of Soviet communism, readily admitted that capitalism run wholly according to the *laissez faire* ideal did not strike the appropriate balance between social justice and economic freedom and was therefore untenable. (p.16)

Leeson has discussed the affinity of the early econometric movement to socialism:

The econometrics movement was molded, to a large extent, by the desire to understand and tame the interwar business cycle...

Keynesian and Marxian economics are modern versions of the 'endogenous instability of capitalism' thesis...The Great Depression gave an added dimension to these controversies. In the 1930s, many observers were concerned about the long run viability of capitalism and of the apparently infeasible combination of political liberty and economic freedom...Marschak initiated one of the earlier debates on the viability of socialism as an economic system, involving Pareto, Barone, von Mises, Schumpeter, von Hayek, Lange and Lerner. The 1940 Cowles Commission Report stated that unemployment was the primary economic problem to be tackled...Many economists lost their faith in the ability of markets to solve the problem of unemployment, and many embraced the new faith of economic planning. Tinbergen...for example, retrained as an economist under the influence of the onset of world depression; he regarded his econometric work, and in particular his estimation of parameters, as providing the tools to effect socialist intervention in the economy in order to minimize cyclical fluctuations and poverty...

Lawrence Klein shared this approach to econometrics; later he would be persecuted because of his socialist convictions. Harold Hotelling also favored market socialism...Oscar Lange was a 'proclaimed socialist', and later a member of the Polish Communist government...Ragnar Frisch also had socialist leanings, according to Tinbergen...Frisch came to believe that uncovering the underlying structure of the economy – the structural parameters – would enable the business cycle to be tamed...Part of this optimism may reflect the initial training in physics which Tinbergen, Frisch, Koopmans and others had been exposed to...These ideological undercurrents were present in many of the business cycle research institutes which were established all over Europe and the United States in the 1920s. (pp. 14-15).

The socialist leaning of the pioneer econometricians is likely to have been a factor in Friedman's rejection of structural macro-econometric modeling. The question of the usefulness of such models should be a purely scientific one. When developments are driven by a variety of ideological commitments, both political and methodological, a state can be reached when young scholars entering the profession have no idea of what originally motivated the various, often counterproductive, assumptions that they are taught to make by the adherents of one school or another.

5.2 Chicago Schools

Leeson (2000) who is the primary source for the present section focuses on the ideological campaign launched by Friedman and Stigler at the economics department of the University of Chicago during the 1960s. Both are however members of a tradition that dates further back. Reder (1987) dates the beginning of an identifiable 'Chicago School' to around 1930 with the arrival in the department of Frank H. Knight and Jacob Viner.

What Knight and Viner had in common was a continuing adherence to the main tenets of neoclassical price theory and resistance to the theoretical innovations of the 1930s, Monopolistic Competition and Keynes's General Theory. This theoretical posture paralleled an antipathy to the interventionist aspects of the New Deal and the full employment Keynesianism of its later years.

Reder identifies a further important characteristic of the Chicago School:

For over half a century, the need to prepare for course and preliminary examinations, especially in price theory, has provided a disciplinary – cultural matrix for Chicago students. Examination questions serve as paradigmatic examples of research problems and 'A' answers exemplify successful scientific performance. The message implicit in the process is that successful research involves identifying elements of a problem with prices, quantities, and functional relations among them as these occur in price theory, and obtaining a solution as an application of the theory. Although the specific content of examination questions has evolved with the development of the science, the basic paradigm remains

substantially unchanged: economic phenomena are to be explained primarily as the outcome of decisions about quantities made by optimizing individuals who take market prices as data with the (quantity) decisions being coordinated through markets in which prices are determined so as to make aggregate quantities demanded equal to aggregate quantities supplied.

5.3 Friedman and Stigler

The above characteristics of the Chicago School have remained in place; however with the advent of Friedman and Stigler the ideological battle assumed a new intensity and novel methods were employed in waging it. A further dramatic change occurred with the advent of Robert E. Lucas. I will therefore distinguish three Chicago Schools, shaped by Knight and Viner; Friedman and Stigler; finally, Lucas. After Lucas, the Chicago School merges with general trends in the economics profession that were however strongly influenced by the Chicago School.

As a label for the political ideology of the Chicago School in all three of its manifestations I will use the current term 'neoliberalism' that has the same meaning as 'neoconservatism', the term often employed in the United States.

Both Friedman and Stigler viewed themselves as carrying forward the tradition of Frank Knight. The following quotation from Leeson illustrates both continuity and dramatic changes in emphasis.

...Friedman and Stigler did not share Knight's fatalistic despair about the Chicago project for social and economic transformation... neither did they share his lack of interest in empirical or policy-oriented economics or his belief in the inherent contradiction between thought and action.... Both attended his seminars on the sociologist Max Weber; he was their dominant dinner-table subject of conversation... This sociological perceptiveness involved doubt about the outcomes of rational debate: 'Frank Knight's First Law of Talk' was that 'cheaper talk drives out of circulation that which is less cheap'...

...The modern Chicago School began to take shape, culminating in the famous 1960 'Coase versus Pigou' evening at Aaron Director's house which was 'the most exciting intellectual event' of Stigler's life. The evening ended up with 'no votes for Pigou' and effectively partitioned economics into two epochs: A.C. and B.C. ('Before Coase'). According to Stigler... the previous epoch had confused 'all economists... from at least 1890 until 1961'. The realization that Pigou had previously obtained such a 'hold', even in places like Chicago, generated both enthusiasm and sociological reflectiveness. It also laid out a detailed research agenda for the Chicago School and the *Journal of Law and Economics*... The potency of the Coase conversion evening may have been intensified for Stigler given the 'mistakenly' Pigovian role that Coase allocated to the second edition of his *Theory of Price* (1952). (pp. 57-58).

Leeson's description of this event as a 'conversion' is well chosen. The meeting resembles one of evangelists, culminating in a mass conversion. It is instructive to examine the 'Coase Theorem', as it was later called, that elicited such religious fervor in order to gain an insight into the nature of ideological beliefs. The theorem states that in the presence of externalities private parties can arrive at an efficient solution in the absence of government regulation. In the case of an external bad, those affected by it can pay the offender to desist. In the case of an external good, those who desire to have it can pay the producer to supply it. Prior to Coase, the standard position on externalities had been that of Pigou who advocated government intervention in order to restore efficiency.

As Coase well realized, his theorem requires **a.** the absence of transaction costs. Another obstacle **b.** that is prominent in the literature is the 'free rider' problem that arises whenever many individuals are subjected to an externality. I believe that there are several other serious obstacles. One **c.** typically occurs when a developer wishes to acquire a number of adjacent plots in order to realize a development project. If his plan becomes public when he has acquired most of the plots, the remaining land owners can hold out for unreasonably high payments, since they are in a position to block the project. **d.** The compensation required may be larger than the means available to effected persons. In Germany there was a few years ago a legal case in which the owners of land and dwellings below a railroad bridge sued the national German railroad to force it to take action to prevent the flushing of toilets as the trains passed overhead. I don't know the outcome of the legal case, but I think that a payment

to the railroad that would cause it to voluntarily desist, creating a precedent, would far exceed the cost of abandoning the land. **e.** Finally, as is suggested by the previous example, most people would feel a moral revulsion at paying an offender to desist. Such payments would also be an incentive for deliberate production of negative externalities in order to be paid for ceasing. The usual name given to such an action is extortion. For all of these reasons, private payments to deal with externalities are almost never observed. In reality the importance of the Coase solution is negligible relative to the Pigou solution.¹

How was it possible for a result that objectively appears quite modest to have been received with quasi-religious euphoria? I believe the answer is ‘tunnel vision’. I borrow the term from Schaef (1987). The author is a psychologist who worked with Alcoholics Anonymous and applied insights obtained there to society at large. In AA it was observed that reformed alcoholics often acquired a new addiction. Sometimes it was another drug, such as smoking. However, the new addiction could also be to an idea; most often a fanatical desire to reform other alcoholics. Schaef found the phenomenon of addiction to be a characteristic feature of modern societies, attaching itself to a variety of activities, feelings and ideas. All consuming drives to consume, or to work (‘workaholic’) are obvious examples. Ideologies also produce fanatical adherents, willing to kill if they attain the means, as history abundantly testifies. To the intellectual, living in a more stable and peaceful society, the task becomes that of impaling the opponent intellectually with a stiletto-like argument.

I cannot in this space summarize Schaef’s book on the psychology of addiction, but I will mention two characteristic traits: One is tunnel vision, which means that the addict (ideologist) ignores or denies those aspects of reality that contradict the picture of the world that he is attached to. The other is a diminishment of the moral sense. The alleged ultimate benefits promised by the ideology are taken to justify the twisting of truth, or even outright lies and other evil deeds in the present.

The religious fervor, biased argumentation, selective use of evidence and aggressive debating style that characterize the Chicago School as shaped by Friedman and Stigler can in my view be completely understood only by means of the psychology of addiction.

The Architecture of the Friedman/Stigler Chicago School

The logical structure created by Friedman and Stigler to advance their ideological agenda can be compared to a three-tiered building. The upper floor contains the neoliberal ideology. It is supported by the lower floors, each of which consists of two parts. The ideology rests directly on the second floor that contains the appropriate economic theory; the micro-part being the responsibility of Stigler, the macro-part of Friedman. The ground floor contains Friedman’s methodology of positive economics, used to justify the theory constructed on the floor above. It also contains Stigler’s sociology of economics, the basis for the effective advocacy of theory and ideology. In the writings of the two and of their followers the four building blocks tend to intermingle, with sometimes one, sometimes another assuming center stage.

Stigler’s Sociology of Economics

Some preliminary remarks: **a.** Stigler often refers to economics and economists, but sometimes to science generally. I don’t believe that he ever seriously studied the natural sciences. His writings in a sociological vein should be interpreted as being about economics, not science generally. **b.** He writes impressionistically, his claims are hardly ever backed by evidence; of course, that does not mean that they are false. **c.** Following Leeson, I also use the term ‘sociology’. The sociology of science is a well established field of sociology, founded by

¹ An excellent critical discussion of the Coase theorem is Chapter 3 in Olson (2000).

Robert K. Merton.¹ Stigler's writings are not up to the standard established there; they can better be regarded as sociological impressions and as guides to persuasion.

A key feature of Stigler's sociology is elitism. I quote from Leeson's section entitled 'The elite and the masses'.

Stigler clearly distinguished between 'major scientific entrepreneurs' and the rest, some of whom could only employ 'an inferior mind', and some of whom were 'ersatz economists'. They entered the market as demanders, not suppliers, of ideas, and conference participants reminded Stigler of traveling salesman exchanging stale jokes. He believed that economists were analogous to the purchasers of second-hand automobiles and he wondered why some ideas 'wouldn't run far or carry many passengers'. Part of the explanation involved a quality differential: 'which socialist propagandist has been as logically lucid as Friedman?'

Stigler concluded in his study of 'The Literature of Economics' that two-thirds of the articles surveyed were virtually worthless. There were commonly only about six really first-class scholars in any field; a small minority in the profession had 'superb instincts' with regard to the pursuit of ideas. The 2 or 3 per cent of the profession who were 'active and ambitious' were also 'reformers of economic science' Occasionally, 'economists-missionaries' successfully ventured into the territory of 'apprehensive and hostile natives', but academic consensus (which could be unreliable) was achieved not by a professional 'plebiscite', but only by an elite group within the profession. Science was defined as the consensus interpretations that emerged from this process. In contrast, for the 'mass' of scholars in any discipline, risk aversion, and a desire to preserve already acquired human capital, created a bias in favor of scientific conservatism. Given this structure of the sociology of economic knowledge, the 'most irresistible' of all the weapons of scholarship was 'infinite repetition' a 'form of the classical Chinese torture'. (pp. 51-52).

In a section entitled 'The technique of the huckster', Leeson brings the following quotation:

Great economists are those who influence the profession as a whole, and this they can do if their doctrines do not involve too great a change from the views and the knowledge of the rank and file of the science. It is simply impossible for men to apprehend and to adopt wholly unfamiliar ideas...New ideas are even harder to sell than new products...One must put on the best face possible and much is possible. Wares must be shouted – the human mind is not a divining rod that quivers over truth. The techniques of persuasion also in the realm of ideas are generally repetition, inflated claims, and disproportionate emphases, and they have preceded and accompanied the adoption on a large scale of almost every new idea in economic theory...It is possible by mere skill of presentation to create a fad, but a deep and lasting impression on the science will be achieved only if the idea meets the more durable standards of the science. Among these standards is truth, but of course it is not the only one. (pp. 52-53).

The final sentence raises the question of how agreement on 'truth' is to be secured. The reference to other criteria that exist 'of course' is puzzling; can there be criteria in science other than those that help to determine the validity, i.e. the truth of theories and generally of statements? It may be that the sentence is meant to refer more to empirical than to normative determinants of success. I quote Leeson on what in Stigler's view determines the long run acceptance of theories in economics.

