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25 May 2008

Online at https://mpra.ub.uni-muenchen.de/8866/
MPRA Paper No. 8866, posted 26 May 2008 08:14 UTC

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Abstract

The present paper discusses the 'battle of methods' in economics in its epistemic pursuit in the framework of a dialectics between science and art. The traditional distinction between deduction and induction has come to be characterized as a 'theory-data confrontation'; while the former *a priori* approach has flourished in terms of mathematical economics, the inductive approach has fulfilled its mission through econometrics and experimental economics. The paper outlines the recent trends in econometrics and experimental economics in the context of empirical pursuit. We conclude the study, reiterating the contemporary consensus on the complementary roles of the two approaches: a theory-data confluence, not in a static, but in a dialectical framework.

In Quest of Truth:
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1. Introduction

'Economics is the only field in which two people can share a Nobel Prize for saying opposing things.' Thus goes a joke, with a tag of truth: 'Specifically, Myrdal and Hayek shared one.' And we are all familiar with the following remark by Sir Winston Churchill: 'If you put two economists in a room, you get two opinions, unless one of them is Lord Keynes, in which case you get three opinions.' Such pregnant jokes drive home the unpleasant implication of the common state of dissent among economists that surfaces in terms of the numerous schools of thoughts. It is true these different shades of schools, often opposite and overlapping, stand to enrich the subject, but also present a chaos of feuds capable of denuding the subject of its identity. This chaos of feuds is said to be characteristic of all social sciences, including economics, in contrast to natural science. And it primarily takes us back to the age-old riddle of finding the identity of economics in science or in art. We argue in the next section that economics is a dialectics between science and art.

Given the scientific nature, the methodological debate in economics has always been centred on what is called the theory-data confrontation. The 'theory first' approach was in vogue during the nineteenth century, whereby economic theorizing starts with deriving deductively certain laws based on certain initial postulates and then goes on to assess their validity in interpreting economic phenomena. Countering this *a priori* or deductive school was the 'data first' approach that argued that theory should emerge by induction from data, rather than being developed purely deductively. While the *a priori* approach has flourished in terms of mathematical economics, the inductive approach has fulfilled its mission through econometrics and experimental economics. The subsequent sections deal with these issues: Section 3 presents the development of mathematical economics as the engine of *a priori* quest and part 4 that of econometrics as the channel of inductive approach. Section 5 discusses the recent trends in econometrics and experimental economics in the context of empirical pursuit. The last section concludes the study, reiterating the contemporary consensus on the complementary roles of the two approaches.

2. A Dialectics between Science and Art

So, what is science? What is art?

In general, art refers to a collection of maxims or practical precepts to be followed in a successful venture; and science, to the truths resulting from the observation or examination of a phenomenon whatever. Art dictates precepts, lays down rules and directions; science observes real phenomena, exposes and explains the truths. For example, when an astronomer observes and explains the course of the stars, he is engaged in science; but when he deduces from his observations some rules to apply in navigation, he is in the realm of art, because his object is different, so also his method.

Note that art and science are closely related in this sense as the precepts of art are usually derived from the observations of science, but they *do* differ from each other in substance. For another example, take the case of medicine, which the physicians enthrone as a science; it is but no more than an art, the art of healing, as it consists in a collection of rules applicable to the cure of human diseases. But anatomy is a science; physiology is a science; as they both have as their object a *knowledge* of the human body, which they study, the first one its structure, and the other the functions of its organs.

Given this well drawn distinction between science and art, let us now consider that age-old question: Is economics a science, or an art? Is it a set of precepts, or of truths obtained from the observation of real phenomena? That is, does it show us how to do something? Or does it explain what takes place?

It goes without saying that economics since its inception has consisted in a great number of really objective observations that do tell us what takes place, or what exists. Thus economics is a science. At the same time, it also includes sets of directions, precepts, to be followed, solutions to questions relating to industry, commerce or the economic administration of states: questions of taxation, credit, finance, or foreign trade; that is, all the elements of art. Thus economics is an art. Here we are in a strange field of hermaphrodite: economics is both a science and art. However, it would not appear strange if we remember the close relationship between the two: the precepts, rules applicable to the conduct of human affairs are drawn from the established scientific truths. Scientific truths are not just an end in themselves; they essentially become meaningful with some fructification, say, in art. Science can lend its lights to art, correct its processes, direct its course. At the same time, art, in reciprocity, can impart the true value to the scientific truths. Art, without science, wanders off, and science, without art, remains barren. Thus there exists a dialectical relationship between the two and that characterises economics. Economics is a dialectics between science and art.

That economics is a science should not, however, be construed as setting it equal with natural science. The crucial distinction between the two is that economics is a social science that studies the behaviour of entities, individuals, with a higher degree of unpredictability, as opposed to the natural science that studies the natural world. Man is endowed with an untraceable freewill that leaves his behaviour beyond prediction. Given this unpredictability of individual behaviour, the behaviour of society, taken as the aggregate behaviour of individuals, remains beyond the reach of any deterministic analysis. Where general rules are derived in economics, heroic assumptions on rationality and consistency on the part of the economic agents are liberally utilized in contrast to the real facts of the world swarmed with agents of irrational and inconsistent behaviour. Needless to say, such exercises remain sterile.

3. Deduction: The Theory First School

Another major difference between the two is in respect of the scope of experiments in the face of theoretical incongruities. Natural sciences are fortunate in forging solutions to disputes by means of experiments where pure theory or mathematics fails. In physical experiments, variables and factors can be controlled and manipulated very precisely, and the discoveries can be formulated in mathematics as exactly as they are in reality. Such scientific experiments are impossible in economics; economists are totally unable to carry out experiments and gather data in such controlled environment, under the hypothesis of *ceteris paribus*.[1] That theories remain empirically unverifiable has lent strength to the unanimous acceptance of the *a prioristic*, deductive, approach as the unquestioned methodology of economics since J S Mill (1806 – 73),[2] who argued that the deductive (*a priori*) approach is the only method available to economic science as the experimental (*a*

[1] 'One possible way of figuring out economic laws ... is by controlled experiments ... Economists [however] cannot perform the controlled experiments of chemists or biologists because they cannot easily control other important factors. Like astronomers or meteorologists, they must be content largely to observe.' (Samuelson and Nordhaus 1985: 8).

[2] 'This is certainly not to suggest that methodological writing .. did not exist before Mill. There was, of course, an extensive methodological literature that preceded the publication of the *Wealth of Nations* in 1776, and there were also numerous methodological commentaries on Smith's treatise as most of the major works in early nineteenth-century British political economy. (See Redman (1997) for a recent discussion of this literature.) (Hands 2001: 14).

posteriori) approach is inefficacious in economics.[3] He also acknowledged that the deductively derived economic laws are only *tendency laws*, in contrast to the exact physical laws, and hence economics is an *inexact* science; the tendency laws are 'capable of identifying the underlying causal tendencies, but it may be very difficult to identify the myriad of countervailing forces necessary to refine the law so that it can be applied to specific concrete cases' (Hands 2001: 24).

