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The Interaction Between Theory and Observation in Economics*

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Abstract: In economics both theories and observations tend to be very heterogeneous. This paper discusses the methodological difficulties that are involved in relating theories and observations and the characteristics of each that influence the way they interact. We examine the factors that influence the impact of observation on theory in terms of the precision of measurement of the observations; the correspondence of the measurements to the theoretical concepts; the applicable domain of the theory, i.e. the extent to which it has implications for observable features of the economy; and the importance of the results to decision makers. In this interaction between theory and observations, statistical models play a central rôle and we discuss how this rôle evolved in the context of four statistical models: single equation and multivariate regressions, ARIMAs and VARs. The paper concludes with a brief discussion of an approach to modelling theories which do not impose standard restrictions on these statistical models.

I INTRODUCTION

The interaction between theory and observation in economics has always been problematic both at a methodological and at a substantive level. John Stuart Mill commenting on the tension between "theory" and "practice"

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writes "The most universal of the forms in which this difference of method is accustomed to present itself, is the ancient feud between what is called theory, and what is called practice or experience", (reprinted in Hausman, 1984, p. 55). In a similar vein Jevons notes that "The deductive science of economics must be verified and rendered useful by the purely inductive science of statistics. Theory must be invested with the reality and life of fact. But the difficulties of this union are immensely great", (cited in Morgan, 1990, p. 5). Despite the repeated call for a close relationship between theory, observation and practice, the reality has often been quite the contrary. Currently, the gulf between theory and measurement remains as wide, and in some respects, wider than ever. This is graphically illustrated by the comments of Mirowski (1991) and Summers (1991). Mirowski using a post-modernist philosophy of the relative autonomy of different functions within a discipline uses an analogy between the role of theorists, instrument builders and experimentalists in science to examine the lack of communication between economic theory, econometric theory and applied econometrics. Summers comments on "the negligible impact of formal econometric work on the development of economic science". Similarly, many econometricians would comment on the negligible impact that economic theory has had on any explanation of the data.

This paper examines some aspects of this fraught interaction between theory and observation in economics. The first part of the paper considers the influence of observation on theory. One important feature is the heterogeneous nature of both the theories and the observations used in economics, and Section II discusses some aspects of this heterogeneity and their consequences for the interaction of theory and observation. Section III examines some of the factors that influence the impact that observations have on theory. The second part of the paper considers how theory is used by empirical workers, particularly applied econometricians, in analysing the observations. Empirical work has to try to synthesise theory and observation in constructing effective models. Section IV characterises this process and discusses the criteria used to evaluate empirical economic models. In constructing such models, econometricians use statistical models, and Section V discusses the evolution of the main statistical models used and the rôle that the theory has played (if at all) in their development, since the work of the Cowles Commission. Currently, there is a major tension within particular parts of the subject. Stochastic optimising theories which take a particular form, the LQ form, involving Linear constraints and Quadratic objective functions are well understood and can be relatively easily confronted with the estimates from a statistical model. However, this is not possible with more complicated dynamic stochastic optimisation processes. This has caused a number of workers to calibrate rather than estimate their models. This approach is also discussed in Section V. Section VI suggests an alternative approach which builds on the rigour and precision of these more complex dynamic optimisation theories but allows a more flexible specification of the theory for confrontation with the data. Section VII contains some concluding remarks.

This paper complements Pesaran and Smith (1992) which emphasises the historical evolution of the use of economic theory in econometrics and provides more technical detail on the suggested alternative approach for bridging the gap between theory and evidence.

II THE NATURE OF THEORIES AND OBSERVATIONS IN ECONOMICS

The interplay of theory and observation raises a host of difficult philosophical issues, extensively discussed in the methodology of science literature. For example, see O'Brien (1991). In addition, within economics, individual choice, social interaction and the non-experimental nature of observations further complicate the interaction between theory and observation and involves considerations not present in natural sciences. We focus on observations generated by the economy and do not discuss the recent work on "experimental economics". This section examines some of the main characteristics of economic observations and theories that lie at the root of the tension that exists between them. The main characteristic of different economic observations that we would like to emphasise here is their precision and the degree to which they correspond to the theoretical concepts. On the theory side it is their applicable domain and their relevance to decision making which are of concern. In the next section, we give examples of how these characteristics influence the impact of observation on theory. Here we wish to explain the characteristics themselves.