Stigler noted that economists have a tendency to 'float on the tide of theory'. He reflected that empirical generalizations 'fail to achieve the continuity and the widespread influence of the formal theories' He was also aware that for theorists, statistically derived relationships could be 'frankensteins over which he has little or no control'...

With respect to conclusions that contained policy implications, Stigler sought to elevate received theory over empirical analysis. The reason for this confidence in orthodoxy was that it was 'our most tested and reliable instrument for relating policies to effects'. Received theory, presumably, operated with a considerable advantage. The idea that a new theory 'is presumed innocent until shown guilty...is the exact opposite of the presumption I would use'. Not all theorists were to be trusted; unorthodox approaches were sometimes denied the label 'theory'. (p. 50).

Stigler's sociology of economics is a defense of orthodoxy. This defense is based on what I would call the 'Stigler twist'. Neither Stigler nor Friedman denies that only empirical testing should ultimately decide the fate of theories. Both stress however that testing is difficult and laborious and particular tests often indecisive. Stigler argues that orthodoxy has become that because of the long period of testing that it has successfully survived. We *should* therefore be biased in favor of orthodoxy. Under the Stigler twist, what begins as an empirical question

¹ Not to be confused with his son, the economist Robert C. Merton.

ends up as a methodological prescription. Of course, that orthodoxy has become that because of a long history of successful empirical testing is precisely what its critics would deny. From Marx to current opponents of globalization, they would argue that the persistence of orthodoxy is due to the fact that it provides the argumentative foundation for policies that favor the rich and powerful, who in turn have the better means of influencing opinion.

Friedman's Methodology of Positive Economics

Leeson has very little to say about Friedman's methodology of positive economics. This is surprising given how influential this methodology has been, both generally and in advancing the Friedman/Stigler agenda. I believe that for most economists Friedman's methodology can be expressed in one sentence: Realism of the assumptions of a theory does not matter; all that matters is the theory's predictive success. I will refer to this as the 'vulgar interpretation' of Friedman's methodology. I confess to having believed in the vulgar interpretation myself until rereading *The Methodology of Positive Economics* for the purpose of writing this section. What Friedman actually wrote is quite different and to my surprise I found myself in virtually complete agreement.¹ In the following I will review what Friedman actually wrote, followed by an evaluation and a discussion of the damage done by the vulgar interpretation.

The interpretation of Friedman's methodology hinges on the meaning one assigns to 'plausibility of assumptions'. Friedman makes it very clear that his purpose is to defend economic theory against those who attack its lack of 'descriptive realism'. The following passage defines the essence of his position and also points to the economic controversies that motivated his exposition:

In so far as a theory can be said to have "assumptions" at all, and in so far as their "realism" can be judged independently of the validity of predictions, the relation between the significance of a theory and the "realism" of its "assumptions" is almost the opposite of that suggested by the view under criticism. Truly important and significant hypotheses will be found to have "assumptions" that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense). The reason is simple. A hypothesis is important if it "explains" much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone. To be important, therefore, a hypothesis must be descriptively false in its assumptions; it takes account of, and accounts for, none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained. To put this point less paradoxically, the relevant question to ask about the "assumptions" of a theory is not whether they are descriptively "realistic", for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions...

The theory of monopolistic and imperfect competition is one example of the neglect in economic theory of these propositions. The development of this analysis was explicitly motivated, and its wide acceptance and approval largely explained, by the belief that the assumptions of "perfect competition" or "perfect monopoly" said to underlie neoclassical economic theory are a false image of reality. And this belief was itself based almost entirely on the directly perceived descriptive inaccuracy of the assumptions rather than on any recognized contradiction of predictions derived from neoclassical economic theory. The lengthy discussion on marginal analysis in the *American Economic Review* some years ago is an even clearer, though much less important, example. The articles on both sides of the controversy largely neglect what seems to me clearly the main issue – the conformity to experience of the implications of the marginal analysis – and concentrate on the largely irrelevant question whether businessmen do or do not in fact reach their decisions by consulting schedules, or curves, or multivariable functions showing marginal cost and marginal revenue... (pp. 14-15).

The most salient characteristic of the Chicago School is generally taken to be an unqualified belief in the competitiveness of existing markets. It is interesting to read what Friedman had to say about this:

¹ I must have read *The Methodology of Positive Economics* decades ago, perhaps as a graduate student. My agreement with it could be the reason for my lack of a specific recollection of its contents.

The confusion between descriptive accuracy and analytical relevance has led not only to criticisms of economic theory on largely irrelevant grounds but also to misunderstanding of economic theory and misdirection of efforts to repair supposed defects. “Ideal types” in the abstract model developed by economic theorists have been regarded as strictly descriptive categories intended to correspond directly and fully to entities in the real world independently of the purpose for which the model is being used. The obvious discrepancies have led to necessarily unsuccessful attempts to construct theories on the basis of categories intended to be fully descriptive.

This tendency is perhaps most clearly illustrated by the interpretation given to the concepts of “perfect competition” and “monopoly” and the development of the theory of “monopolistic” or “imperfect competition”. Marshall, it is said, assumed “perfect competition”; perhaps there once was such a thing. But clearly there is no longer, and we must therefore discard his theories. The reader will search long and hard-and I predict unsuccessfully-to find in Marshall any explicit assumption about perfect competition or any assertion that in a descriptive sense the world is composed of atomistic firms engaged in perfect competition. Rather, he will find Marshall saying: “At one extreme are world markets in which competition acts directly from all parts of the globe; and at the other those secluded markets in which all direct competition from afar is shut out, though indirect and transmitted competition may make itself felt even in these; and about midway between these extremes lie the great majority of the markets which the economist and the business man have to study.” Marshall took the world as it is; he sought to construct an “engine” to analyze it, not a photographic reproduction of it. (pp. 34-35).

Friedman gives an example involving the American cigarette industry during The Second World War. The firms appear to have been mainly concerned about maintaining market share and the predictions of the competitive model would have been falsified. The balanced position taken by Friedman in his 1953 essay may not be typical of his later views or of the Chicago School as an institution. The latter, according to Bhagwati, cited by Leeson, was

...very Friedmansque... The seminars seemed to oscillate between proving that elasticities were large with markets therefore stable, and formulating competitive hypotheses for apparently imperfectly-competitive industries and coming up with high enough R^2 s. Econometrics was the handmaiden of ideology: things looked imperfect to the naked eye, especially to that of Chamberlin and Joan Robinson, but they were ‘really’ not so and the world was ‘as if’ competitive...market imperfections were ‘demonstrated’ to be negligible and the imperfections rather of government intervention were the subject of active research. (p. 58).

My conclusion is that Friedman’s essay on methodology is excellent and still very worthwhile reading for an economist. In essence it is a defense of the scientific method, with its emphasis on simplicity, against the claims of the adherents of descriptive realism. The limitation of the essay is that it very largely equates plausibility with descriptive realism. This is both false and the root of the vulgar interpretation that has done much harm. Plausibility is a much more general concept. The claims of critics notwithstanding, the basic assumptions of economic theory, that consumers try to get a maximum of satisfaction from their purchases and that firms attempt to maximize their expected long-run profits, are in fact highly plausible and constantly born out by experience.¹ Criticism is often aimed at a straw man, the *homo oeconomicus*, assumed to have complete information and perfect rationality. Economic theorists bare some fault for these misinterpretations because they seldom elaborate the concrete functioning of their concepts in empirical contexts.

The situation in economics today is quite different from that in 1953 when Friedman published his essay. Today the most prestigious approach in economics is applied econometrics and the central concept employed there is not the individually rational agent of traditional economic theory, but rather the *representative agent*. As discussed in detail in Section 6, the representative agent concept is incompatible with the concept of individually rational agents. It is therefore completely implausible in the light of traditional economic theory. It is only in the light of the vulgar interpretation that realism of assumptions does not matter, that the representative agent concept appears acceptable.

¹ As with all theories that have wide applicability, there are many and well known qualifications. For example, the interests of managers and shareholders often differ; a high price may contribute to the prestige attached to a consumer product. Such qualifications do not render the theory empty.

Economic Theory and the Chicago School

I will deal only briefly with the microeconomic position of the Chicago School. There are two reasons: One is that this is not my own field of research and expertise. The other is that the microeconomic position of the Chicago School is not very original. Essentially it is an affirmation of Marshallian theory against the claims of the proponents of the theory of imperfect competition formulated by Edward H. Chamberlin and Joan Robinson. I believe that this position is correct; the theory is simply not being used in the empirical analysis of economic conditions. Empirical analysis generally assumes competitive or oligopolistic industries, or monopolies. This is the position taken by Friedman in his essay on positive economics. Beyond this, I do not wish to deny a tendency on the part of members of the Chicago School, and more generally of adherents to the neoliberal ideology, to claim the existence competitive conditions where these may be objectively in doubt.

The macroeconomic position of the Chicago School, as developed by Friedman, is also simple to state. More complex is the history of its influence.

Friedman's macroeconomics stands Keynes on his head. Keynes believed the economy to be without a self-regulating mechanism that would lead to full employment. An actively managed fiscal policy was required. Monetary policy would be ineffective, due to various 'pathologies', particularly the 'liquidity trap'. Friedman takes the contrary position: The economy is inherently stable. Deviations from full employment are due to exogenous shocks, primarily due to an erratic monetary policy. The cure is to expand the money stock at a constant rate.

Friedman was instrumental in rehabilitating the quantity theory of money; much empirical work on it was done by members of the Chicago faculty and graduate students. An implication is that in order to have price stability, or a very low inflation rate, the money stock should grow at, or slightly above, the growth rate of real GDP.

Leeson very largely attributes the success of the Friedman/Stigler Chicago School to the efforts and persuasive skills of these two economists. I am more skeptical and a believer in the internal dynamics of ideologies, major or minor. Almost by definition, ideologists have tunnel vision, ignoring or denying the aspects or reality uncongenial to them. Reality however has a way of catching up. The first half of the Twentieth Century witnessed the ascent of totalitarianism and socialism or its milder kin, social democracy or American style liberalism. Even the Nazis called themselves Nationalsozialisten. Around 1970 the governments of the left were everywhere faltering and on the defense. They were experiencing rising unemployment, rising inflation, rising state deficits and ever larger public debts. The standard Keynesian remedies that they professed to be following no longer worked. Two related ideologies were thus on the defense; the broad political ideology of the left and Keynesianism as the set of macroeconomic beliefs shared by mainstream economists. The common element in these ideologies was the faith in active government intervention. The stage was thus set for a reversal of direction in both politics and economics. Who had how much effect in actually bringing the changes about? I cannot answer that question, nor does it interest me very much. Leeson stresses the influence of the Chicago School, specifically of Friedman and Stigler. Amadae, whose work is discussed in Section (7), emphasizes the role of the Rand Corporation and associated scholars. In politics, Margaret Thatcher and Ronald Reagan executed decisive turns to the right. Friedrich Hayek and Karl Popper are often mentioned as early and influential critics of leftist ideology.

A pillar of (post-Keynes) Keynesianism was the Phillips curve, an alleged negative association between the inflation rate and the rate of unemployment. Friedman challenged the Phillips curve in an influential presidential address to the American economics association in 1967. He argued that once the population had become used to a given rate of inflation they would tend to extrapolate it into the future. Prices and wages would accordingly rise in anticipation so that there would be no positive stimulus to real demand and hence to

employment. He advanced the concept of a *natural rate of unemployment* to which the economy would tend. Leeson points out that a number of prominent economists had provided similar arguments, but without much impact.

But these (mostly scattered) judgments were not packaged in such a way as to convince the economics profession of the un-wisdom of believing in the long-run inverse trade-off. Only Friedman it seems was able to accomplish that. (p. 87).

Was it packaging or timing? Most likely both.

6. REPRESENTATIVE AGENTS

The representative agent concept has come to dominate applied economics. The reason is that applied economics almost always deals with aggregate data, while the central concept of economic theory is the maximizing agent. The representative agent combines these two aspects in the simplest possible manner by assuming that the aggregate data behave as though they were the outcome of the decision making of a single maximizing agent.

For my discussion in this section I rely on Hartley (1997) *The Representative agent in Macroeconomics* as well as on other sources and my own relevant work. As the title of his book indicates, Hartley is primarily concerned with the use of representative agent models in the contemporary macroeconomic mainstream that is referred to as new classical macroeconomics or real business cycle theory. This is somewhat limited, since the representative agent concept is also central to modern public economics and the difficulties connected with it are the same, regardless of which application is being considered. The book does have an excellent and detailed discussion of the historical origins of the concept.

The representative agent is a very strange concept because it has been rejected by virtually everyone who has seriously considered the question of its justification; this rejection however has done nothing to curtail its popularity. The representative agent model would be justified if it were possible, starting from the assumption of individually maximizing agents, to deduce that observable aggregate variables would behave as though they were the outcome of the maximizing behavior of a single maximizing agent. That such a drastic reduction in complexity should be possible is implausible on the face of it. Aggregation theory confirms this judgment and shows that such a result is possible only under implausible conditions.