Note that deduction was the typical method not only of the marginalists (Menger, Jevons, Wicksteed, Marshall, Robbins), but also of the equilibrium economists (Walras, Pareto, Arrow, Debreu), of the Austrian school (Mises, Hayek), and of the expectations economists (from Friedman to Lucas). Mathematical exercises in economics have been in line with this *a priori* epistemic pursuit.

Mathematical Economics

Note that the two great areas in which real *a priori* knowledge seems possible are logic and mathematics. Mathematics has been regarded as the acme of exact reasoning, a body of truths in itself, and the truth about the design of nature. Since the very birth of mathematics as an independent body of knowledge, fathered by the classical Greeks, as well as the Indians and the Chinese, mathematicians have pursued truth. The vast body of theorems about number and geometric figures has offered in itself an almost endless vista of certainty. The conviction that mathematicians are securing truths is epitomised in Laplace's remark that Newton was a most fortunate man because there is just one universe and Newton had discovered its laws. It has so happened that whenever someone

[3] 'But we can go farther than to affirm that the method *a priori* is a legitimate mode of philosophical investigation in the moral sciences; we contend that it is the only mode. We affirm that the method *a posteriori*, or that of specific experience, is altogether inefficacious in those sciences, as a means of arriving at any considerable body of valuable truth… (Mill 1874: 145)

wants an example of certitude and exactness of reasoning, he employs mathematics. So it has occurred in economics also.

The Lure of Mathematical Clarity and Certainty

It was the classical Greek philosophers who first realised the power of mathematics in clearing up the confusions and uncertainties of a social science – that is philosophy in that time. They believed that mathematics helps gain the same clarity and certainty for philosophy as for geometry, and as for physics and astronomy. The mathematician-philosopher Pythagoras was the source of Plato's view of mathematics as the supreme example of true knowledge. The Pythagorean motto 'all things are numbers' means that the essences and structures of all things can be determined by finding the numerical relations contained in them. Plato insisted that the reality and intelligibility of the physical world could be comprehended only through the mathematics of the ideal world. And the father of modern philosophy, Rene Descartes of the 17th century, did contribute a powerful methodology, analytic geometry that has revolutionised mathematical methodology. Descartes's mathematical bias was expressed in his determination to ground natural science not in sensation and probability (as did Francis Bacon) but in a principle of absolute certainty (mathematicism).

As is now clear, mathematics provides a logical, systematic framework within which quantitative relationships may be explored, and an objective picture of the reality may be generated. As already mentioned, the *a priori* reasoning about social and economic phenomena naturally invites and involves the use of mathematics. 'Among the social sciences, economics has been in a privileged position to respond to that invitation, since two of its central concepts, commodity and price, are quantified in a unique manner, as soon as units of measurement are chosen. Thus for an economy with a finite number of commodities, the action of an economic agent is described by listing his input, or his output, of each commodity. Once a sign convention distinguishing inputs from outputs is made, a point in the commodity space, a finite-dimensional real vector space, represents the action of an agent. Similarly, a point in the price space, the real vector space dual of the commodity space, represents the prices in the economy. The rich mathematical structure of those two spaces provides an ideal basis for the development of a large part of economic theory.' (Debreu 1987: 399-400). Remember economics includes a large number of quantitative variables based on these two central concepts, such as quantity of product produced or consumed, investment, price of the product, wages, interest, profit, income and so on. And the price-commodity two-dimensional space realises itself through the Cartesian geometry in the now common text book diagrams.

Application of Mathematics in Economics

Almost all the text books on mathematical economics start with the argument that '*there is no dichotomy between 'economics' and 'mathematical economics'*' (Archibald and Lipsey, 1973: 3) in so far as to find economics full of contexts for mathematical application such as:

1. Functions: Economics is all cause and effect relationships, such as: quantity of product demanded or supplied depends on price; consumption or saving depends upon income; saving or investment upon interest rate; labour supply or demand upon wages; and so on. All these are instances of the application of the fundamental mathematical notion of functional relationship.

2. Equilibrium: At equilibrium, supply equals demand – and we have equilibrium price, wage rate, interest rate, and so on. This involves the solution of a system of two simultaneous equations in two unknowns, say, price and quantity.

3. General Equilibrium: 'Everything depends on every thing else'. This suggests a large number of relationships, that is a set of simultaneous equations.

4. Marginal Concepts: all marginal concepts such as marginal cost, revenue, utility, product, propensity to consume, save, import, and so on are first derivatives of the relevant functions;

5. Optimisation: Profit maximisation, cost minimisation, utility maximisation, welfare maximisation, and so on are all obvious applications of the mathematical notion of optimisation, whether we manage it in words, diagrams or algebra.

6. Constrained Optimisation: The search for optimal points with reference to the production frontier or indifference curve or any other frontier involves an application of the mathematical theory of constrained maxima.

'This list could easily be extended, but we hope we have made our point: the familiar, basic ideas of economics usually turn out to be particular cases of problems that are handled in mathematics' (*ibid.*).

In the post World War II period, economics was so totally transformed; for one thing, there was an enormous increase in the application of mathematics, which came to permeate virtually every branch of economics (see Debreu 1987 and 1990 for a discussion). It is not to say that economics in its early period was devoid of the beneficial insights of mathematical reasoning. Extensive use of mathematics and modelling were there. The *Tableau Economique* of Francois Quesnay, the leader of the Physiocrats, the corn model of Ricardo and the two-department schema of Karl Marx are a few examples. With the advent of the marginalists, *viz*., Stanley Jevons, an Englishman, Anton Menger, an Austrian, and Leon Walras, a Frenchman, who replaced the labour theory of value of the classical political economy with the marginal utility theory of value, marginal values and optimal values came to the analytical forefront of economics, and justified the use of differential and integral calculus. It is worth noting that the Austrian school, led by Carl Menger, though in the deductivist line, deliberately shunned mathematics. The focus of their work was on the dynamics of the economic process, particularly the activities of entrepreneurs, rather than on market equilibrium.

It should be stressed that few economists made use of mathematics other than calculus in the pre World War II period. But economics underwent a revolution after that. Matrix algebra became important with the formulation of ‘input-output analysis’, an empirical method of reducing the technical relations between industries to a manageable system of simultaneous equations. It was an attempt to put quantitative flesh on the bones of a general equilibrium model of the economy. It was also aimed to reconcile the incongruence between microeconomics and macroeconomics by means of laying what are called the microfoundations of macroeconomics, as the latter, in its emergence as a distinct mathematical system, often tended to flout the axioms of individual behaviour on which microeconomics was founded. A closely related phenomenon was the development of ‘linear programming’ and ‘activity analysis’, which opened up a whole host of industrial problems to numerical solution and introduced economists for the first time to the mathematics of ‘inequalities’ rather than exact equations. Likewise the emergence of ‘growth economics’ or economic dynamics promoted the use of difference and differential equations.