Theories come in a very wide range of forms. They differ in type (Marxist, monetarist, neo-classical, etc.); use of mathematics; aspects of the economy they are concerned with and so on. Theorists, however, tend to have certain characteristics in common. They tend to put a high value on rigour, generality and simplicity with a resulting preference for the abstract. In Marxist writings this abstraction is a major feature of the analysis because it allows one to distinguish between reality and the appearances which are observed. Marx in the Preface of Vol. I of Capital describes abstraction as the economist's substitute for the microscopes and chemical reactions used by physical sciences. This preference for abstraction is not confined to Marxists. For Hahn (1985) the primary purpose of theorising is to develop a framework to enhance "understanding", which he is at pains to distinguish from prediction of observables. Like Lucas (1980) he would probably explicitly reject the view that theory is a collection of assertions about the actual economy. But empirical work must involve turning theory into an assertion about the actual economy. The degree of applicability of the theory, the extent to which it makes assertions about observables, is an important characteristic. Theories which are more applicable are more likely to be influenced by observation.

A second aspect of theory which bears directly on the way it interacts with observation is the domain of its applicability. Theories with a wide domain of applications tend to be more sensitive to data. This sensitivity will be increased if the theories are of practical relevance or importance to decision makers. In areas like Finance, the theories tend to be highly applicable, the Black-Scholes option pricing formula diffused from the *Journal of Political Economy* to dealing rooms very rapidly, and has a wide domain of application, ranging from speculative asset markets, to investment decisions with irreversibilities. Business cycle theories of output, prices and employment, tend to be both applicable, and of interest to policy makers. Neo-classical general equilibrium theory at its most abstract level has a very low level of applicability and little direct interest to decision makers. Its innovations are conceptual and methodological and its interaction with observation depends largely on its implementation in other more applicable theories.

To say that certain types of theory are relatively insensitive to observation is not necessarily a criticism, given the methodological difficulties of relating theory and observation. Induction, the inference of general rules from particular observations, although extremely widespread raises logical difficulties. However many times a particular pattern has been observed in the past there is no logical justification for assuming that it will hold in the future. Statistical theory makes a set of assumptions, e.g. homogeneity of the data generating process through time, which allow probabilistic inference by assuming away the problem of induction. However, the probabilistic framework itself is a matter of substantial dispute (e.g. whether "empirical" probabilities should be interpreted as limits of relative frequencies or as personal estimates).

Falsification, the contradiction of a general rule by particular observations, while logically less fraught than induction, also faces major philosophical difficulties about whether the observation is valid or whether the theory has been contradicted. Of particular importance in economics is the "Duhem-Quine" problem. Any applicable theory is an inherently complex construct, made up of a large number of components including many auxiliary assumptions that enable it to be applied to particular cases. Should the theory be rejected by an observation, it is rarely clear which component of the theory is responsible for the rejection.

To confront the theory with the data, namely to construct models that relate theoretical concepts to their observable counterparts, requires numerous auxiliary assumptions. The econometrician must specify measurement models, dynamic adjustment processes, expectation mechanisms, and functional forms before the task of testing or evaluating theories can even begin. The models that the econometrician constructs and tests are often many steps removed from the theory model that the theorist has in mind. As a result there is a continuing tension between economic theory and econometric practice. The theorist is rarely content with the econometrician's choice of auxiliary assumptions, and the econometrician can always complain that the theorist's model is incomplete for the purpose of empirical analysis.

The validity of the observations is also central to the application of the falsification strategy in economics, but the nature of observations varies widely in economics. Economic data spans a wide spectrum of precision. At one end, one has direct observations of the outcomes of particular transactions, such as observed price and quantities in a market. In principle, these can be made as precise as one wishes and in financial markets, where large amounts of money can be made from arbitraging small differences in the price, the level of precision of observation is very high indeed.

At a middle level of precision are synthesised aggregates like national income, which are constructed within a whole set of theoretical measurement conventions. The imprecision arises primarily from two sources. Firstly, these measures are aggregated, thus involving a loss of information. The aggregation may be over time, products, or individuals, and in practice often involves all the three entities. Secondly, imprecision may result from imputation often necessitated due to non-market activities. In principle, national expenditure, measured as total marketed transactions could be measured with a high degree of precision, but it would not be very interesting. In practice, various outputs are either left out altogether (such as home production), or to get a closer correspondence to the measures of theoretical interest, some outputs are included using their imputed values (such as value of owner-occupier housing) which are often subject to a wide margin of errors. These inevitably result in imprecise measurements. In addition, there is a third source of measurement errors due to errors of sampling. The nature of this error, however, differs markedly from the other two (i.e. errors of aggregation and imputations) and its size can be controlled by increasing the proportion of population sampled, but of course, at a cost.