Hartley writes:

Under what conditions will we be able to derive a consistent representative agent (or equivalently a macro model)? The most important conditions are in Gorman (1953): If all agents have parallel, linear Engel curves, or equivalently, if all agents have identical homothetic preferences, consistent aggregation is possible...

Since Gorman wrote, there have been several papers providing additional conditions which will yield consistent aggregation... There is no real need for us to explore all of these conditions in detail. It is enough to note that every one of them is thoroughly implausible; it would be remarkable in and of itself if anyone argued that any of these functional forms was in any way realistic. Indeed, Lewbel (1989, p. 631), after an entire paper devoted to developing general forms which allow for aggregation, concludes, "It is a fact that the, use of a representative consumer assumption in most macro work is an illegitimate method of ignoring valid aggregation concerns." (p. 134).

In concluding his review of the literature on representative agents Kirman (1992) writes:

Given the arguments presented here – that well-behaved individuals need not produce a well-behaved representative agent; that the reaction of a representative agent to change need not reflect how the individuals of the economy would respond to change; that the preferences of a representative agent over choices may be diametrically opposed to those of society as a whole – it is clear that the representative agent should have no future. (p. 134).

Franklin M. Fisher (1987) gives a broad review of all aspects of the aggregation problem; over consumers, over firms, and over commodities. He concludes:

Such results show that the analytic use of such aggregates as 'capital', 'output', 'labour' or 'investment' as though the production side of the economy could be treated as a single firm is without sound foundation. This has not discouraged macroeconomists from continuing to work in such terms.

How is it possible that this devastating and largely unanimous criticism of the representative agent concept is essentially being ignored by the mainstream? Much of

Harley's book is devoted to finding answers. Before turning to these, some preliminary comments:

It should be pointed out first of all that the rejection of the representative agent is by no means a rejection of macroeconomics, in the sense of constructing and estimating models involving relationships between macro-variables. Relationships between macro-variables have long been observed and are fairly stable.¹ That relationships between micro-variables that generally go in the same direction at the individual level should be evident also in macro-data is highly plausible. For example, individuals consume on average roughly constant proportions of their income. Unless there are large shifts in the income distribution between households with different propensities to consume, a similar relationship will also be observed in the aggregate. In traditional macroeconomic modeling, such relationships are simply estimated from the data. In contrast to this, representative agent models impose prior restrictions on the estimate. These restrictions have two sources: one is the general maximization assumption. An example would be the Slutsky equation imposed on estimates of consumer demand functions. Other restrictions result from the specific parametric functions that are assumed in order to make the econometrics of these models tractable, for example Cobb-Douglas or CES production or utility functions. All of these restrictions are illegitimate, given that the representative agent model itself is deeply flawed.

At this point I introduce a distinction and terminology used by Hartley. Marshall had founded partial equilibrium analysis with the idea that an industry may respond to a common disturbance with the same kind of response as the individual firms comprising it. In fact, he used this condition to define an industry. In this context he also referred to the response of the industry as being that of a representative firm. This use of the term is innocuous, but also superfluous. This type of analysis will be referred to as *Marshallian* and this term can be applied also to the associated concept of a representative firm.

The representative agent models used in contemporary economics are *Walrasian* in that they embody the central assumptions originally employed by Leon Walras. The basic substantial assumptions are that the economy is made up of rational agents, maximizing profits in the case of firms and utility in the case of consumers. The agents are always in a state of competitive equilibrium; when this is disturbed, they move instantaneously to the new equilibrium. From the methodological point of view, the emphasis is on the rigor of the mathematical analysis of the models; empirical relevance of the models is not seriously considered or tested.

Hartley cites Walras:

The pure theory of economics ought to take over from experience certain type concepts, like those of exchange, supply, demand, market, capital, income, productive services and products. From these real-type concepts the pure science of economics should then abstract and define ideal-type concepts in terms of which it carries on its reasoning. The return to reality should not take place until the science is completed and then only with a view to practical applications. Thus in an ideal market we have ideal prices which stand in an exact relation to an ideal supply and demand. And so on. (p. 60).

Everyone who has studied geometry at all knows perfectly well that only in an abstract, ideal circumference are the radii all equal to each other and that only in an abstract, ideal triangle is the sum of the angles equal to the sum of two right angles. Reality confirms these definitions and demonstrations only approximately, and yet reality admits of a very wide and fruitful application of these propositions. (p. 61)

Hartley comments:

If, in the real world, one does not find circles with equal radii, one does not alter the theory of the circle. So it is to be with economics. Economists should seek to construct the "true" model of the economy. Walrasian models are thus not subject to our battery of econometric tests. It is impossible to empirically verify or deny the conclusions or the accuracy of the model; they are simply held to be true.

¹ Empirically constant ratios between macro-variables that are relevant for economic growth are discussed in Klein and Kosobud (1961). Business cycle stylized facts observed in macro-variables are discussed in Hillinger (2005).

The severity of this break between reality and economic theory is illustrated by a mistake Walras made in later editions of *Elements*. Milton Friedman... has noted that as Walras progressed in the development of his model through successive editions of *Elements*, he (Walras) lost sight of the real world counterparts of the concepts in his model. In the first three editions of *Elements*, Walras carefully distinguished savings (flows) from consumption goods (stocks). However, in the fourth edition, Walras suddenly treats savings as merely another good. Friedman argues:

Surely, the explanation must be that when Walras made the change in the fourth edition, he no longer had the system and its meaning in his bones the way he did when he developed it; he was taken in by considerations of pure form; the substance which the form was to represent was no longer a part of him. It would be hard to find a better example of the nonsense to which even a great economist can be led by the divorce of form from substance. (p. 61).

In Chapter 7 of his book Hartley demonstrates that the dominant modern school of macroeconomics, the new classical economics, based on the use of the representative agent concept, continues the Walrasian methodology. Rather than quoting from the body of the chapter, I cite the summary at the beginning of the chapter:

It is now apparent that in order to determine the propriety of the new classical use of representative agent models, we need to establish which methodological tradition is being followed. The purpose of this chapter is to demonstrate that new classical economics follows the Walrasian tradition. The implication immediately follows that since the representative agent is a poor instrument with which to construct Walrasian models in general, it is not of much use in helping the development of new classical Walrasian models.

New classical economics is at its heart a methodological school. It is not a set of policy conclusions or views about the real economy which set these economists apart. Rather, what makes new classical economics distinct is the methodology it uses. (p. 84).

With the advent of the representative agent macroeconomics as well other parts of economics have disassociated themselves from reality.

7. SCIENCE IN MACROECONOMICS: A VIEW FROM THE MAINSTREAM

After completing a version of this paper. I came across a new working paper by Gregory Mankiw (2006), titled *The Macroeconomist as Scientist and Engineer*. It deals with the history, methods and prospects of macroeconomics, i.e. with precisely the topics of the preceding sections. I felt that it would be instructive to contrast my critical evaluations with the current position of a consummate insider.

Mankiw begins his paper as follows:

Economists like to strike the pose of a scientist. I know, because I often do it myself. When I teach undergraduates, I very consciously describe the field of economics as a science, so no student would start the course thinking he was embarking on some squishy academic endeavor. Our colleagues in the physics department across campus may find it amusing that we view them as close cousins, but we are quick to remind anyone who will listen that economists formulate theories with mathematical precision, collect huge data sets on individual and aggregate behavior, and exploit the most sophisticated statistical techniques to reach empirical judgments that are free of bias and ideology (or so we like to think).

The paragraph is a naïve expression of the ideology of scientism. Economists use mathematics and analyze a lot of data, using sophisticated statistical tools; therefore they are doing science! The fact that the statement is attenuated with some self-deprecating irony does not alter the basic message that is conveyed throughout the paper.

A basic message transported by the paper is that Keynes, Keynesians and later new Keynesians were ‘engineers’. This is contrasted with the new classical movement, defined by Mankiw to include Friedman type monetarism, Lucas type rational expectations and real business cycle theory. The neoclassicals according to Mankiw are doing ‘science’. His definition of the two styles is brief:

Engineers are, first and foremost, problem solvers. By contrast, the goal of scientists is to understand how the world works.

This definition would be objected to by both genuine engineers and genuine scientists. Can an engineer solve problems without an understanding of the relevant aspects of how the world works? In the philosophy of science the initial impetus that motivates a scientific inquiry is referred to as a ‘problem situation’. Some aspect of the world has been observed, but the

observations have not been understood.¹ True, new classical macroeconomics has very largely evolved not in relation to observed empirical regularities that are asking for an explanation; as outlined in the previous sections, it has evolved under the impact of ideological forces. That this is so is a sign, not of science, but of pathology. The claim that Keynes was an engineer, while the new classicals are scientists is plain silly. Keynes was an empiricist who attempted to learn about the world by observing it. As Mankiw himself states in his paper, it is Keynesian theory rather than that of the new classicals that is used in all contexts where the real economy has to be dealt with. The national accounts are a further lasting monument to Keynes' empiricism.

Mankiw's history of macroeconomics, in a nutshell, can be described as follows: Keynesianism was followed on the one hand by the "three waves" of neoclassical theorizing: monetarism, rational expectations and real business cycles. The common assumptions are rational forward looking behavior of agents, flexible prices and instantly clearing markets. In opposition to this were the new Keynesians. While also committed to the assumption of rational behavior, they furnished justifications for the assumption of sticky prices and non-clearing markets. The antagonism that prevailed between these competing movements is described by Mankiw under the heading 'Digression and Vitriol'. Mankiw thinks that the antagonism that prevailed between the two sides was one reason why many students in the Nineties chose the field of economic growth rather than fluctuations as their specialty.

Mankiw's assessment of what has been accomplished is modest:

New classical and new Keynesian research has had little impact on practical macroeconomists who are charged with the messy task of conducting actual monetary and fiscal policy. It has also had little impact on what teachers tell future voters about macroeconomic policy when they enter the undergraduate classroom. From the standpoint of macroeconomic engineering, the work of the past several decades looks like an unfortunate wrong turn.

Now, in Mankiw's view, the dichotomy has been overcome by a 'new synthesis' regarding which he says little beyond stating that he expects it to be the wave of the future. A rich harvest promised in the future to compensate for the current drought is a common feature of ideologically motivated research movements. A new research program to challenge the new classicals, or the new consensus, is in fact well under way. It is based on the assumption of 'heterogeneous agents' which does away with the new classicals' indefensible assumption of representative agents. The fashion show of ideologies in macroeconomics will continue.

8. THE COLD WAR ORIGIN OF RATIONAL CHOICE

8.1 *The Rand Corporation and the Cold War*

This section is in large measure based on Amadae (2003). She describes efforts at the Rand Corporation in the post World War Two era to develop a counter ideology to communism and other forms of collectivism. These efforts, later supported by the Ford Foundation, transformed political science and much of the other social sciences as well. This is described in Part I of her book. She refers to the entire movement that resulted for these efforts as the 'rational choice movement', or as in the title of her book, 'rational choice liberalism'. The entire movement is divided into three parts, corresponding to newly established academic disciplines; each with its canonical text. These are: Social choice with the canonical text Kenneth Arrow (1951), *Social Choice and Individual Values*. Public choice with the canonical text James M. Buchanan and Gordon Tullock (1962), *The Calculus of Consent*. Positive political theory with the canonical text William H. Riker (1963), *The Theory of Political Coalitions*. Further canonical texts mentioned by Amadae are: John von Neumann and Oskar Morgenstern (1944), *The Theory of Games and Economic Behavior*; Anthony

¹ This is discussed at length in Ravetz (1971, Chapter 4).

Downs (1957), *An Economic Theory of Democracy*; Mancur Olson (1965), *The Logic of Collective Action*.

In the following I will concentrate on two topics. The first is the genesis, in the Cold War context, of the rational choice approach at the Rand Corporation, the Pentagon and the Ford Foundation. The second is Arrow's work on social choice, specifically his 'impossibility theorem'. Both topics allow an exemplary analysis of the role of ideology. Furthermore, for the subsequent developments in the political and social sciences none can approach the fame and influence attached to Arrow's work. First I will cite Amadae's assessment of the importance of the rational choice movement:

The history of the development and establishment of policy science as an institutionalized and disciplinary norm has all the drama of a Hollywood screenplay involving the missile gap, the Sputniks, John F. Kennedy's presidential election, the overhaul of decision making procedures throughout Robert S. McNamara's Department of Defense, and the introduction of these same policy tools into domestic politics in Lyndon B. Johnson's Great Society program. This set of events gathered a momentum of its own but had crucial intersections with rational choice theory's development in the academy. Two vignettes illustrate the extent of these interconnections. Thomas Schelling, one of the key figures in establishing rational choice theory as mainstay approach to international relations, was one of RAND's alumni who formed McNamara's team of defense analysts in the Pentagon. In a pivotal series of U.S. Senate hearings, Schelling testified on the behalf of the administration about the development and use of rational policy tools throughout the U.S. Department of Defense in 1968. Subsequently he and other RAND alumni, including Howard Raiffa, helped to establish rational choice theory as part of the mainstream American intellectual endowment by virtue of their prominent academic posts at Harvard University's professional school of business. The rational policy analysis approach initiated at RAND would become central to the shift during the 1970s away from the outmoded "public administration" paradigm to one of "public policy"... (p. 10).