4. Induction: The Data School

All these developments inevitably evoked a reaction. The Historical School in Germany argued that theory should emerge by induction from data, rather than being developed purely deductively, through mathematics or logic. This led to one of the most important ‘battle of methods’ in the history of economics, waged between Carl Menger and Gustav Schmoller during 1883-84 (for a good review, see Ritzel 1951). Menger (Austrian school) made a case for pure theory based on assumptions about behaviour and antecedent conditions: logical deductive method; and Schmoller (Historical school) for a study based on empirical historical data and inductive method. It goes without saying that this ‘*Methodenstreit*’, supported by the British economic historians and the American institutionalists, had a significant impact on the development of economic methodology to the extent of force-opening neoclassical economics for empirical studies, though in the Mengerian fashion: the discipline still remained largely a theoretic one based on 'as if' assumptions, developed through rigorous logic and used to derive general propositions, which are in turn used to derive hypotheses about reality, to be tested in empirical studies. 'Schmoller's vision of an empirical discipline based on factual studies, in which generalizations are both derived from and tested against data as they are developed', has still remained a far cry (Fusfeld 1988: 455). It should however be noted that 'Schmoller's vision' marked the genesis of the subsequent massive data-collection exercise in the US.

The impact of the *'Methodenstreit'* has still continued in terms of a bifurcation between pure theory and applied theory. The former builds up sophisticated mathematical models of individual decision making based on maximisation principles such as to derive existence proofs, whereas the latter considers only the reduced forms with a look out for corresponding data. This sharp difference in objectives naturally drives a wedge in the mathematics employed in the two approaches. There has been an increasing tendency to separate the 'pure theory' part of an investigation from the 'empirical' part even within individual articles (Backhouse 1998). As suggested above, this trend is more obvious in the mainstream, or neo-classical, economics in the Walrasian tradition in contrast to the tradition of Menger, Marshall and Keynes that makes only limited use of mathematics on methodological grounds.

Enter Econometrics

As explained above, scientific experiments as in controlled environment are impossible in economics that is but full of *ceteris paribus* assumptions. It was in this context that econometrics was developed in order to analyze the data collected from uncontrolled real world situations. The regression coefficients are assumed to represent the impact under the *ceteris paribus* assumption. Thus has become possible a fine combination of economic theory, mathematical model building, and statistical testing of economic predictions. The development of econometrics has in turn redesigned economics in general such that most of the new theories formulated have started to appear in empirically testable framework.

Econometrics is the study of empirical data by statistical methods, the purpose of which is the testing of hypotheses and the estimation of relationships suggested by economic theory. Whereas mathematical economics considers the purely theoretical aspects of economic analysis, econometrics attempts to falsify theories that are expressed in explicit mathematical terms. But frequently the two go together.

It was Sir William Petty who started to record and collect economic data in 17th Century Britain, giving economics an empirical quarter. The significance of statistics in economics was not ignored by even the *a priorists*; for instance, J. N. Keynes (1891: 328) wrote: "Besides affording absolute additions to economic knowledge, statistics are of great value in enabling the deductive economist, on the one hand, to test and where necessary modify his premises, and on the other hand, to check and verify his conclusions. By means of statistics, also, he may sometimes roughly measure the force exerted by disturbing agencies." Both the deductive and inductive economists have since worked on data, the former for validating theories and the latter for gaining data-driven insights. Thus Stanley Jevons ('Influence of the Sun-Spot Period on the Price of Corn': 1875) *inductively* spotted evidence for a sunspot-driven business cycle; Clement Juglar in 1862, for a credit-driven cycle; and H. L. Moore (1914; 1923) for a weather and astral-driven cycle. Jevons was one of the first economists to undertake serious statistical work in economics; he pioneered index number studies and time-series research (Morgan 1990: Ch 1) and Moore is credited to have also attempted to fit data to Walras's *deductive* demand equations. Similar exercises were carried out by A. C.Pigou in Britain and Jacob Marschak in Germany. E. W. Kemmerer tried to prove the quantity theory of money through inductive tests using the statistics of the US for the period 1879-1904; W. M. Persons (1910), however, was able to disprove the quantity theory using the same data set, though he was also fair enough to point out that the data might be inadequate for the test.

The inductive approach gained a fillip with the American Institutionalists, as also with W. C. Mitchell and his National Bureau of Economic Research (NBER) that set out deeper into the measurement of business cycles along with such empirical economists as Arthur F. Burns, John Maurice Clark, Simon Kuznets, Frederick C. Mills, Rutledge Vining, Solomon Fabricant, Leonard P. Ayres and other Institutionalists. Business cycle measurement engaged other economists also such as Persons and Bullock at Harvard, Kondratieff in Moscow, Wagemann in Berlin, Åkerman in Lund, Morgenstern at Vienna, and at the Kiel Institute. Notable among the 'data banks' in this context were of national income accounts of Simon Kuznets at the NBER and of Colin Clark in England; and of family expenditure data of R. G. D. Allen and Arthur Bowley in England. Also on the progress, side by side, were deductive, theoretical, treatises on business cycle, authored by J. A. Schumpeter, D. H. Robertson, A. C. Pigou and G. Haberler, of course, with empirical data presentation, sans statistical inference.

The NBER exercises came under severe fire from George Yule, Eugene Slutsky, Ragnar Frisch and Tjalling C. Koopmans. From the ashes rose econometrics, with the first econometric model of business cycles by Jan Tinbergen in the late 1930s based on the *General Theory* of J. M. Keynes. Tinbergen's econometrics was the first attempt at an estimation of the parameters of the Keynesian relationships; he was the first recipient of the Nobel prize in economics (jointly with Frisch). However, Keynes was highly sceptical about this 'black magic' (Keynes, 1939). He echoed the caution expressed by Mill (Hausman 1992, ch. 6), Marshall (Stigler 1999, ch. 1), and Robbins (1937) that social relationships are too complex, too multifarious, and too infected with capricious human choice to generate stable relationships that could be modeled with tractable probability distributions; like his many precursors, he assumed the precedence of theory over evidence. It is now generally accepted that "Keynes' criticisms of specific aspects of Tinbergen's work are often awry….what it amounts to is that on many points of detail Keynes' complaints are misplaced and on others he merely shows his ignorance of the econometrics literature of the previous twenty years." (Hendry and Morgan 1995: 55).