At the lowest level of precision are the measures of the unobservables which play such a large rôle in economic theory: the natural rate of unemployment, expected inflation, etc. As one moves along this spectrum the precision and the reliability of the observations declines as does their credibility, and thus the correspondence between observations and the theoretical concepts. But even when precise observations are available, their relationship to the theory may not be direct. When Moore, in 1914, observed a positive association between pig iron demand and its price, both relatively precisely measured, others rapidly disputed his interpretation of the observation as a positively sloped demand curve. Epstein (1987) and Morgan (1990) discuss this episode. As Morgan (1990) emphasises, identification is only one aspect of this more general correspondence problem.

III THE IMPACT OF OBSERVATION ON THEORY

From the broad discussion above we would expect the nature of observations to have a greater impact on theory, the more precisely they are measured, and the more closely they correspond to the theoretical concepts (which implies that the theory is highly applicable). One would also expect that, *ceteris paribus*, the impact of observations on theory will be greater in cases where the theory has a wide domain of applicability allowing more opportunities for the theory to be confronted with the data. Finance fits these characteristics quite closely and one can provide examples of how observations that were anomalous within the context of the theory rapidly prompted revisions.

Business cycle theory is another interesting case, theory and observation interact but in a complicated manner. The theory has wide applicability, many different types of observations are available but they lack precision and their correspondence with the theory can be a matter of dispute. In these circumstances preferences between theoretical and empirical criteria matter. Mankiw (1989, p. 89) comments "Yet like all optimising agents, scientists face trade-offs. One theory may be more 'beautiful' while another may be easier to reconcile with observation."

Akerloff (1984, p. 2) comments "The unwritten rules that only *economic phenomena* be considered in economic models with agents as individualistic selfish maximisers, restricts the range of economic theory and in some cases even causes the economics profession to appear peculiarly absurd — because, without relaxation of these rules, certain *almost indisputable* economic facts, such as the existence of involuntary unemployment, become inconsistent with economic theory." But the evidence of the business cycle literature is that the fact of involuntary unemployment is highly disputable, partly because measured unemployment is imprecisely observed and because it does not correspond to the theoretical concept and partly because many feel that relaxing those unwritten rules would logically undermine the coherence and achievements of large parts of economic theory.

Yet even in this area, the observations do have an impact. Barro (1989, p. 3) describes why the original New Classical "surprise" model was abandoned in favour of developing the Real Business Cycle models, which we discuss further below. As he notes, information lags did not seem, with hindsight, important; and the relation between price shocks and money supply surprises, and output or employment turned out to be weak or non-existent, and that it was difficult within this model to reproduce observed features of the economy such as the strong procyclical behaviour of investment and the fact that consumption and leisure (unemployment) tend to move in opposite directions. The theory was expanded to try to take account of these observations.

The observations that the theorists apprehend are not, in general, the test statistics of econometric work, but stylised facts which summarise well-established regularities, often themselves the results of much statistical and econometric work. Summers (1991, p. 129) argues "that formal econometric work where elaborate technique is used to apply theory to data or isolate the directions of causal relationships when they are not obvious a priori, virtually always fails", and advocates greater reliance on "pragmatic empirical" work. By pragmatic he means an approach which is easy to understand, simple to use, and focuses on stylised facts. Summers follows McCloskey (1985) in emphasising persuasiveness as the main criterion for the evaluation of empirical work. He cites the works of Friedman on the consumption function, Fama on the stock market, Solow and Dennison on growth and Phillips on wage determination as examples of pragmatic research. These exemplars, he claims, presented empirical regularities that were sufficiently clear cut that formal techniques were not necessary to perceive them. He explicitly rejects the whole basis of the Cowles Commission approach. "It is difficult to believe that any of the research described in this section would have been more convincing or correct if the author had begun by laying out some sort of explicit probability model describing how each of the variables to be studied should evolve within a specific pseudo world. Conversely, it is easy to see how a researcher who insisted on fully articulating a stochastic pseudo world before meeting up with the data would be unable to do most of the work described in this section" (Summers, 1991, p. 143). Later he comments that "macroeconomic theory is excessively divorced from empirical observation as a result of the failure of empirical work to deliver facts in a form where they can be apprehended by theory."

While we would agree with Summers' choice of influential empirical research, there are historical problems with his account. It is incorrect to claim that these exemplars did not use probabilistic models and, as the response to their research at the time attests, their results were regarded as anything but clear cut; and were accepted (if at all) as robust only after much subsequent econometric work. But more importantly, these examples can be read in almost exactly the opposite way, identifying a failure of theoretical rather than empirical work. The advantage these exemplars had was that at that stage in the development of the subject the theory delivered models that could be apprehended by data (it imposed restrictions on conditional distributions) which as we argue below is not always true of the new theory. A major task facing the applied econometrician today is to cast theory into a form in which the data can apprehend it. This problem is discussed in later sections after we have discussed some of the characteristics of empirical work.