The rising tide of academic rational choice theory is confluent with the institutional prestige and weight of established practice achieved by practitioners of the new decision sciences. The routinization of social practices around abstract decision tools gave these tools a de facto legitimacy: as Schelling testified, no one knew if the decision tools actually achieved better decisions, but the tools had theoretic legitimacy insofar as they claimed to be based on scientific analysis, and had practical legitimacy sheerly as a consequence of their use. This tendency of the new rational policy tools to attain stature through their widespread use can also be seen in McNamara's eventual departure from the Pentagon and rapid installation as the president of the World Bank, pioneering yet another venue, this time international, for rational policy tools.

It is no exaggeration to say that virtually all the roads to rational choice theory lead from RAND. This observation draws attention to its role as a quintessential American Cold War institution, and in turn to the Cold War motives that underlay much of the impetus propagating rational choice theory. While RAND was one of several foci for the development of decision theory – along with the Chicago-based Cowles Commission, the Public Choice Society, and the University of Rochester's political science department – its members and alumni played an especially pivotal role in making rational choice part of the established American institutional endowment. In the late 1940s and 1950s, there was active sharing of resources and ideas between RAND and the Cowles Commission; in the 1960s many rational choice pioneers from all walks of the academy participated in Public Choice Society meetings; also in the 1960s, University of Rochester political science students benefited from summer workshops on game theory held at RAND. (pp. 10-11).

The significance of the rational choice approach in American academic political and social science is summarized by Amadae as follows:

The three chapters that address Arrow's social choice theory, Buchanan and Tullock's public choice theory, and Riker's positive political theory explore the significance of the rational choice approach to politics for grounding American capitalist democracy. In its guise as "objective" or "value free" social science, it is difficult to appreciate the full import of social choice, public choice, and positive political theory for reconceptualizing the basic building blocks of political liberalism. In light of the Cold War ideological struggle against the Soviets, this enterprise of securing the philosophical basis of free world institutions was critical. Not surprisingly, Arrow, Buchanan, Tullock, and Riker pitched their newly forged social scientific method as part of the campaign to reassert the tenets of governmental rule legitimized by popular consent, but not susceptible to fascist or authoritarian perversions. The preeminent status attained by rational choice theory throughout American academia is indistinguishable from the vital Cold War activity of reconstituting democratic theory in the wake of Hitler's and Stalin's transgressions against humanity. Appraising the significance of social science methodology for American liberalism

requires understanding the nuanced manner in which it refashioned the theoretical cogency and institutional integrity of democracy. Democratic theory itself required revision because its prior reliance on “public opinion”, “popular sovereignty”, or a “general will” had been severely challenged. Rational choice scholars simultaneously rebuilt the theoretical foundations of American capitalist democracy and defeated idealist, collectivist, and authoritarian social theories. Now that it is apparent that capitalism and democracy have prevailed over Soviet communism and Marxism, it is worth knowing the precise series of arguments through which such a philosophical victory, as important as a military victory, was won.

I agree fully with Amadae’s description of the institutional success of the rational choice approach. I differ sharply with regard to her exalted view of its intellectual achievements. On this I believe that she has been a victim of the ideology. This will be elaborated below.

8.2 Rand and the Pentagon

The origins of the Rand Corporation reach back to rather humble applications of what today would be called operations research during World War II. After the war, in the context of the developing Cold War, Rand developed a much more ambitious vision: to make all aspects of the defense effort ‘scientific’, which in essence meant to make the required decisions subject to mathematical, mainly decision theoretic, analysis. The total effort was to have two grand subdivisions. One was the use of the mathematical theory of games developed by von Neumann and Morgenstern in order to gain an edge relative to the Soviet Union by means of an intellectually superior approach to the formulation of strategy. The other was the cost/benefit analysis of military procurement decisions. Amadae discusses these in considerable detail. I discuss here only the two most important instances of the Rand approach. Their importance derives from the fact that they resulted in the transfer of vast sums to the defense effort and in the centralization of decision making away from the military to the systems analysts at the Pentagon.

The first episode concerns the famous (better infamous) ‘missile gap’. I cite Amadae’s introduction to this subject:

Soon after taking office, Secretary of Defense Robert S. McNamara held his first press conference. It was to be one of the shortest on record, consisting of only one question. A reporter asked, “Mr. Secretary, you’ve been here three or four weeks. The missile gap obviously was an important element in the campaign and a major security issue. What are you doing about it?” McNamara replied, “Well, you’re quite right, it was important [and] it is important. I focused on that, and I’ve determined there wasn’t a missile gap, or if there is, it’s in our favor.” In McNamara’s own words, reporters “broke the doors down” running to call their editors. The next day’s headlines screamed, “McNamara Denies Missile Gap” and the Republican Senate majority leader called for his resignation. Despite this complete reversal of official U.S. government position and the resulting public outcry, every policy idea based on the belief in the (nonexistent) Soviet missile advantage was implemented over the next seven years. This chapter explores the processes of knowledge production and political interaction that manufactured the “gap” in the public mind and public record, initiated a sea change in American national security policy whose rationale originated in the missile gap, and empowered a new policy elite whose authority was grounded in the supposed objectivity of rational policy analysis.

In the case of the ‘missile gap’, game theory was used to scare the public and the political elites into allocating unprecedented sums to the defense effort. The next episode involves the planning-programming-budgeting-system (PPBS) developed at the RAND corporation and installed at the Pentagon by Secretary McNamara. The proponents of PPBS

...were agreed on inverting the policy process: instead of fiscal appropriations being handed down from Congress to meet operational needs, defense planners would articulate their needs using presumptively objective and thus incontrovertible cost-effectiveness studies. Instead of Congress’ determining how much national security the nation could afford, national defense imperatives should determine defense allocations on the principle that “there [be] no presumption that the defense budget is now, or should be, near any immovable upper limit”. Hitch and McKean further authoritatively observed that “[a]s far as physical and economic feasibility is concerned, national security expenditures could be raised. by, say, \$30 billion per year.”

The authority for this bold restructuring came from the supposed scientific rigor promised by such a budgetary process. (p.63).

The breakthrough for the establishment of PPBS at the pentagon came at the beginning of McNamara's tenure with a huge procurement project to supply the air force and the navy with new fighter planes. The military desired two different planes designed for the differing mission requirements of the two services. I quote Amadae:

Whereas it is easy to be distracted with the appearance that McNamara and the defense rationalists where proponents of *civilian* control over the U.S. armed forces, it is necessary to recall that arguments for such authority are based on the premise that legitimate military authority be granted to serve the ends of representative government and to uphold the Constitution. The new policy elite were altering the rules such that authority over military procurement, strategy, and operations would be in the hands of "objective" policy analysts, removed from democratic politics.

McNamara initiated the most dramatic and forceful showdown with military leaders within his first months of taking office. He launched his campaign for greater efficiency in the military with his proposal to build one kind of tactical fighter to satisfy divergent Air Force and Navy specifications, promising that this move to "commonality" would save the nation \$1 billion. Military commanders were aghast when McNamara, who had no engineering training or mechanical background, overruled expert engineering judgment and concluded that having one aircraft design serve two conflicting functions was technically feasible and pragmatically wise. (p. 65).

McNamara prevailed over the strenuous objections of the generals and the contract for the combined plane was awarded to General Dynamics in Fort Worth. Extensive congressional hearings on this decision were subsequently held. They revealed that the sophisticated 'scientific' analysis on which the decision allegedly rested had never been performed.

Ultimately the investigating committee was led to conclude, as McNamara himself admitted, that the TFX decision rested on "rough judgments". In 1963 Congressmen could only suspect what history would confirm. The Navy later canceled its contract with General Dynamics in 1968, and the Air Force ultimately obtained only six hundred of the originally contracted twenty-four hundred planes, at a cost of \$22 million each instead of the initially proposed \$2.8 million. (p. 67).

It is not far from the truth that the claims of scientific objectivity that were used to propagate the Rand methodologies of game theory and PPBS were a hoax, used to provide a respectable cloak to what were actually naked power grabs.

Game theory was ultimately abandoned as a tool for the formulation of American foreign and military policy. Not so PPBS. In August of 1965, President Lyndon B. Johnson mandated that PPBS became standard operating procedure for all federal agencies.

8.3 Rand and the Neocons: A Postscript

This section has been added later than the rest of the paper. The preceding deals with the military role of the Rand Corporation to the extent that it was discussed by Amadae. After completing the paper certain thoughts kept going through my mind. For one, Amadae has almost nothing on the Vietnam War, after all the most dramatic and consequential event in which McNamara was involved. Moreover, the Vietnam war was shaped by the same patterns of thought that characterized McNamara's actions at the Pentagon from the beginning: a faith in advanced military technology coupled with contempt for the views of the military services. I was also struck by the parallel between the Vietnam War and the current Iraq War. The attitudes just attributed to McNamara are the same as those characterizing the secretary of defense managing the Iraq War, Donald Rumsfeld. The consequence in each case has been a disastrously mismanaged war, resulting ultimately in the ouster of the secretary of defense and the search for an exit strategy.

I decided to do some more research in the internet and I chanced on the article by Husain (2003) which traces the role of key individual associated with Rand up to and including the current presidency of George W. Bush. The following owes much to that article from which the quotations are also taken.

Two influential Cold War strategists at Rand were Albert Wohlstetter and Andrew Marshall. Wohlstetter's principal contribution was the doctrine of nuclear deterrence that became the key concept of America's defense strategy vis-à-vis the Soviet Union. The

essence of the doctrine was that effective deterrence required an assured counterstrike capability after a first strike carried out by the Soviet Union.

...looking ahead to anticipate future threats was a central part of Wohlstetter's methodology. In an essay published in 1959 Wohlstetter argued for significantly boosting of America's nuclear umbrella, in number and in its capacity to resist a first strike, in anticipation of Soviet moves to deploy more missiles with greater accuracies in the next ten years. To the imperatives of nuclear deterrence, he went on to recommend a large conventional force capable of fighting a general war against the USSR alongside a full blown nuclear conflict...

Wohlstetter also projected that in the 1960s, the American deterrent would have to deter not just the USSR, but China as well. Grimly pointing out that the Russians suffered 20 million dead in World War II and still emerged as a superpower, he wondered how much damage they would consider as "unacceptable"...All this meant that there would be no rest for the military in the 1960s according to Wohlstetter.

Wohlstetter left Rand in 1962 to enter academia and in 1964 took a professorship in the political science department at the University of Chicago.

It was here that he met a bright young student doing his dissertation in the Political Science department. His name was Paul Wolfowitz. Wolfowitz was drawn to Wohlstetter's intellect and temperament and began working under his supervision to carry his ideas further.

Wolfowitz became interested in strategic questions in the nuclear age and picked up where Wohlstetter left off. Where Wohlstetter had warned of preparing for a rearmed USSR and a nuclear China, Wolfowitz considered the third dimension along which nuclear strategy would evolve in the future: nuclear proliferation.

Wolfowitz took a job at the Pentagon and began to make alarmist projections of future Soviet nuclear capabilities. These were initially ignored. when George Bush Sr. became CIA director in 1976 he requested an assessment of Soviet nuclear capabilities. There resulted, with the active participation of Wolfowitz, the so called 'Team B reports'.

The contents of the Team B reports are alarming for the threats they saw in the Soviet nuclear build up, and startling for the methodologies they used. They projected that by 1984, the USSR would deploy about 500 Backfire strategic bombers, whereas the real figure in 1984 was less the half that. They claimed that the Soviet Union was working on an anti acoustic submarine, and failing to find any evidence of one, stated quite seriously that one may already be deployed since it appears to have evaded detection!

...

The Reagan defense buildup of the 1980s and the evil empire rhetoric of the second cold war built on the work of Team B members. The result was the largest defense budget increases in peacetime history.

I turn to the role of the other influential Rand strategist Andrew Marshall. He is described as a secretive individual about whose 23 years at Rand little is known.

There is very little to tell us about Marshall's work at RAND since hardly any of it has been declassified. In 1972, his friend and fellow RAND researcher, James Schlesinger who was serving as Secretary of Defense in the Nixon administration, created a little office in the Department of Defense titled the Office of Net Assessments (ONA), and made Marshall the Director. The ONA had a murky brief. Marshall's job was to imagine every kind of threat the US military might ever face...For 30 years Marshall has directed the ONA, and built for himself a formidable reputation and an equally formidable network of protégés in and out of government.

...

His best known protégé is probably Donald Rumsfeld, whose association with Marshall is decades old, dating from Rumsfeld's early days in the Pentagon. Rumsfeld became an early proponent of ballistic missile defense, a Marshall idea and belonged to that clique of hawkish policy makers who were opposed to Kissinger's ideas of detente and engagement with China.

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Marshall developed the concept of a 'Revolution in Military Affairs' (RMA) involving novel technologies, novel strategies and novel management methods.