Despite such criticisms, 'the probability approach in econometrics', as famously proposed by Trygve Haavelmo (1944, who converted econometrics into probability), and popularised by the Cowles Commission, did come to stay. The 'measurement-without-theory' debate of 1947-49 between Koopmans' Cowles Commission and Rutlege Vining's NBER resulted in the clear victory of the 'probabilistic' approach, supporting Koopmans' argument that theory must be prior to data (Hendry and Morgan 1995, ch. 43). There then followed an econometric boom that brought out the Cowles Commission's Klein-Goldberger (1955) model of Keynesian macroeconomics and Modigliani *et al*.'s MPS model (M. I. T., Penn and the Social Science Research Council). The *identification problem* in econometrics, that is, the mapping between theory and data, was the major concern at the Cowles Commission. The general problem is illustrated in most of the econometrics textbooks as follows: given that both supply and demand describe quantity as a function of price and that we have only data on prices and quantities, how can we separately identify the supply and demand curve using the data? The Cowles Commission solution stressed that theory can deliver the exclusion restrictions necessary for identification of the behavioral parameters; these restrictions in turn appear as exogenous variables in one equation but not in another. For example, Klein's (1950) work is important in terms of its insistence on developing the relevant behavioral theory for each structural equation. Despite the failure of systems-estimation of macroeconomic models or of microeconomic demand systems to live up to its promise, the Cowles Commission methodology continued to dominate econometric thought; but by the middle of the 1950s, econometrics textbooks and research journal articles reverted to single-equation regression models.

Critiques galore tended to improve the tempo. Herman Wold, who undertook extensive theoretical work in time-series methods and empirical exercises of demand relationships, suggested recursive or block-recursive systems instead of the simultaneous techniques used in these large models; he argued that econometric observations are usually disequilibrium ones and developed an econometric version of 'process analysis' models in which prices adjust through a separately specified adjustment equation for the behaviour of wholesalers in the markets (Morgan 1991). Then there occurred two major events in the mid-1970s that marked a turning point: (i) the worldwide stagflation, the coexistence of high inflation and high unemployment, that played havoc with major macroeconometric models (for example, the Phillips curve, which had earlier appeared to trace out a stable negative relationship between the rates of inflation and unemployment and (ii) the famous Robert Lucas' (1976) criticism about the constancy of the structural parameters of large scale models that paved the way for time series macroeconometrics with microfoundations of dynamic choice and rational expectations. It is significant to note here that Oscar Morgenstern's 1928 claim that forecasts can be invalidated by agents' reactions to the forecasts was one of the forerunners to the Lucas critique; Tinbergen and Wagemann also considered the problem (Hendry and Morgan 1995: 17). Now the new research soon got grounded on better theoretical foundations and rational expectations. Also note that it was the critical stance of Yule (1926) in respect of what he called the nonsense/spurious regressions in time series that laid the groundwork for later developments in cointegration analysis.

Rational Expectations Econometrics

The breakdown of the Philips curve also called into question the predominant Keynesian idea of demand-driven business cycles and took note of the role in the contemporary macroeconomic fluctuations of supply shocks, such as the drastic oil price hikes in 1973-74 and 1979 and the worldwide slowdown in productivity growth as of the mid-1970s. This necessitated and Lukas proposed formulating a new macroeconomic theory grounded on microeconomic foundations with assumptions about consumers' preferences and firms' technologies. It is on the basis of these deep parameters that general-equilibrium implications for aggregate variables would be derived and confronted with data. But, Lucas was unable to raise an operational channel for his guidelines and the development of an alternative macroeconomic modelling approach, based on microeconomic foundations, had to wait for the advent of 'quantitative theory' of

Kydland and Prescott (1982). It should however be noted that there did exist some simple dynamic models of rational expectations with econometric estimation (for example, Sargent 1977, 1978, 1979), but these models were simplified representation of the economy or its parts. The first estimated macroeconometric models incorporating rational expectations ('from non-rational models') were Anderson (1979) and Fair (1979); the former included only current dated expectations of current dated variables and the latter was a fairly large model with 84 equations including expectations of future prices in the bond and stock markets. Notable among these models are the Liverpool model (Minford et al. 1984), the IMF Multimod (Masson et al. 1988), and the Global Econometric Model (GEM: Gurney 1990).

5. The Recent Trends in Theory-Data Confrontation

In the theory-data confrontation, econometrics, as we have seen, has commanded a pride of place; nonetheless, a great deal of recent attention has favoured what is known as experimental economics. We now turn to the contemporary trends in these areas, econometrics and experimental economics.

(i) Econometrics

Modern econometric methodology is characterized at present mainly by five approaches: vector autoregression (VAR), general-to-specific approach, vector error correction (VEC), calibration and nonlinear modelling.

Vector Autoregression

The VAR approach appeared as an alternative to the large scale econometric models based on the Cowles Commission approach in response to the identification problem they faced. The Commission's solution through restrictions came under attack (see for example, Liu 1960) in that the number of restrictions/exogenous variables required for identification of large-scale macroeconomic models often far exceeded the number that economic theory could legitimately afford to bear. Hatanaka (1975) showed that the lagged dependent variables cannot be used in identification, if we have to estimate the maximum lag length of an equation, or the order of serial correlation of the error process. The final attack came from Christopher Sims (1980), who, relying on Hatanaka (1975), argued that the classification of variables into endogenous and exogenous and the constraints implied by the traditional theory on the structural parameters are all arbitrary and 'incredible.' He therefore proposed an *atheoretical* way of estimating the dynamics of economic shocks, treating all variables in the reduced form equations as endogenous in order to avoid spurious identifying restrictions; VAR model is only the stacked form of stationary autoregressive (AR) models, with the same regressors for all equations. Estimation of unrestricted VAR model is hence simple, as single equation methods like ordinary least squares are consistent and, under normality assumption of the errors, efficient.

SVAR

The reduced-form interpretation of VARs, especially its atheoretical approach, however, came under severe criticism (for example, Cooley and Leroy 1985; Bernanke 1986) in that VAR results cannot be seen independently of a more structural macroeconomic model and that recovering those structural parameters from a VAR model requires imposing some identifying restrictions under *a priori* assumptions, which cannot be checked by statistical tools. That is, in a VAR analysis, the dynamic interactions between the variables are usually investigated by impulse responses or forecast error variance decompositions. The impulse response functions are used to generate the time path of the dependent variables in the VAR in response to shocks from each of the explanatory variables. If the system of equations is stable any shock should decline to zero (equilibrium), but an unstable system produces an explosive time path. Variance decomposition is an alternative method to the impulse response functions for examining the effects of shocks to the dependent variables. This method tries to determine the proportion of the forecast error variance for any variable in a system that is explained by innovations to each explanatory variable over a time horizon. Usually own series shocks explain most of the error variance, though the shock also tends to affect other variables in the system. Both the impulse response functions and the variance decompositions are, however, not unique. Identifying the shocks or innovations and the associated impulse responses also requires a priori assumptions. Thus was the genesis of the structural vector autoregressions (SVARs), as a popular tool for evaluating macroeconomic models. And with this we are back to square one: as Canova (1995) points out, once *a priori* restrictions are used to identify a VAR, the distinction between the VAR approach and the old simultaneous equation approach (of the Cowles Commission) becomes a matter of degree, not of content. Indeed, current research (for example, Diebold 1998) brings out the equivalence between the two approaches by demonstrating how VARs can be derived formally as the reduced forms of dynamic structural models. And this is how some of the recent researches now proceed: starting with a structural model and then deriving the reduced form as VAR, rather than considering the VAR as a reduced form and then deducing a structure from it by imposing restrictions.