IV EMPIRICAL RESEARCH

The applied econometrician synthesises theory, data, and statistical techniques into quantitative empirical models which can be used for particular purposes like forecasting, policy analysis or evaluating theoretical explanations. The ingredients for this synthesis are provided by other communities: the economic theorists who supply the formal framework; the historians and statisticians who supply the data; the econometric theorists who supply the statistical techniques; and the decision makers who define the scope and what is required of the models. These communities tend to be quite separate with different values. As we have argued above, theorists, for instance, value rigour, generality and simplicity even at the cost of explanatory power; decision makers value forecasting ability even at the expense of explanatory coherence. Some individuals do operate in more than one of these communities, but it is noticeable that they tend to use quite different styles of argument and rhetoric depending on which group they happen to be addressing.

Although the methodological difficulties discussed in Section II mean that it is probably impossible to test economic theories in any formal sense, it is possible, within some conventions about inference (e.g. the Neyman-Pearson framework of classical inference) to evaluate particular empirical models (based on theory and auxiliary assumptions) relative to alternatives. It does not seem possible to provide any absolute criteria that specify a good model. A model can only be evaluated relative to how well it does compared to an alternative and relative to a particular purpose.

The purpose of the model is crucial and so the evaluation must take account of multiple criteria. Models are deliberately simplified representations of reality constructed for particular purposes. Different purposes give rise to different models with different emphases and orientation. This is not confined to econometrics. "Models are used by engineers in three ways: (a) to summarise data for simulation purposes; (b) for explanatory purposes; and (c) for predictive purposes. These are quite different things; a summarising model, or simulator, may have no useful explanatory qualities, and a good explanatory model may have poor predictive qualities. Engineers need a multiplicity of models in their work" (MacFarlane, 1986, p. 143).

In an earlier paper we group these criteria under three broad headings: "relevance", "consistency", and "adequacy" which correspond to practice, theory and observation. (See Pesaran and Smith (1985)). The model should provide a reasonable characterisation of the data, i.e. be statistically adequate. It should be consistent with a priori knowledge (physical, institutional and historical). The model should be useful, i.e. relevant to a particular purpose (understanding, testing, forecasting, decision-making, etc.). Given that there are multiple criteria for evaluation of models, different people will choose different models according to their preference-ordering over the three criteria. In trying to both represent the theory and the observations, the form of the statistical model used plays a crucial part. In the next section, we shall examine the evolution of the interaction between theory and data in terms of the developments in the use of particular statistical models, since the seminal work of Haavelmo and the Cowles Commission.

V STATISTICAL MODELS

Haavelmo said "Econometric research aims, essentially, at a conjunction of economic theory and actual measurements, using the theory and technique of statistical inference as a bridge pier" quoted in Morgan (1990, p. 242). Econometric analysis in the post war period has been dominated by four statistical techniques or models and quite different ways have evolved of relating them to the theory and to each other. The four models are the single equation and the multivariate regression models, the univariate ARIMA models, and the VAR.

The period up to the 1970s was dominated by single equation and multivariate regression models. In the regression model the conditional mean of some endogenous variable is explained in terms of a certain set of exogenous variables. Regression was the basis of what Hylleberg and Paldam (1991) call the "traditional strategy" of doing empirical research: of marrying theory and observations. This "traditional strategy" emerged from the work of Tinbergen, Haavelmo and the Cowles Commission. Central to it was a dichotomy between theoretical and empirical activities: the theorist provided the model and the econometrician estimated and tested it. This proved a highly productive strategy which dominated empirical econometrics until the 1970s and still remains healthy. It was effective because the theory involved, (IS-LM, static demand theory, explanations of cycles in terms of stochastic linear difference equations) could easily be cast in the form of a linear or simple non-linear regression. The primary role of what might be called "old" theory in this context was "identifying the list of relevant variables to be included in the analysis, with possibly the plausible signs of their coefficients" (Tinbergen, 1939), though it also suggested linear or non-linear parametric restrictions, such as homogeneity with respect to prices, which could be tested.

The old theory primarily focused on conditional statements, such as what would happen to demand if prices were to fall; decision makers focused on conditional predictions, such as what would happen to unemployment if government spending were to increase. Regression methods, by estimating the conditional means, provided a flexible way of quantifying and testing qualitative statements about conditional moments. The testing was usually of a limited, though useful sort: was the effect significant and of the correct sign? In the pragmatic application of the traditional strategy, though not in the strict Cowles Commission view, regression also allowed the empirical analyst great scope to make auxiliary assumptions: add variables to allow for *ceteris paribus* conditions, choose different functional forms, add lags for adjustment processes and experiment with proxies for unobservables. In terms of the "Duhem-Quine problem" the theory became almost unfalsifiable: it was never clear whether the theoretical core or the auxiliary assumptions were rejected. However, this approach allowed the empirical analyst to take account of a wide range of historical, institutional and physical constraints. This increased the applicability of the theory and allowed the model to better represent the data while remaining consistent with theory.