When the Bush administration came to power, the RMA was put into practice. Rumsfeld was made the Secretary of Defense...and empowered Andrew Marshall to conduct a sweeping review of the military and make recommendations to make the military into a 21st century fighting force. The RMA was no longer part of the lunatic fringe from where it had originated. Its adherents were now in control, and were going to make their presence felt.

...

The ideas of Marshall and Wohlstetter drive the foreign policy of the Bush administration. The doctrine of pre-emptive action only takes Wohlstetter's logic behind the second strike capability to its logical

conclusion in a world where those who possess weapons of mass destruction may not be as easily deterred as the USSR was. And the war on terrorism has provided that environment of perpetual uncertainty in war that Marshall and his protégés have been thinking about for decades. As the superpower girds itself for a ruinous war in an uncertain part of the world, one is reminded of the hubris of power and the follies that led America into the Vietnam war. Today America is being steered into an endless war precisely by those who have been preparing for this sort of world all their lives. We shall soon see whether they know what they are doing.

The above was written in 2003. More recently, the Republicans lost both houses of congress in the 2006 mid-term elections; Rumsfeld has been forced out of office for mismanaging the Iraq war, just as McNamara had been forced out after mismanaging the Vietnam war. Marshall at 82 still heads ONA and recently published an apocalyptic scenario of nuclear war as a result of a climate catastrophe. Wohlstetter protégé Wolfowitz heads the World Bank.

There exists a complex web consisting of the various threads of the Neoconservative movement and of movements that have allied themselves with it. Given the enormous effect that the combined movement has had on the politics of the United States, and hence of the world, it is surprising how little scholarly attention has been focused on it. Moreover, information on one aspect of the phenomenon is usually not connected with other aspects. For example, the Wikipedia article on 'Neoconservatism' is focused on publicists, most prominently Irving Kristol, who is credited with inventing the term. A list of Neoconservative think-tanks does not include Rand.

The Neoconservative ideology rose to the apex of power under the administration of George W. Bush. A large part of the credit for bringing this about is usually given to the political operative and later presidential advisor Carl Rove. The various institutions and movements that Rove gathered under the umbrella of the Republican Party include the military industrial complex; other highly politicized business sectors, particularly energy, construction and finance; Christian Evangelicals; large segments of the mass-media that either espoused the neoconservative cause, or else muted their criticism; a variety of neoconservative think-tanks

It is no exaggeration to say that the ultimate goal of this alliance is the creation of an American empire that dominates the world. This goal has been rarely publicized. The fullest discussion of it has been given by Chalmers Johnson (2004). Much of the book is devoted to the extensive system of American military bases that gird the globe. The following quote focuses on American plans to dominate space.

Even prior to the Afghan war, a group of right-wing "defense intellectuals" had started to advocate a comprehensive new strategy for global domination. Many had served in earlier Republican administrations and most of them were again given high appointive positions when George W. Bush became president. They focused on plans for the next decade or two in much the same way that Captain Alfred T. Mahan of the navy, Senator Henry Cabot Lodge, and Assistant Secretary of the Navy Theodore Roosevelt had emphasized sea power, Pacific bases, and a two-ocean navy at the end of the nineteenth century. Rarely taking the public into their confidence, the members of this new clique were masters of media manipulation, something they acknowledged they had "learned" as a result of bitter experience during the Vietnam War. The terrorist incidents of 2001, much like the sinking of the battleship Maine in 1898, gave a tremendous boost to their private agenda. It mobilized popular sentiment and patriotism behind military initiatives that might otherwise have elicited serious mainstream doubts and protests.

The determination to militarize outer space and dominate the globe from orbiting battle stations armed with an array of weapons includes high-energy lasers that could be directed toward any target on earth or against other nations' satellites. The Space Command's policy statement, "Vision for 2020," argues that "the globalization of the world economy, will continue, with a widening gulf between 'haves' and 'have-nots,'" and that the Pentagon's mission is therefore to "dominate the space dimension of military operations to protect U.S. interests and investments" in an increasingly dangerous and implicitly anti-American world. One crucial goal of policy should be "denying other countries access to space." (pp. 80-81).

The principal actors advancing this agenda, as named by Johnson, are the same ones mentioned earlier in this section.

9. ACADEMIC RATIONAL CHOICE

The Origins

A crucial role in the institutionalization of rational choice first in the government and later in academia was played by H. Rowan Gaither Jr. He had managed the transformation of RAND from a division of the Air Force to a private foundation. Seeking philanthropic aid for RAND he had met Henry Ford II. This had momentous consequences as described by Amadae:

Meanwhile, Gaither had so impressed Ford at their meeting that the auto manufacturer asked him also to produce a policy statement for the Ford Foundation. Thus, while Gaither oversaw the reorganization of RAND, he was additionally responsible for creating a report expressing the Ford Foundation's mission. This statement is telling of Gaither's vision of society as a technocracy governed by an objective elite, and was personally acknowledged by Henry Ford II to have been "the most important step in formulating the policies of the Foundation"... Most crucially, the statement articulated as a plan for philanthropic support specifically what RAND managed to achieve in the 1950s: the development of a professional corps who, due to their superior expertise, could guide the nation through challenging policy decisions. The report describes a society managed by an educated elite outside the public arena and suggests that it is the duty of philanthropies to support this elite.

A quintessential Cold War document, the Report for the Study of the Ford Foundation on Policy and Program drew attention to the current "world crisis" and to the seemingly black and white choice between democracy and totalitarianism. According to the foundation's report, a primary goal of philanthropy should be advising "those responsible for the formulation or execution of policy". Policy advice should come in the form of objective analysis or expert consultation. In bestowing charity, the goal is "to replace partisan controversy with objective fact". Unsurprisingly, according to the mission statement, none other than a philanthropy is most qualified to support nonpartisan research, because a philanthropy such as the Ford Foundation, which "has no stockholders and no constituents... [and] represents no private, political, or religious interests" is the height of neutral objectivity. The report is unequivocal in suggesting that philanthropies and their beneficiaries manifest an objectivity that best entitles them to provide leadership in a democratic society. (pp.35-36).

A third institution founded by Gaither with a grant from the Ford Foundation was the Center for the Advanced Study of the Behavioral Sciences (CASBS) at Stanford University. Amadae describes the Center's purpose and functioning:

H. Rowan Gaither Jr.'s steadfast support of the social sciences as tools for social management and rational defense management had a two-fold impact on the emergence of the rational choice framework. Both at RAND and through the Ford Foundation's establishment of the CASBS, theorists had the freedom to generate a body of ideas. Furthermore, the empowerment of the defense rationalists helped to gain currency for their ideas of rational and objective policy analysis. As these theorists found their way back to academia after stints of service in Washington, they returned with the prestige helpful to making their idea set part of the mainstream intellectual endowment of American society. (p. 79).

These passages illustrate a profound contradiction of the rational choice movement. Ostensibly it aims at providing an intellectual bulwark defending capitalist democracy against the temptations of totalitarian ideologies. But the architects of this bulwark were in an elitist rather than a democratic tradition. The idea of social control by scientific elites hardly differs from the communist ideology that they so arduously combated. The anti-democratic tendency of the Western intellectual tradition, going back to Plato, was a theme of Popper's *The Open Society and its Enemies*. B. F. Skinner, the American psychologist and leader of the behaviorist school, advocated social control by a scientific elite, as illustrated by his utopian novel *Walden Two*.

Regarding the anti-democratic tendencies of elites generally, Amadae has the following comment:

The history that emerges is relevant to broader discussions of the tension between the ideal of liberal democracy and the tendency of elites to develop means to control societal decision-making processes. Since its inception as a social form predating the French and American Revolutions, and going back to at least the British civil wars, the drama of democratization has in part been about conveying the appearance of inclusion while designing means to retain actual control over decisionmaking in the hands of a social elite. (p. 31).

Arrow and the Consequences

The subject matter of this section is one on which a huge, highly technical and mathematical literature exists. My own discussion will be entirely non-technical, it is however based on a rigorous paper (Hillinger, 2005) to which the interested reader may turn.

Arrow's 'general possibility theorem', popularly known as his 'impossibility theorem', or as 'Arrow's paradox', along with its reception and interpretation, is surely one of the strangest episodes in the annals of science. I will try to show that it is this reception and interpretation that deserves to be called 'paradoxical', rather than the theorem itself.

In discussing Arrow's work I will first take at face value the interpretation given by him that has been accepted essentially unchanged by the huge subsequent literature. According to this standard interpretation, Arrow has proven that there is no way of going from a set of individual preferences over some alternatives to a social ordering, given that we require the aggregation to satisfy a few very reasonable conditions. Arrow's theorem is usually interpreted as applying to voting. Since voting is the most important formal method for reaching decisions in a democracy, the theorem is taken to imply the impossibility of democracy. Arrow has stressed that his theorem is even more general than that since it applies to any method for aggregating preferences. Specifically, the theorem applies also to market outcomes; these cannot be rationally justified as being particularly desirable.

There is only one other specific analytical result in economics, or for that matter in all of social science, that has achieved equivalent fame. It is the argument given by Adam Smith to show that a competitive market economy is efficient. The market achieves efficiency as if guided by an 'invisible hand'.

But the annual revenue of every society is always precisely equal to the exchangeable value of the whole annual produce of its industry, or rather is precisely the same thing with that exchangeable value. As every individual, therefore, endeavors as much as he can both to employ his capital in the support of domestick industry, and so to direct that industry that its produce may be of the greatest value; every individual necessarily labours to render the annual revenue of the society as great as he can. He generally, indeed, neither intends to promote the publick interest, nor knows how much he is promoting it. By preferring the support of domestick to that of foreign industry, he intends only his own security; and by directing that industry in such a manner as its produce may be of the greatest value, he intends only his own gain, and he is in this, as in many other cases, led by an invisible hand to promote an end which was no part of his intention. (Smith, 1776 [1993], pp. 291-2).

This brief analytical passage could easily have been overlooked amidst the many, discursive pages of the *Wealth of Nations*; happily it was not and became the foundation of modern economics. Though verbal, the argument matches exactly the most elegant mathematical derivation of what now is called 'the first theorem of welfare economics'; that a competitive economy is Pareto efficient.¹

A large literature dealing with the 'Arrow paradox' appeared, but it is fair to say, did not find a way around it. For example, it was shown that if preferences satisfy the condition of being single peaked, than the paradox becomes inoperative; however, the condition cannot generally be expected to hold. In the following I will discuss a series of quite different paradoxes involved in the reception and interpretation of Arrow's paradox.

First Paradox:

As described by Amadae, the rational choice movement arose in the context of the Cold War with the purpose of countering totalitarian ideologies and providing the ideological foundation for capitalist democracy. For this purpose they focused on the idea of a 'general will'. Arrow (1963) discusses this issue in considerable detail in Chapter VII, particularly Section 3. I cannot summarize his position better than Amadae has already done:

In his effort to defeat what he takes to be the philosophical idealism of Rousseau, Kant, and Marx in the spirit of the "liberal heritage," Arrow refers to "the debate a few years back between Mr. Dobb and Professor Lerner. In raising doubts as to the reliability of individuals' tastes as a guide for sound public policy, Dobb had suggested that there be a superior standard for policy. Reviewing the debate at some

¹ Cf. Varian (1992, Section 17.6).

length, Arrow concludes that Lerner was just in his assessment that Dobb “implies some transcendental optimum other than that shown ‘by a free market or in any other way.’” In Arrow’s view, Dobb represents “the rationalist tradition common to utilitarianism and Marxism,” and comes up against the difficulty of erecting an absolute moral standard that transgresses liberalism and the sanctity of the individual to select his own ends and preferences. In grouping together Rousseau, Kant, and Marx as theorists looking to absolute philosophical ideals to enforce standards of behavior beyond the discretion of individuals’ subjective desires, Arrow argues that moral relativism is more consistent with liberalism. His theorem rejects the possibility that a social consensus on ends could emerge as a result of a philosophical ideal transcending individuals’ desires as a guide to collective decisionmaking. Since, as Arrow presents it, Marxism requires such an ideal, it is incompatible with the liberal tradition to which Social Choice and Individual Values contributes.

What is noteworthy in Arrow’s discussion of these three philosophers is the manner in which his set-theoretic proof undermines their philosophical systems: he insists on a thorough-going individualism that is incompatible with any standard for collective social norms or self-legislation that may impinge on an individual’s right to have any (transitive) set of desires, and he defines collective rationality in accordance with this priority granted to individual desire. Here Arrow’s philosophical position clearly reflects the attempt to erect a basis for American economic and political liberalism that cannot be thwarted by authoritarianism. (p. 113-114).

Both Arrow and his interpreters, such as Amadae, seem to be blind to the obvious paradox that Arrow, on his own terms, proved the impossibility of capitalist democracy. Totalitarian systems are based on the belief that their leaders are already in the possession of the relevant truth; they see no need for the aggregation of individual preferences. If they allow voting at all, it is only to affirm the pronouncements and actions of the leader.