Granger Causality

The 'Granger (non-) causality' test (Granger 1969; Sims 1972) in the VAR framework has become so popular among the researchers as an unfortunate means of conducting causality tests. 'Granger causality' in fact implies only an association between the current value of one variable and the past values of others (and its own) – a 'lead-lag relationship'; by no means does it mean changes in one variable cause changes in another. More formally, the 'Granger causality' test is used as a test for exogeneity, i.e., if the past values of a variable do not help predict the dependent variable, then it is exogenous with respect to that variable (Engle et al., 1983).

The test was first suggested by Wiener (Wiener 1956), and is often referred to more properly as 'Wiener-Granger causality' test. This model has prompted a great deal of debate among economists (for example, Zellner 1979) and even philosophers (for example, Holland 1986). Adrian Pagan (1989) sarcastically remarks: “There was a lot of high powered analysis of this topic, but I came away from a reading of it with the feeling that it was one of the most unfortunate turnings for econometrics in the last two decades, and it has probably generated more nonsense results than anything else during that time.”

Bayesian VAR

An alternative way of specifying VAR systems, thanks to Litterman (1986a, 1986b), was from a Bayesian perspective. This starts from the premise that the signal-to-noise ratio in economic data is very low and that economic theory involves a lot of uncertainty as to which economic structures are useful for inference and forecasting. The premise necessitates that in specifying an econometric model, the researcher try to filter maximum possible information from the data. However, in deciding whether a particular variable or lag enter the autoregression, this method makes use of a symmetric atheoretical prior probability distribution[4] on all variables to trade off overparameterization with oversimplification, instead of relying on classical hypotheses testing or economic theory (Todd 1984). ‘The prior is characterised by a small number of parameters’ and ‘acts as an orientable antenna which, when appropriately directed, may clarify the signal.’ (Canova 1995: 85). The prior is *objective* in the sense that it is the assessment, though subjective, of an experienced expert and it is *atheoretical* since the restrictions put in the prior have no economic interpretation.

[4] In Bayesian statistical inference, a prior probability distribution, often called simply the prior, of an uncertain variable (say, the proportion of voters who will vote for a particular candidate in a future election) is the probability distribution that would express one’s uncertainty about it before the ‘data’ (for example, an opinion poll) are taken into account. It is meant to attribute uncertainty rather than randomness to the uncertain variable.

The BVAR methodology is distinct in its flexibility as (i) all the tools developed in the context of time varying coefficient models are applicable to some of its specifications (see, for example, Nicholls and Quinn 1982) and (ii) it includes as special cases several time series models used to characterize the properties of macro and financial time series, such as ARCH and ARCH-M models, bilinear models, discrete regime shift models and subordinated models (see Nickelsburg and Ohanian 1987; Canova 1993). Though the VAR methodology is based on time series principles and has been widely employed in a variety of macroeconomic fields, its analytical applicability has been extended to panel macro data (for example, Canova and Dellas 1993) and panel micro data (for example, Holtz-Eakin 1989). Despite such wide recognition, a number of objections have been raised against the methodology at all stages of the analysis, on the robustness of data characterizations with VAR, its usefulness in analyzing historical episodes and their significance for policy analyses: see for a detailed review of criticisms, Canova (1995).

Vector Error Correction Models

Another way of formulating and estimating VAR models was in terms of making use of the parallel developments in non-stationary time series analysis that culminated in cointegration. Note that the VAR framework does not take seriously the relevance of unit root consequences and hence the associated tests. The spurious regression problem as discovered by Granger and Newbold (1974) was an eye-opener to the consequences of ignoring non-stationarity in time series variables that can lead to seriously misleading conclusions when used to model relations between them. This prompted a long chain of unit root tests to identify the degree of integration of the variables of interest or the degree of differencing to induce stationarity before modeling relationships. It should be noted that differencing to induce stationarity in non-stationary series has been an important stage in autoregressive-integrated-moving average (ARIMA) model building (Box and Jenkins 1976). However, solving the non-stationarity problem via differencing was soon equated to ‘throwing the baby out with the bath water’, because differencing results in ‘valuable long-run information being lost’. The concept of cointegration as an alternative was introduced by Granger (1981) and Engle and Granger (1987), and is used as a statistical property to describe the long run relationship among economic variables. Engle and Granger (1987) suggested a residual based single equation method for testing for cointegration relationship between two variables. This single cointegrating vector was interpreted as a structural identified relationship; however, in a multivariate context, it was still recognized as a reduced-form equilibrium relationship, not a structural identified one. Since a structural system implies the existence of multivariate cointegrating vectors, preferences went for system estimation methods; the most popular is the system method in a VAR framework, the Johansen (or Johansen and Juselius, JJ) method (Johansen 1988; Johansen and Juselius 1990), based on maximum likelihood technique that estimates the cointegrating space of a reduced form VAR system, that still fails to solve the fundamental problem of identification: none of the cointegrating vectors generated in the framework can be given a structural interpretation.

An important property of cointegration or long-run relations is that they can be associated with relations derived from economic theory. It is therefore useful to construct models that explicitly combine the long-run and short-run parts of a stochastic process. And this is made possible by the 'Granger representation theorem' (Engle and Granger 1987) that states that if a set of variables are cointegrated, then there exists a vector autoregressive moving average (VARMA) representation for them and an error-correction mechanism. This defines the vector error correction or equilibrium correction model (VECM), that brings together 'Granger causality', concerned with short run forecastability and cointegration, concerned with long run equilibrium. The model also opens up the possibility to separate shocks or innovations with permanent and transitory effects and this helps to identify impulse responses of our interest.

VECM can be seen as a reduced form VAR model in which the structural coefficients are identified. Like VAR, it treats all variables as endogenous, but limits the number of variables to those relevant for a particular theory. Thus, unlike VAR, VECM is very much theoretical.

The LSE Approach

In a limited sense, the London School of Economics (LSE) approach emerged in response to the VAR methodological problems: the major drawback being that all the variables entering the VAR system have identical lag lengths, which need not be a valid economic or statistical restriction and may reduce the efficiency of the estimates. For selecting the optimal lag length, Hsiao (1981, 1982) suggested a sequential procedure by combining Akaike's (1969) final prediction error approach for univariate autoregression with 'Granger causality' testing. The appeal of this procedure lies in its major advantage that by imposing statistical exclusion restrictions, it substantially reduces the number of parameters to be estimated. As an alternative to this 'specific-to-general' approach, David Hendry and others at the LSE suggested a 'general-to-specific' (Gets) procedure (Hendry 1990; Hendry and Mizon 1990; Clements and Mizon 1991). The key to this approach is to select an unrestricted 'congruent' VAR, given available information, where a congruent model is one that captures the dynamic relationships existing in the data, that is free from specification errors and that has constant parameters. After selecting such a model, the dimension of the VAR is reduced with a number of t-tests and F-tests on the significance of the lag of one variable in one or all the equations. By purging the insignificant lags from the original specification, we get a parsimonious VAR (PVAR) model. Thus practically, the gets approach involves starting with as broad a general specification as possible and then searching over the space of possible restrictions for the most parsimonious specification. This method is also known as Hendry's theory of data reduction (Hendry 1995, ch. 9). The origins of the theory of reduction are found in the analysis of exogeneity in Engle, Hendry, and Richard (1983).

It is significant to note that the LSE approach, like the VAR approach in its initial formulation, functions in terms of probabilistic reduction rather than the theory of errors. However, theory does play a part, as a bridge principle, in helping to define the range of variables to be included in the local data-generating process and to choose their interpretable transformations, but Koopmans' notion that a complete, *a priori* theoretical articulation must precede statistical investigation is rejected. Although data may be packaged in more or less illuminating ways, it is the job of theory in the LSE view to conform to, and explain, the facts of the data, not of data to conform to the presuppositions of theory.

The LSE methodology originated in the work of Denis Sargan and is now usually known as Hendry or Gets methodology; it has much appeal mainly in Britain and Europe (see Mizon 1995 for a systematic discussion). It is closely related to a wider range of works on integrated and cointegrated systems of Engle and Granger and of Johansen and Juselius. While Koopmans' apriorist view of the Cowles Commission's methodology and Sims' SVAR methodology belong to the theory-of-errors approach, the LSE procedure (like the VAR method in its initial formulation) belongs to the probabilistic-reduction approach. However, like the VAR methodology, the LSE approach stresses dynamic specification with special attention to the lag structures. The key difference is that the LSE approach accepts the framework of stationarity and cointegration; and detests profligate parameterizations but aims at parsimonious specifications (PVAR).

Whether models are nested or non-nested, it is important to compare them and evaluate their relative merits. The LSE approach here offers its leading principle: the theory of encompassing (Mizon 1984, Hendry 1988). "The encompassing principle is concerned with the ability of a model to account for the behaviour of others, or less ambitiously, to explain the behaviour of relevant characteristics of other models." (Mizon 1984). Roughly, a model encompasses another model if the former can account for results obtained from the latter; that is, one model encompasses another if it carries all the information of the other model in a more parsimonious form such that if the first model is available, then the second has nothing more to offer. It should be noted that while encompassing is the key idea of the LSE methodology, it has somewhat been overshadowed by Hendry's general-to-specific modeling strategy, which has mostly been targeted by the critics.

The main problem with the Gets approach as pointed out by the critics (Faust and Whiteman 1995, 1997) is that it is vitiated with a data mining process, wherein the large number of sequential tests leaves the reported test statistics uninterpretable. This objection finds support from studies of data-mining algorithms that show large size distortions (for example, Lovell 1983). However, the supporters contradict the claim: there are in fact two sources of error or costs that need to be considered: (i) the cost of search in terms of size distortions; and (ii) the cost of misspecification. It is pointed out that the critics of data-mining tend to stress the first but ignore the second. However, Monte Carlo studies of the efficacy of Gets search shows that the costs of search are small: size tends to be close to the nominal size of the exclusion tests used in the search; power achieves a high fraction of the power given knowledge of the correct specification; and the ability to recover the true specification is high (Hoover and Perez 1999, 2004; Hendry and Krolzig 1999 and Krolzig and Hendry 2001).

The calibration methodology stands opposite to the LSE approach by maintaining a commitment to prior core economic theory.

Calibration

The calibrated dynamic general-equilibrium macroeconomic models have emerged as a challenge to both the conventional 'systems of equations' macroeconometric and VAR approaches, in response to Lucas's call for an alternative to the Keynesian paradigm; the methodology has originally arisen from that of computable general-equilibrium models (Johansen 1960; Mansur and Whalley 1984) and is now largely associated with the 'quantitative theory' of Finn Kydland and Edward Prescott (1982), the first to characterize the general equilibrium of a dynamic and stochastic macroeconomic model based on microeconomic foundations..

Like Koopman, but unlike the LSE approach, the calibration methodology firmly believes in prior core economic theory. Thus it starts with a theoretical model – for example, in the general framework of Kydland and Prescott, a microeconomic representative-agent rational-expectations neoclassical growth model – that involves solving a set of interrelated dynamic optimization problems through extensive use of numerical methods; hence model simulation or calibration. The theoretical model is completed by assigning numerical values to the key parameters (see Hartley, Hoover, and Salyer 1997 and 1998; Kim and Pagan 1995, for detailed discussion). However, these values are not estimated ones, as in the Cowles Commission program, through system equation macroeconometric methods (as, for example, in Hansen and Sargent 1980). Rather, they are taken from considerations of national-accounting ratios, common sense, experience, and other informal sources; for example, microeconomic data on various shares and elasticities in Kydland and Prescott (1982) and data on the input-output structure of industries in Long and Plosser (1983). Thus an advantage of the calibration approach is the imputation into the model of established findings, including salient 'stylized facts' of economies. Note that Kydland and Prescott (1991) reject Haavelmo's probability foundations in econometrics, that is, the formal Neyman-Pearson inferential statistics,[5] because that variant of the Cowles Commission methodology seeks to directly estimate completely articulated theoretical models. Once the 'well-established theory' and the 'deep parameters' are accepted, the calibrated model is validated, or the relevant

[5] "We choose not to test our model against the less restrictive vector autoregressive model. This would most likely have resulted in the model being rejected given the measurement problems and the abstract nature of the model. Our approach is to focus on certain statistics for which the noise introduced by approximation and measurement errors is likely to be small relative to the statistic. Failure of the theory to mimic the behavior of the post-war U.S. economy with respect to these stable statistics with high signal to noise ratios would be grounds for its rejection." (Kydland and Prescott 1982: 1360)

'law of motion' is computed,[6] through computer simulation in such a way that the 'carefully specified set of dimensions' of the model are in line with reality, that is, the simulated moments are consistent with the actual moments of business cycle phenomena. For instance, Kydland and Prescott (1982) found that the covariances between model series and the autocorrelations of model output were consistent with the corresponding statistics for the U.S. data.[7] After validation, the model is ready for use, for example, to explain historical economic performance and for policy analysis.