Second Paradox:

There is an apparent contradiction between Smith’s theorem, in its modern form the first theorem of welfare economics, and Arrows. For the general case of unrestricted individual preferences, Arrow was unwilling to grant any rationality to either voting or the market. However, efficiency, which is what the first theorem of welfare economics attests competitive markets, is certainly a strong rationality property. The welfare theorem would otherwise not have been assigned the importance that it has. The contradiction is not at a logical level, since Arrow has a broader concept of rationality that requires *all* possible states to be included in a ranking; The rationality of the market is more restrictive since possible initial distributions of wealth are not evaluated. For a given distribution, the market is rational in producing the ‘best’ outcome in the Pareto sense. This is more than Arrow seems to be willing to grant.

Third Paradox: One would think that given Arrow’s proof of the impossibility of rationally aggregating preferences that the subject would cease receive the attention of scholars. On the occasion of Arrow’s receiving the Nobel Prize, Paul Samuelson (1972) had written:

What Kenneth Arrow proved once and for all is that there cannot possibly be found such an idea! voting scheme: The search of the great minds of recorded history for the perfect democracy; it turns out, is the search for a chimera, for a logical self-contradiction.

Why would anyone wish to chase the chimera that had defeated the great minds of the past? After all, given the proof that a square with the same area as a given circle cannot be constructed using compass and ruler alone, this problem ceased to occupy the attention of mathematicians. Similarly, given the impossibility of constructing a *perpetuum mobile*, engineers do not try to construct machines that run without energy. In the case of collective choice the experience has been the opposite; before Arrow the subject barely existed, after Arrow the literature veritably explodes. Dennis Mueller’s text *Public Choice III* has 768 pages with 64 large and closely printed pages of references, almost all post-Arrow.

The theory of collective choice has undeniably produced much printed paper: What else has it produced? I am tempted to say very little. The mathematical theory of voting began about 300 years ago with the work of Borda and Condorcet who realized the defects of plurality voting and began looking for a superior alternative. Plurality voting is still universally used and theorists do not agree on the superiority of any other method. McLean and Urken (1995), in their review of social choice theory reach the following conclusion.

... modern social choice theorists develop very general models and are consequently reluctant to give advice. To the nonspecialist, the obvious question thrown up by social choice is, What is the best electoral system? This is an urgent practical question all over the world, never more so than since the collapse of communism began in 1989. But whereas theoretical molecular biology has started to play a large role in curing bodily disease, theoretical social choice has played almost no role in curing constitutional disease. Social choice theorists have usually regarded “the earnest efforts of electoral reformers...with the same kind of amused contempt as mathematicians in the past reserved for claims by amateurs to have succeeded in squaring the circle (Barry 1986, 1). Barry continues:

A few years ago, at a conference on the theory of democracy, a group of five eminent social choice theorists were trying to decide which of several restaurants to dine at. Since each knew the preferences of the others and could immediately compute the outcome to be expected from any proposed procedure, it was impossible to find any agreement on a method of voting. (The impasse was in the end resolved by one of their number setting off in the direction of the restaurant he favored; after he had gone about thirty yards the others fell in behind). (1986, 1-2)

Since 1989, Czechoslovakia, Hungary, Poland, and the three ex-Soviet Baltic republics have all written new voting laws... Other new democracies, including those in the former Soviet Union, will shortly have to do the same. Even at the peak of the Enlightenment, only three countries (the United States in 1787; France in 1789, 1791, and 1793; and Poland in 1791) wrote constitutions containing voting laws. In two years we have had at least six; but as far as we know, not one of them consulted any social choice theorists. (In 1990 members of the Mongolian legislature asked the president of the Public Choice Society for advice on a new constitution; but we have reason to suspect that the advice he gave them may have been along the lines lampooned by Barry.) It adds up to a lamentable failure of social science. Too much research in social choice has been conducted in the spirit of the pure mathematician’s prayer: “May it never be of any use to anybody!”

These negative evaluations are what Arrow’s theorem would lead one to expect. The motivation for producing this literature does appear paradoxical.

Fourth Paradox

The proof of the impossibility of democracy appears to be one of the most momentous discoveries of all time, its potential impact on the human race perhaps greater than the heliocentric hypothesis of Copernicus and Galileo, Newton’s theory of universal gravitation, or Einstein’s relativity theory. Children in school learn these laws of the natural world, but they and their elders remain ignorant of Arrow’s law concerning the social world. Countries that have traditionally thought of themselves as democracies continue to think this way. The United States keeps pursuing its agenda of spreading democracy; sometimes with apparent success, as in post-WWII Europe, at other times with disastrous consequences as presently in Iraq. No one associates these developments with Arrow’s theorem.

The Positive Theory of Social Choice

I begin this section by stating the

Fifth Paradox:

Barely after the appearance of Arrow’s impossibility theorem, Fleming (1952) and Harsanyi (1953, 1955) published what may be called possibility theorems on collective choice in the form of plausible and non-contradictory sets of axioms. Arrow assumed that preferences are to be expressed as weak orderings of the form $a \geq b \geq c$ meaning that a is not worse than b and b is not worse than c. The axioms proposed by Fleming and Harsanyi imply a cardinal representation of the form $u(a) = 10, u(b) = 2, u(c) = 1$ which would suggest in the present example that the gain in going from b to a is greater than the gain in going from c to b. The results of these three authors show that there is an open door to social choice, based on a cardinal representation, and another one, based on the ordinal representation, that is closed. One would have expected choice theorists to pass through the open door; they chose instead to bang their heads against the closed one. In my paper on voting (Hillinger 2005b) I went through the open door.

FORMELABSCHNITT 1010. ON THE POSSIBILITY OF SOCIAL SCIENCE: I. THE QUANTITY THEORY

10.1 Background and History

For the purpose of this section I define science as a social enterprise that is able to secure agreement on aspects of reality on the basis of publicly available and publicly scrutinized evidence. This definition accords with that given by Ravetz, cited earlier: an immature science is one that has not agreed on criteria for determining factual truth. This definition corresponds to the social system of science that has been attained in the natural sciences and is the basis of the prestige that they enjoy; it is also the motivation for imitation-science that attempts to share this prestige.

I do not believe that science by this narrow definition exhaust meaningful thought either in the social realm or elsewhere. This will be the subject of the next section. Nevertheless, science by the narrow definition is immensely powerful and also exerts its influence on other modes of thought. Also, immense efforts at being scientific, or at least appearing so, have been made in economics and other social ‘sciences’; with little apparent success. There also exists a substantial literature on the methodology of economics that is concerned with this issue, but has been unable to reach a consensual verdict. For all these reasons, the question of the possibility of science in the social realm remains open and important.

I deal with the issue pragmatically by pointing out that there have been at least two fields that fully meet the narrow criterion for science. They are the quantity theory of money and the newer research on happiness. Examination of these fields also reveals the difficulties that scientific findings in economic and social thought have in spreading beyond a narrow circle of experts.

The first formulation of a quantitative macroeconomic relationship is due to Copernicus, better known for his advocacy of the heliocentric theory. In a statement to the Polish king, based on an address to the Prussian diet in 1522, he writes: “Money usually depreciates when it becomes too abundant.” Regarding this earliest rough formulation of the quantity theory of money, the economic historian Spiegel (1971) wrote:

Copernicus’s tract was not published until the nineteenth century and may not have had much influence on the thought of his contemporaries. In any event, his discovery, whatever its range and effect may have been, is especially remarkable because chronologically it antedates the large-scale movement of precious metals from America to Europe. By the power of reasoning and by the ability to invent fruitful hypotheses, a great mind may discover relations that ordinary people can recognize only if driven by the stimulus of observation. (p. 88).¹

The subsequent histories of the heliocentric hypothesis and the quantity theory of money are highly instructive in illustrating the difference with regard to the establishment of factual truth between the natural and the social sciences. The heliocentric hypothesis was long forbidden by the Church, but as the prestige of science increased and that of the Church declined, the hypothesis became accepted by all educated people including the hierarchy of the Church. Once the hypothesis was broadly accepted, it was never again seriously brought into question.

The quantity theory of money had a very different fate. It did not face the same kind of opposition as the heliocentric hypothesis, instead it was for some centuries simply ignored. The incorporation of the hypothesis into classical economics was based on abstract economic reasoning rather than on systematic empirical evidence. During periods of high inflation, people tended to become aware of the relationship between money and prices, but when the memory of inflation faded so did this understanding. Keynesian economics emphasized the various ‘pathologies’ that might prevent a change in the quantity of money from having any effect.

¹ An excellent short history of the heliocentric hypothesis is given by Shakman (1989):

10.2 Modern Work

Serious empirical work on the quantity theory began at Chicago as part of the monetarist attack on Keynesianism.¹ This was around 1950, some 400 years after the first formulation of the hypothesis by Copernicus! The Chicago studies provided strong evidence in favor of the quantity theory.

Largely inspired by Chicago, empirical studies of major inflations, which were at the same time test of the quantity theory, attracted a considerable number of scholars in following decades. Capie (1991) assembled 21 such studies. Together they provide the most impressive validation of the quantity theory that is available. In the following paragraph I will try to justify this statement.

I have defined 'science' as "a social enterprise that is able to secure agreement on aspects of reality on the basis of publicly available and publicly scrutinized evidence". Crucially important in this context is the greatest possible diversity of both investigators and evidence. If a hypothesis is confirmed only by one set of cooperating investigators working on a narrowly defined set of data, then the confidence in the validity of the hypothesis will not be great. The criteria of diversity for investigators and evidence are extremely well met by the Capie collection. The investigators come from a number of different countries and their articles were published over a span of four decades – the earliest being Bresciani-Turoni in 1937 and the last Capie in 1986. The inflations studied are highly diverse in time and location. Following the introductory Part I, Part II has 10 articles on inflations before 1900; Part III has 6 articles on inflations in the 1920s; Part IV has 5 articles on inflations in the 1940s. The most impressive example for the diversity of evidence is the article by Francis T. Lui 'Cagan's Hypothesis and the First Nationwide Inflation of Paper Money in World History'. It deals with the introduction of paper money and subsequent inflation in 12th Century China.

Two more publications are relevant. One is Fischer, Sahay and Végh (2002). They do a cross-sectional study of the inflationary experience of 133 countries since 1957. From their conclusions I quote the statement that bears on the quantity theory: "As expected, the long-run (cross-section) relationship between money growth and inflation is very strong".

The final paper to be discussed, Dwyer and Hafer (1988), is my favorite empirical investigation of the quantity theory. It illustrates a common sense approach to doing empirical science, untouched by self-defeating econometric fashions. I quote their introductory paragraph because it illustrates the inability of the economic profession to come to an agreement regarding the evidence on an empirical phenomenon.

Many economists recently have been claiming that money has little or no effect on inflation and economic activity. For example, Lyle E. Gramley, past governor of the Federal Reserve Board, has been quoted as saying "the relationship between growth of the economy and the growth of the money supply is just no longer there." Meanwhile, even a noted monetarist such as Beryl W. Sprinkel, the current chairman of the Council of Economic Advisers, says: "It's a problem. Nobody knows where we are going."

These recent statements are hardly novel, nor have they changed all that much over the years. In 1971, Federal Reserve Board Governor Andrew F. Brimmer noted that it has "not [been] demonstrated convincingly that the relationship between the money supply and economic activity is especially close."

Two decades and innumerable empirical studies later, their statement applies just as well to current macroeconomics.

Dwyer and Hafer discuss the quantity theory in terms of the two equations

$$(10.1) \quad \dot{M} = k + \dot{Y},$$

$$(10.2) \quad \dot{Y} = \dot{p} + \dot{y}.$$

where the dots indicate growth rates, M is the money stock, k a proportionality constant, Y nominal national income, p the price level, y real national income. For their investigation they use data on 62 countries for the period 1979-1984. Since the quantity theory has been

¹ Cf. Friedman (1969).

generally interpreted as an equilibrium relationship they use 5 year averages for the main part of their study and do a cross-sectional analysis with these. In such a cross-section no systematic change in k can be expected. Their key equation is

$$\dot{Y} = 1.592(1.128) + 1.007(0.027)\dot{M}, \quad R^2 = 0.96.$$

The coefficient of \dot{M} is highly significant and almost exactly unity, as the quantity theory predicts for a constant k . The estimated constant term is of low significance and a graphic plot shows that a line through the origin fits the data very well.

The variation in inflation rates between the countries is equally well explained by the variation in the growth of the money stock:

$$\dot{p} = -1.354(1.055) + 1.031(0.025)\dot{M}, \quad R^2 = 0.96.$$

Finally, there is no long-run relationship between the growth rates of the money stock and real income:

$$\dot{y} = 2.613(0.366) - 0.018(0.009)\dot{M}, \quad R^2 = 0.07.$$

All in all an impressive validation of the quantity theory, using only elementary techniques.

10.3 Conclusions

My conclusion from this survey is that the validity of the quantity theory has been established beyond the shadow of a doubt, using the most rigid scientific criteria. What has been the consequence? My conjecture is that few contemporary economists are aware of the cited evidence and few would express any confidence in the quantity theory. I have no direct evidence for this statement, but I do have some indirect evidence.

The most recent advanced macroeconomics text in my library is Birch and Sørensen (2005). The subtitle of the book 'Growth and Business Cycles' already indicates a lack of interest in the money/inflation nexus. There is no explicit mention of the quantity theory, but the relationship does appear in their Figure 3.3: Money growth and inflation, Denmark and the US, 1870-2000. Strangely, this is in Chapter 3: 'Capital Accumulation and Growth' and in a section titled 'Money?'. The principal argument of this section is that money has no effect on output in the long run. The purpose of the figure is apparently to demonstrate that the effect of money growth is exhausted by inflation.¹ The section concludes: "This should explain why you will hear no more of money in Book One of this text, while monetary policy will be at the heart of the analysis throughout Book Two". Book Two is titled: The Short Run: Economic Fluctuations, Short-run Unemployment and Stabilization Policy. Much of it is devoted to the Phillips curve and related issues. The student of this text will never have heard of the quantity theory and the empirical relationship that it denotes will hardly have made an impression on him.

Further evidence that the quantity theory has faded from economists' awareness is furnished by the monetary policies pursued by central banks. They are universally pursuing active discretionary policies. Generally, the key consideration is some form of inflation targeting; monetary policy is related to an inflation forecast. Secondly, an estimate of the stage of the business cycle also plays a role. For most industrialized countries, the growth rate of the money stock has been well above that suggested by the quantity theory as appropriate to secure the desired low inflation rate in the long run. Currently inflation is accelerating world wide and the central banks are stepping on the brake by raising their discount rates. Their focus on inflation targeting has evidently led them away from being guided by the quantity theory.

¹ They do not substantiate this inference. It is based on equations 9.1,2 above, which they do not discuss, with the additional assumption that k is constant.

9.4 Evidence and Ideology in Relation to the Quantity Theory

The natural sciences are characterized by the steady accretion of knowledge. Knowledge, once firmly established, may subsequently be further refined; rarely is it refuted or simply forgotten. Why is this progress not visible in the social sciences? In general terms I will deal with this question in the final section. Here I examine it in relation to the quantity theory.

The history of the quantity theory, as well as of macroeconomics generally, is one of the increasing dominance of ideology over empirical science. The early work done at Chicago to rehabilitate the quantity theory was pure empirical science. The motivation for this work was however partly ideological: to discredit Keynesianism with its interventionist bias and thus to promote the neoliberal ideology. To advance this agenda further, Chicago monetarism went beyond the quantity theory and asserted that short-run fluctuations of output had their origin in shocks originating in the monetary sector. This aspect of monetarism steered empirical work away from the quantity theory towards the investigation of the short-run effects of monetary changes on output.

A related development was the discrediting of structural macro-econometric modeling, at least in academic economics. The large-scale macro-econometric models moved to governmental agencies or private research institutes. For this there were essentially three causes. The major one was the unsatisfactory nature of the large-scale models, both in their conceptualization and in their performance. A second element was the rising interest in time series analysis, strongly influenced by the work of C. W. J. Granger. A third factor was the aforementioned development of monetarism; the assumed unicausal influence from money to the macro-economy called for time series analysis rather than multi-equation structural modeling.

Granger's methodology emphasized statistical testing which was in line with the general tendency of econometrics. An important role in the present context was played by the 'causality test' developed by Granger. There was an explosion of papers testing for causality from money on macroeconomic variables. Ultimately, this research failed to identify any reliable relationships involving money and served to undermine the belief in the existence of *any* such relationship.

Another major reason for the eclipse of the quantity theory is what may be called the professional ideology of central banks. The implication of the quantity theory for monetary policy, strongly advocated by Friedman, is to let the money supply grow at a constant rate. This would eliminate discretionary monetary policy and drastically reduce the importance of central banks. I read a newspaper interview with Milton Friedman in which he was asked about monetary targeting, he replied that it was a make work program for central bank professional staffs.

This history is an interesting example of the interplay of reality and ideology. Friedman's proposal of a constant monetary growth rate was based both on his scientific work on the quantity theory of money and his ideological preference for governmental non-intervention. It was defeated by the professional, self-interest motivated, ideology of the central banks.

11. ON THE POSSIBILITY OF SOCIAL SCIENCE II: HAPPINESS RESEARCH

Since the 1980s there has been a burgeoning interdisciplinary literature on the determinants of individual wellbeing, or happiness. While originally conducted mainly by psychologists, this research has increasingly also attracted the attention of economists. Two recent books by economist, Frey and Stutzer (2002) and Layard (2005) summarize the findings. I discuss this research here for the following reasons: First, it is another demonstration of the possibility of genuine science in a social field. It also demonstrates a crucial difference to natural science, namely, that we cannot expect in the social sciences to discover anything that is startlingly new. This raises profound, and in my view unsolved, problems in assigning a meaningful role

to the social sciences. The expectation of the continuous generation of novelty, adopted from the natural sciences, has in the social realm led mainly to pseudo science.

The evidence summarized in the above surveys cannot be repeated here. I limit myself to a brief characterization. The amount of evidence is by now massive; it comes from many studies on the populations of many different countries. The dominant methodology is that of survey questionnaires. This methodology is well established and has a sophisticated methodological literature. The results have also been corroborated by other means, particularly neurobiological measurements.

The most important negative finding: beyond a certain level of material well being further increases of consumption do not add to happiness. The advanced industrialized countries have already moved beyond this level decades ago. This finding is an indictment of contemporary, advertisement driven, culture and the associated political-economic system with its neoliberal ideology. How is it possible to have a culture that is so unreal in its basic premise? The answer is that people are often poor judges of the determinants of their own happiness (Frey and Stutzer 2002, p.13-14). This connects back with Section 3 on the enlightenment conception of man.

What then determines happiness? Frey and Stutzer list the following types of determinants:

- a) *Personality factors*, such as self-esteem, personal control, optimism, extraversion, and neuroticism.
- b) *Socio-demographic factors*, such as age, gender, marital status, and education.
- c) *Economic factors*, such as individual and aggregate income, unemployment, and inflation.
- d) *Contextual and situational factors*, such as particular employment and working conditions, the stress involved at the workplace, interpersonal relations with work colleagues, relatives and friends, and most importantly – the marriage partner as well as living conditions and health.
- e) *Institutional factors*, such as the extent of political decentralization and citizens' direct political participation rights.

To the reader desiring to learn more about the causes of happiness I strongly recommend these two fine surveys. In the remainder of this section, as well as in the Conclusions, I deal with the implications for social science and social thought.

As a starting point I note that the findings of happiness research are not startlingly new; as Layard emphasizes in his introduction, they are in line with the teachings of the great religions and of modern psychology. The novelty is mainly in adding detail and precision and in firming the results with copious quantitative evidence. What will be the likely impact of this research on specific disciplines, particularly economics? I am pessimistic that score. The central message that emerges from this paper is that the social sciences have been ideology driven. At present, the strongest ideology is the scientism of the various disciplines and sub-disciplines. They are committed to defend as 'scientific' whatever it is that they are doing; equivalently they may be said to maximize the value of whatever intellectual capital they have acquired. In the absence of an objective mechanism of evaluation, they see no need to have their intellectual capital diminished by some uncomfortable empirical evidence.

I conclude this section by looking at how the authors of the surveys see the impact of this work.

Under the heading 'A better World: Taking Happiness Seriously' Layard makes 8 proposals for social policies that would generate more societal happiness. Some excerpts: "We should spend more on helping the poor..." "To improve family life, we should introduce more family friendly practices at work..." "We should subsidize activities that promote community life." "We should eliminate high unemployment". "...we need better education..." (p.233-4). These are the familiar exhortations that we hear day in, day out from pulpits, the media and public interest groups. Should we not also do more for peace? the environment? Do these noble aims stand a better chance given that they are supported by happiness research? Common sense suggests otherwise.

The discussion in Frey and Stutzer is more sophisticated since they discuss some of the fundamental difficulties that must be overcome before the findings of happiness research can be expected to actually raise the average level of happiness in a society. They criticize the

view that happiness research furnishes an empirical approximation to the long sought for social welfare function that could be maximized to determine social policy. Their objection is based on Arrow's impossibility theorem that precludes any rational aggregation of individual preferences. Needless to say, I disagree with this particular objection, since I argued in Section that Arrow's theorem is irrelevant. Furthermore, it is the principal achievement of happiness research that the factors conducive to happiness are rather universal, not only within societies, but also cross-culturally. This near unanimity also removes the sting from Arrow's objection. My objection is rather that happiness research does not furnish weights for the different factors that cause happiness; we cannot, for example, know on the basis of happiness research how much society should spend on education as compared to the environment. Another objection, in my view more fundamental, that is raised by the authors, is that even if the social welfare function were known, governments generally have no incentives to maximize it. Politicians, bureaucrats, and various social elites are often more interested in maximizing their own welfare than that of society.

Frey and Stutzer's positive proposal is that societies should focus on creating the institutions that are conducive to happiness rather than trying to influence particular outcomes. There is much truth in this general recommendation, but it does not solve the problem. The choice of institutions is a social choice like any other, only perhaps more difficult. The leaders of institutions generally resist change with all their might, since they cannot expect to maintain their leadership under alternative institutions. For example, altering the boundaries of nation states, or their internal subdivisions is a near impossibility and rarely comes about except in the context of war or revolution.

My conclusion is that happiness research, its impressive achievements notwithstanding, is by itself not a complete guide to social action. Of course, short of being a complete guide to social policy, as some of its more enthusiastic advocates envision it, happiness research could still be very useful if its findings came to be widely known and accepted. Even in this regard skepticism is in order. The various fields of academic social science surveyed in this paper suggest that they are driven more by various ideologies than by empirical relevance. As I have already noted, the basic insights have been around in one form or another for a very long time. There are also some fine books by economist that thought written in a more traditional style make many of the same points.¹ Thus far, none of this has been effective against the tide of neoliberal policies.

12. NOVELTY, REPLICATION AND THE GROWTH OF KNOWLEDGE

12.1 Novelty without validation

The material surveyed in this paper allows only one conclusion: social science has been increasingly dominated by varieties of both political ideologies and forms of scientism; contact with reality was increasingly lost; the most basic problems of society are not being identified, let alone adequately analyzed and workable solutions proposed. I do not mean to imply that a great deal of valuable material is not contained in the vast flood of social science publications. The problem is rather that the social sciences have not developed a mechanism for securing agreement on what is true, or relevant. Referring again to Ravetz' criterion of an immature science, the social sciences are unable to agree on what is factually the case.

Among the criteria that the social sciences have adopted from the natural sciences that of *novelty* is prominent. It is reflected in the torrential and ever expanding flow of social science publications. In the absence of criteria of relevance, this flood of publications produces alternating fashions instead of a cumulative growth of knowledge.

¹ I am aware of the following: Frank (1985), Mishan (1993), Scitovsky (1976, 1986), Thurow (1980).

The state of the social sciences can best be understood by looking at examples where agreement on factual truth, or on relevance, was actually secured. It is useful to distinguish two different kinds of knowledge that differ in how specific they are and consequently in the ease of verification.

12.2 *Quantitative knowledge and replication*

The type of knowledge that is typically associated with the natural sciences concerns quantitative empirical laws that are established and verified in controlled experiments. In this case it is evident how replication of the experiments leads to agreement. It is often claimed that the relative backwardness of the social sciences is due to their inability to conduct controlled experiments. This assertion is invalid for several reasons:

The alternative to establishing empirical regularities in controlled experiments is to establish them by statistical means. This paper has several examples that demonstrate that such replication is possible: the regularities of business cycles; the quantity theory of money and happiness research. The problem is both that more is not done by way of replication and that the results of replication do not become widely known, since they are not considered to be 'novel'.

The *Journal of Money, Credit and Banking* started a project on replication in the early 1980s that was reviewed by Anderson and Dewald (1994). The idea was to have authors submit their data, as well as computer software used, along with their articles, so that readers could check if the reported results actually follow from the cited data and reported statistical methods. Apart from some practical difficulties, such as data revisions, the project generated little interest and few such replications were actually performed. The project reveals a lack of understanding of what replication in science means; namely, the demonstration of the same empirical regularity against different data sets by independent investigators. This is the only meaningful form of replication since a regularity that applies only to one data set is hardly of interest.¹ Moreover, the verification of an empirical regularity against different data sets obviates the need to check an investigator's arithmetic; if he made errors of this, or any other kind, his results will not be replicated on other data sets. The previous sections have demonstrated that genuine replication in this sense is also possible in the social sciences if the required effort is made.