Calibration approach also has come under severe criticism:[8] Can we count this methodology, with its rejection of statistical estimation, as an econometric methodology? Kydland and Prescott (1991) do defend it as *bona fide* econometrics, as, according to them, it clearly agrees with the original vision of the Econometric Society that views econometrics as 'economic theory in its relation to statistics and mathematics.' Moreover, they regard basic economic theory as established, secure, knowledge that need not be tested. They argued that applications of the available econometric methods were inappropriate for the current stage of model development: "in spite of the considerable recent advances made by Hansen and Sargent, further advances are necessary before formal econometric methods can fruitfully be applied to testing this theory of aggregate fluctuations" (Kydland and Prescott 1982: 1369). Some econometricians have since gone to the extent of recasting calibration approach into a form that would stand more in common with the mainstream understanding of econometrics (for example, Gregory and Smith 1991; 1993). Regarding the ad hoc parameterization, it is suggested that an

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[6] "Unlike theory in the physical sciences, theory in economics does not provide a law of motion" (Kydland and Prescott 1994: 2); hence we have to compute the dynamic components.

[7] The "results indicate a surprisingly good fit in light of the model's simplicity" (Kydland and Prescott 1982: 1368).

[8] Also see the symposium on calibration in the 1996 Winter issue (Vol 10, No. 1 pp. 69-120) of the Journal of Economic Perspectives.

adequate statistical characterization of data may help supply robust parameterization for calibrated models: Hoover (1994a), for example, has in this context proposed the application of the encompassing principle for a better choice from among competing calibrated models. There are also several attempts at improvements on Kydland-Prescott initial measures (see Basu and Fernald 1997, 2000). Surprisingly, econometric estimation also has gradually come on the scene, starting with the maximum likelihood estimation of Altug (1989) and McGrattan (1991) in a linearized version, using similar methodology as in Hansen and Sargent (1981); Christiano and Eichenbaum (1992) argues that generalized method of moments is the appropriate method to calibrate and applies it to their real business cycle model; Smets and Wouters (2003) provides a full structural Bayesian estimation of a nonlinear model for the Euro area. Most of these developments have now collectively given rise to a new school, referred to as new-Keynesian business cycle research, that examines monetary models of business cycles based on frictions in the process of forward-looking price-wage adjustments based on rational expectations (see, for instance, Rotemberg and Woodford, 1997, Clarida, Gali, and Gertler, 2000, and Dotsey, King, and Wolman, 1999). The new approach has synthesized the Keynesian analysis with the Kydland-Prescott's real business cycle analysis.

Non-linear Modelling

Economic dynamics is one of the developments of mathematical economics, but it often falls short of expectations on it. Dynamic models, for example, are typically formulated in terms of linear equations, not because the world is linear but because nonlinear equations can be very difficult to solve. This preference for mathematical convenience has led to intense debates over the limitations of mathematical models in representing the real functionings of nature. For an example, differential equations represent reality as a continuum, in which changes in time and place occur smoothly and uninterruptedly, even though sudden breaks and qualitative changes do take place in nature. The debate on chaos and order has centred on those areas involving breaks in continuity, or nonlinear relationships. And the revolution in computer technology has transformed the situation by making non-linear mathematics accessible to mathematicians and other scientists, determined to crack the chaotic systems.

James Gleick's best-selling 1987 book *Chaos: Making a New Science* describes how different researchers have examined chaotic systems using widely different mathematical models, all but giving the same conclusion: on 'suggestions of structure amid seemingly random behaviour.' Since economic time series appear 'random' and are difficult to predict, economics has got a natural opening for chaos theory: Anderson, Arrow and Pines (1988) thus discuss the possible pathways of complex dynamics and chaotic dynamics in an economy. The October 1986 issue of the *Journal of Economic Theory* presents some interesting works in this context, on economic models constructed to generate deterministic equilibrium trajectories that appear random, some models even with deterministically chaotic rational expectations equilibria (also see Baumol and Benhabib 1989; Lorenz 1989). Testing for the presence of such deterministic generators (that appear chaotic to conventional statistical tests) has led to the development of a powerful tool of statistical inference, called BDS test (Brock, Dechert and Scheinkman 1987); it tests the null that a time series is independently and identically distributed against an unspecified alternative using a nonparametric technique. Thus the test is used to find out the existence of potentially forecastable structure, nonstationarity or hidden patterns in financial and macroeconomic time series models.

Besides the chaos theory specifications, econometrics has developed some useful tools to deal with the nonlinear characteristics of the empirical data, for example, financial time series that display typical nonlinearity, in terms of, say, asymmetric behaviour: say, large negative returns appear more frequently than large positive returns, or large negative returns are often a prelude to a period of great volatility, while large positive returns are less so.

Econometrics now deals with a large number of (univariate) nonlinear models. A popular idea in economic applications is to define different 'states of the world' or 'regimes', which suggests that the data generating process to be modelled is taken as a linear process that switches between a number of regimes and its dynamic behaviour depends on the particular regime. This ‘state-dependent dynamic behaviour’ of a time series implies that its certain properties, such as its mean, variance, autocorrelation, are different in different regimes. As an example for such state-dependent or regime-switching behaviour we may consider changes in government policy leading to a change in regime. LeBaron (1992) shows that the level of volatility of stock returns has regime–switching dynamics, while Krager and Kugler (1993) and Chappell *et al*. 1996) argue that exchange rates also behave so.

Regime-switching models appear in different representations, the important ones in the autoregressive framework are:

1. Threshold autoregressive (TAR) model (for example, the standard self-exciting threshold autoregressive (SETAR) model: Tong 1990; momentum-TAR model: Enders and Granger 1998).

2. Smooth transition autoregressive (STAR) model (Chan and Tong 1986) (exponential autoregressive (EAR) model/exponential smooth transition autoregressive (ESTAR) model: Haggan and Ozaki 1981; Terasvirta 1994; Arango and Gonzalez 2001; logistic smooth transition autoregressive (LSTAR) model: Terasvirta 1994).

3. Hidden Markov or Markov-switching autoregressive (MSAR) model: Hamilton (1989) – a popular model of application to GNP and interest rates.

4. Artificial neural network (ANN) model – another popular model (see Fine 1999; Haykin (1999); White (1989).

(ii) Experimental Economics

If scientific status is denied to economics on the ground that it does not support an environment for experiment and hence yields no natural laws or universal constants, then astronomy, geology, biology, psychology and even parts of physics do not merit the title. As already explained earlier, scientific experiments in controlled environment are impossible in economics with its all *ceteris paribus* assumptions and this experimental vacuum has had much to do with the dominance of *a priori* theory in economics; it was as a challenge to this context that econometrics emerged; so did experimental economics too.