There is also a sense in which replication in the social sciences is possible. Societies have differed enormously both in their institutions and cultures as well as in the challenges they were confronted with; differences associated both with geographic dispersion and with alternate locations along the axis of historical time. This heterogeneity produces many 'natural experiments' that can be exploited analogously to controlled experiments. This is an avenue that has been insufficiently explored. Thus, in the vast literatures on different aspects of history, comparatively little attention has been paid to the question of what types of institutions have tended to produce favorable outcomes. In particular, little effort has been devoted to the study of the conditions that are favorable to the emergence of democracy²

An example of the successful exploitation of natural experiments is Titmuss (1970) that has been regarded as something of a social science classic. He made an international comparison of blood banks and found that those that operated on a voluntary basis worked well, those operated commercially worked badly. Titmuss' findings apparently had no impact

¹ The matter is somewhat different in the non-experimental sciences such as astronomy. Two aspects are important in this connection: One is the careful scrutiny of the data so as to create a canonical data base that different investigators can use. In astronomy, the most famous such data base was created by Tycho Brahe in the 16th Century and furnished the evidence on which Kepler based his laws of planetary motion. Most importantly, the general laws of nature do apply to other data sets. Newton's theory of universal gravitation, that explains Kepler's laws, was famously inspired by his watching an apple fall and imagining that the force that pulled the apple from the tree extended beyond into space.

²

on policy. Subsequent decades saw major scandals involving commercial blood banks, particularly in connection with HIV infected blood.

Still another unexploited source of experimentation is the legislative process. The consequences of new laws are rarely exactly those that were intended. Often, the discrepancy between intended and experienced consequences of a law is major. The consequence is a constant process of revision of existing laws. If the provisional and experimental character of laws were explicitly recognized, it would be possible to design them so as to minimize costs, particularly of unforeseen outcomes, and to maximize the learning effect. The prevailing political culture tends in the opposite direction: politicians feel that they must project an aura of confidence and pretend that they are making laws for all time.

12.3 Knowledge that Requires Judgment

Thus far I have discussed knowledge regarding issues that can be resolved by specific evidence. For example: Are the rich happier than the middle class? Is the quantity theory of money valid? There is another kind of knowledge that cannot be confirmed, or disconfirmed, in any simple, straightforward manner. Here there are many different kinds of evidence that may be difficult to evaluate as being either in favor of, or opposed to a hypothesis at issue. Nevertheless, regarding many such hypotheses a consensus was eventually reached, sometimes after a lengthy period of doubt, or even rejection. Examples are most readily found in fields that are evolutionary in the widest sense of the word; in addition to biological evolution, also the evolution of our planet, the solar system, or the universe. Hypotheses in these fields cannot, by their very nature, be verified experimentally.

The following may be cited as examples: The hypothesis of *continental drift*, proposed by Alfred Wegener in 1912. It remained controversial until the 1960 when it was incorporated into a new theory of *plate tectonics*.

Another example is Mendelian genetics. Mendel published his two laws of heredity in 1866. Regarding the reception and subsequent developments I quote from Wikipedia:

Mendel's results were largely neglected. Though they were not completely unknown to biologists of the time, they were not seen as being important. Even Mendel himself did not see their ultimate applicability, and thought they only applied to certain categories of species. In 1900, however, the work was "rediscovered"...

...the "rediscovery" made Mendelism an important but controversial theory...The model of heredity was highly contested by other biologists because it implied that heredity was discontinuous, in opposition to the apparently continuous variation observable. Many biologists also dismissed the theory because they were not sure it would apply to all species, and there seemed to be very few true Mendelian characters in nature. However later work by biologists and statisticians such as R.A. Fisher showed that if multiple Mendelian factors were involved for individual traits, they could produce the diverse amount of results observed in nature. Thomas Hunt Morgan and his assistants would later integrate the theoretical model of Mendel with the chromosome theory of inheritance, in which the chromosomes of cells were thought to hold the actual hereditary particles, and create what is now known as classical genetics, which was extremely successful and cemented Mendel's place in history.

The description shows that Mendel's laws could be firmly established only in the context of a complex, sophisticated theory of genetics that could explain a large variety of observations, some of them initially thought to contradict Mendel.

In the natural sciences there evidently exists a tendency, even if sometimes slow and halting, for opinion to converge on relevant and valid knowledge. Evidently this is not the case in the social sciences. The problem of voting, considered in some detail in the present paper offers a striking example. Voting theory, viewed as an essentially mathematical discipline began more than 200 years ago with the work of Borda and Condorcet, both mathematicians. Today it is a recognized discipline, with professional journals, associations, meetings and a voluminous record of publications. Yet, as argued in Section (), no progress is visible in the sense of a convergence of opinion regarding a superior method of voting.

I have argued that the problem of voting is just an instance of the problem of aggregating judgments. Doing this is the business of the opinion research industry. Within this industry,

the procedure to be followed is non-controversial. It is what I have called *utilitarian voting*: the judgments are expressed on a numerical scale and averaged. Recently I discovered that utilitarian voting is used by engineers in such field as automatic control and robotics. The typical problem is to obtain a 'best' value for some parameter that is observed by several independent sensors. The estimated is obtained by averaging the observations, a process that the engineering literature also refers to as 'utilitarian voting'.

The contrast between these fields is striking. The engineers and the opinion researchers are pragmatists; faced with real world problems, they espouse, without much debate, a fairly obvious solution. In academic social choice theory the task is to publish as many mathematical papers as possible in order to climb the academic career ladder. To admit a simple solution would undermine the *raison d'être* of the discipline.

What is true about voting theory is also true regarding the other major topics discussed in this paper: The stylized facts of business cycles were forgotten when they did not fit in with the prevailing ideologies. Economic measurement as a field of academic research has been largely abandoned without having arrived at acceptable solutions. The situation is similar throughout the various fields of social science. In any of these fields, if we ask what has been discovered in the form of significant confirmed knowledge, we find very little if any.

The inability to secure agreement on relevant and confirmed knowledge in the social sciences is the mirror image of a mistaken emphasis on novelty. The cumulative growth of knowledge in the natural sciences is intimately connected to observational instruments that penetrate realms unreachable by unaided human senses. In early science it was primarily the microscope and the telescope; in modern science it is the giant particle accelerators and observatories that inform us about the infinitely small and the infinitely large. In contrast to this, *homo sapiens* has been observing and thinking about his society since the beginning of his evolution. Today we have more refined methods for obtaining and organizing social data, the national income and product accounts are an example, that this could lead to a steady stream of startling new discoveries, as in quantum physics or cosmology, is hardly to be expected.

Society has not found a way to organize social thought so as to secure agreement on what is relevant. In one aspect this problem is less difficult than in the natural sciences, in another aspect more so. It is easier because fundamental aspects of society are more obvious. Adam Smith identified the two most basic socially relevant drives: selfishness and empathy. As a social animal man is predisposed to enhance both his own survival and that of his group. To extend empathy beyond the small groups in which *homo sapiens* has lived throughout most of his evolutionary history is a task for civilization that has so far been accomplished only very imperfectly.

The larger difficulty arises from a commitment to a democratic society. In the natural sciences it has generally been sufficient to secure a consensus within each scientific discipline. To the extent that scientific findings have commercial applications it is only needed to find an entrepreneur who will bring the resulting innovation to market; consumers can then decide to buy it, without any real understanding of the scientific principles involved. Questions of social organization, for example the design of a constitution, require not only a consensus among scholars, but also a wider social consensus. The constitution cannot be produced by an entrepreneur and sold on an individual basis. The fundamental problem of social organization of our time is that social science has not been able to accomplish the first of these steps, let alone the second.

Subsequent to Smith, social thought largely separated the two forces he identified and associated empathy with the political Left, selfishness with the political Right. In academic social science, sociology and psychology came to be associated with the Left, economics with the Right. Communism, the pure ideology of the left aimed at a cooperative society without selfishness or competition. Neoliberals, while a bit more ambivalent, certainly favor

competition over cooperation. European social democracy and American liberalism have attempted to bring the two forces into balance, with limited success.

13. IDEOLOGY AND REALITY: CONCLUDING REMARKS

The dysfunctional state of the social sciences is in my view only one instance of the increasingly dysfunctional state of institutions, including universities, and beyond that of the general culture. With modifications, this statement applies not only to the traditionally democratic countries of the West, but also to the rest of the world. I would define the current culture as a field strewn with the ruins of ideologies that failed; no one having an idea of how a new and functional architecture can be constructed from the rubble.

An important book that appeared recently in Germany, Grünewald (2006), illustrates the cultural situation. The author is a psychologist and psychotherapist who founded and directs an opinion research institute. Over the past two decades, psychologists at this institute conducted in depth interviews with almost all segments of the German population and on many topics that impact their lives. The interviews are not standardized; each lasts a minimum of two hours and attempts to reach deep layers of feeling and thinking. At the beginning of the interview, the subject might not be consciously aware of these deeper layers that lie below the image that the she usually projects. Similar research in other countries would undoubtedly yield somewhat different results, but I am convinced that the principal results are a reflection of the current state of what is by now in large parts a world culture.

The most important findings are: **a.** The prevailing attitude is one detachment. People are generally well informed, but non-judgmental. Their attitude towards the world is that of a spectator, as if watching a play in a theater. **b.** Emotionally they feel themselves and their society to be stressed, engaged in a rat race without visible progress. **c.** People lead to a considerable extent substitute, or virtual lives. This means that their attention and emotions are focused on stories and events presented in the media that are without any real connection to their lives.

Of these characteristics, the lack of a firm set of beliefs and values is in my view fundamental, while the other aspects are more in the nature of consequences. Grünewald explains the current mood of detachment as a reaction against the fervent hopes with which earlier generations embraced their ideologies. Nowhere was the alteration of ideologies more pronounced than in Germany. During the 19th and early 20th Centuries socialist movements struggled against autocratic regimes. The brief experiment in democracy of the Weimar Republic ended with the rise of Hitler. The collapse of the 'Thousand Year Reich' was followed in the West by social democracy promising ever rising living standards while all existential risks were taken care off by the state. In the East, communism was propagated as the ultimate form of social organization. German unification in 1991 provided once more a euphoric feeling of optimism. All of these hopes having been defeated, it is not surprising that people feel it is better to be noncommittal and emotionally detached.

There is I believe an additional cause of the turn to the worse of both the general culture and the social sciences. It is the advent of centralized, bureaucratized mass education following WWII. Of course, I share the belief that an educated public is a prerequisite for democracy to function. However, I believe equally that centrally directed mass 'education' is a self-contradiction. The essence of bureaucratic processes is standardization and the elimination of individual variation. This contradicts the essence of any meaningful education that must nurture the capability for critical, independent thought. Public education, like other social innovations, originated in the Eighteenth Century with the aim of teaching basic literacy to industrial workers. This was a limited, well defined task that public education was

able to accomplish. Today's public education systems produce conformist individuals trained to uncritically repeat whatever opinion they think is expected of them.¹

The post-WWII educational system did produce the 68 generation, who viewed themselves as revolutionary, but were highly conformist within their own ideological framework. Many of those active in the student revolts studied either the traditional social sciences, or the newly developing applied fields such as education or social work. These fields, without any clearly established standards, underwent a huge expansion. Many of those entering these fields came from backgrounds without any intellectual tradition, were of a doctrinaire mindset and in the mass of limited ability. The preferred careers sought by the 68 generation were in education, government, politics and the media. The resulting desolate state of both social science and the general culture is not surprising.

In 1960 the sociologist Daniel Bell published *The End of Ideology: On the Exhaustion of Political Ideas in the Fifties*. Since the 68 generation and the subsequent rise of neoliberalism were still in the future, the claim at that time was premature. Today, the end of ideology is at least for the contemporary world a reality. I do not claim that various ideologies, be they scientism, neoconservatism, or other 'isms' no longer have any advocates, the difference is that they no longer carry conviction for large parts of the world population.

Analogous to the idea of an end to ideology is the idea of *The End Of History* (Fukuyama, 1992). Fukuyama, another Rand graduate, argued that American style neoliberalism was the ultimate form of social organization so that with its worldwide adaptation the end of historical evolution would be reached. Fukuyama's mentor Hegel had claimed that the Prussian state of his day exemplified the end of historical evolution. Such claims are actually good predictors, not of what they claim, but rather of the end of an era. Only a society that has lost its creative drive will try to assuage that loss with fantasies of ever lasting uniformity.

Ideology is a subject of enormous importance that has received little by way of objective scholarly study. Beginning with Marx, ideology has been a label that we use to characterize the views we oppose. But ideology is pervasive; it needs to be analyzed objectively and brought out into the open if the decline of reason in modern societies is to be reversed.

I do not know what the future holds; with its general loss of faith in the institutions of society I can envision two possible paths. One is an increase in authoritarian government to compensate the increasing dysfunction of institutions. Authoritarian governments, based on the associated elites, will not be able to deal with the increasing complexity of the modern world. This path is likely to end in dictatorship. The better, though perhaps less likely path is the emergence of *genuine* democracy in which the public regains control over the elites that it appoints to perform various tasks.² This path requires more commitment and intelligence than is currently visible. History though always comes up with surprises

¹ Readings (1996) offers a trenchant criticism of the modern university as having lost its original purpose of contributing to and helping to shape the general culture. Instead, it has become a business geared to the requirements of the job market.

² The most incisive criticism of our current elites is Christopher Lasch, (1995), *The Revolt of the Elites and the Betrayal of Democracy*. The title is an allusion to Ortega y Gasset's *Revolt of the Masses*. Lasch's contrary position is that the current threat to democracy comes from the elites, not the masses.

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