It is significant to note that the theory of probability (R.A. Fisher 1930, 1935) did have a design of experiments in its basic context; for example, it was so with the St. Petersburg paradox of the eighteenth-century Swiss mathematician Daniel Bernoulli (1738; English trans. 1954). Stigler (1999, ch. 10) substantiates the development of experimentation in psychology with its early adoption of probabilistic statistics (for example, by the polymath philosopher C.S. Peirce in the 1880s). The interdisciplinary relationship of economics with psychology and other recent branches of knowledge, such as neurobiology and artificial intelligence, all disciplines sharing the same object of explaining human decision-making processes in different contexts, has naturally opened up experimentation in economics. Thus experimental economics has now become a flourishing field of economic research that uses laboratory methods to study phenomena that are difficult to observe directly in naturally occurring economic contexts; it indulges especially in microeconomics, public economics and environmental economics.[9]

[9] *Experimental Economics*, the official journal of the Economic Science Association, serves the growing group of these economists. See the *Handbook of Experimental Economics* edited by Kagel and Roth (1995), and the works carried out in the *Environmental and Experimental Economics Research Laboratory* of Georgia State University in Atlanta (http://prcweb.gsu.edu/enveco/enveco.html).

The first experiment in economic study was carried out by Thurstone in 1931, though it failed to attract any interest. The study sought to verify the neoclassical consumer theory, by empirically determining an individual indifference curve. Every participant in the experiment was asked to make a *hypothetical* (not actual) choice between baskets containing hats and shoes, hats and jackets, and shoes and jackets. Based on the data related to the choices of each individual, he estimated an indifference curve, found to be hyperbola-shaped, consistent with the theory. After a decade came some valid comments on this experiment; Milton Friedman and Allen Wallis (1942) argued with some force from a methodological point of view that the subjects undergoing such experiments act in an artificial environment such that their motivations are not the same as those assumed in the theory and moreover, in this specific case, they made hypothetical choices only. In the light of this criticism, incentives were later introduced for participants by some experimental economists but others still consider payoffs unnecessary.

It was the expected utility theory of von Neumann and Morgenstern (1944) that gave a new focus to experiments in individual choices; the game theoretic approach sparked a wave of experimental tests of interactive behaviour, resulting in a large volume of literature such as on Prisoner's Dilemma (Flood 1952, 1958), provision of public goods, coordination and equilibration, auction theory, and the effects of different rules of market organization. Compared with the earlier experiments, the expected utility theory made more focussed predictions, though the results were much less definite: while some experiments (for instance, Mosteller and Nogee 1951) supported the utility theory as an adequate approximation of individual behaviour, some others (for example, Allais 1953) refuted the claim. A large number of experiments have since ensued, exploring for alternative choice theories (for example, the Prospect Theory of Kahneman and Tversky 1979).

Besides the game theory, other contributory sources to experimental economics include neural networks, artificial intelligence, cognitive psychology, and neurobiology (Hayek

1952; Rizzello 1997; Leland 1998). Artificial intelligence not only has an instrumental role (by way of constructing computer systems, capable of intelligent action, useful in experiments), but also is concerned with understanding the nature of intelligence, by studying (a) ‘human brain’s syntax’ applied to decision making, or (b) emergence of behavioural rules and multiple equilibria through simulations with artificial agents and neural networks (Terna 1992 and 1995). Expert systems in artificial intelligence programs that attempt to accomplish tasks by acquiring and incorporating the same knowledge as human experts, have sparked important insights in reasoning under uncertainty and causal reasoning. Cognitivism, an offshoot of the development of the concept of feedback in cybernetics and its application in psychology, presents an organism not only as adjusting itself to the environment (consistent with the stimulus-response theory of behaviourism), but also as *modifying* that environment according to its needs. The focus here is on the processes of information gathering by means of perception, subjective reorganization, and interpretation of the environment (Gardner 1985). Unlike cognitive psychology that concentrates on subjects themselves, neural network (or connectionism) is concerned with the system’s dynamic development, on the network connecting subjects (Parisi 1991). It seeks to create behavioural models simulating, by means of artificial agents, the emergence and development of complex adjusting systems using specific software and is thus related to artificial intelligence. However, the extended use of artificial intelligence in experimental economics for computer simulation has launched a volley of criticism: simulations do not supply empirical data, and cannot be taken for actual experiments; rather they may at best be considered on a level with theoretical results (Friedman and Sunder 1994: 5).

The most significant practical objection against experimental economics is on the basis of the *Hawthorne effect*, that is, the subjects under experimentation, if they are aware they are under observation/experimentation, tend to engage in affected behaviour that contaminate the experimental results so that they cannot be taken as indicative of normal behavior. The only way to avoid the possibility of such affected behavior is for the experimenter to try to keep the subjects from perceiving that they are subjects of experimentation, but there may be moral implications to this approach as well. For any of the reasons expressed above, economists have tended to avoid an experimental approach.

6. Conclusion: Theory-Data Confluence, Not Confrontation

Though the theory-data confrontation still continues in terms of distinct exercises (i) in mathematical economics as the engine of real *a priori* knowledge and (ii) in econometrics or in experimental economics as the channel of empirical knowledge, there seems to be a consensus that the deductive and inductive methods are complementary: theory-data confluence. It is significant to note that this view is by no means something new; by the end of the nineteenth century, statistics, as quantitative induction, was considered able to provide strong empirical foundations to economics (for example, see Keynes 1891). Jevons (1871: 12) was emphatic in expressing his support to this complementarity: 'The deductive science of economics must be verified and rendered useful by the purely empirical science of statistics.' And this was later echoed in Moore (1908: 1-2): 'economics will become an empirical science when its deductive component is supplemented with an adequate inductive component based on statistics.' It was such a vision that guided the founding of the Econometric Society in 1930 (Frisch 1933).

However, doubts remain as to the effective achievement of this complementarity, as it is found that 'theorists continue to turn out model after model and mathematical statisticians to devise complicated procedures one after another' (Leontief 1971: 3). It is true that econometrics has developed from its humble start of 'least squares curve fitting' into a powerful statistical tool for mapping all types of data, whether it be cross section or time series or panel. Moreover, the widespread use of statistical software on personal computers has lowered the cost of generating empirical evidence. But its reliability still remains low thanks to non-testable probabilistic assumptions, leading to an 'indifferent performance' on the empirical front (*ibid*.). It goes without saying that such a 'weak and all too slowly growing empirical foundation clearly cannot support the proliferating superstructure of pure or ….speculative economic theory' (Leontief 1971: 1). We feel that this imbalance must be located in the casual consideration of theory-data confluence in a static framework. What is required is a dialectical confluence.

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