The multivariate regression model explains a number of endogenous variables by the same vector of exogenous variables. The reduced form of a linear simultaneous equations model was of this form and the role of theory was then to provide the identifying restrictions that allowed the structural form to be estimated plus over-identifying restrictions that could be tested. Complete systems of demand equations, which were developed following Stone (1954), also took the form of multivariate regression models. In this case, the theory imposed a set of restrictions on the system (adding up, homogeneity, symmetry and negativity) which could be used to improve the efficiency of estimation or be tested.

The Cowles Commission approach was characterised by the development of estimators rather than test procedures, see Qin (1991). Even without formal procedures for diagnostic and misspecification testing, the explanation (conditional prediction) provided by the model could be compared with the realisations, allowing an informal judgement of statistical adequacy.

The univariate ARIMA (p, d, q) model, represents a single variable (which has been differenced sufficiently, say d times, to induce stationarity) in terms of p lagged values of itself and a moving average of q lagged disturbances. Although these models were initially "atheoretical" using no information from economic theory, there were cases where economic theory did impose restrictions on the form of an ARIMA model. For instance, efficient market theory, in its simple form, predicted that speculative asset prices should be random walks: ARIMA (0,1,0). However, during the 1970s it became apparent that univariate ARIMA models could out-perform traditional econometric regression models in forecasting. This led to an increased emphasis on developing measures of model adequacy, a proliferation of diagnostic and misspecification tests and a shift away from emphasis on the estimation of a theoretical model and towards model specification. However, at the same time, there were complaints by theorists that traditional regressions did not represent the theory, and by decision makers that the models were ineffective for practical purposes of forecasting and policy analysis. In terms of our earlier criteria, the models were seen as statistically inadequate, theoretically inconsistent and practically irrelevant.

The response of many econometricians to the evidence that simple time-series models could, on occasion, produce better forecasting performance than econometric models was to put much greater priority on representing the data relative to the theory, which they initially saw as having relatively little to contribute, particularly for the purpose of forecasting and business-cycle research. If ARIMA models out-performed traditional econometric models, then econometric models needed to take account of both the information in the ARIMA models and the linkages between variables embodied in econometric models but ignored by univariate time-series models. There were two strands to this response: dynamic elaboration of single equation regression to produce the Error Correction Models associated with Hendry and his colleagues, and the use of a multivariate time-series model, the Vector Autoregression (VAR), associated with Sims (1980). These two approaches, the VAR and the ECMs, can be combined in the cointegration approach, (Engle and Granger, 1991). In both cases the initial impetus to the research programme was statistical, a desire to provide statistically adequate representation of the data and to forecast more accurately.

Hendry and his colleagues in the LSE tradition, e.g. see Hendry (1987), started from a general Autoregressive Distributed Lag model, which explained an endogenous variable by its own lags and current and lagged exogenous variables; effectively using a moving average of observed regressors rather than unobserved disturbances as in the ARIMA. The estimated model was then subjected to a battery of tests to ensure that it described the data adequately and then simplified by reparameterisation and restrictions which reduced the number of estimated coefficients. The end result was usually a single equation Error Correction Model in which the changes in the dependent variable was explained by changes in the independent variable and lagged levels of the dependent and independent variables. Alogoskoufis and Smith (1991) discuss error correction models in more detail.

The textbook by Spanos (1986) provides an influential exposition of this methodology. Spanos (p. 10) notes the separation of time-series modelling from mainstream econometric modelling, and says that one of the main aims of his book is to complete the convergence between the two strands that began in the mid 1970s. But in this convergence, priority is given to the development of a well defined statistical model which adequately describes the observed data in the sense that the underlying statistical assumptions are not grossly violated. Theory enters at the first stage, with choice of the variables examined as with Sims, and at the final stage when the estimated statistical model can be reparameterised or restricted in view of the theory so that the model can be expressed in terms of the theoretical parameters of interest (e.g. p. 699). "Econometric modelling is viewed not as the estimation of theoretical relationships nor as a procedure for establishing the 'trueness' of economic theories, but as an endeavour to understand observable economic phenomena using observed data in conjunction with some underlying theory in the context of a statistical framework" (Spanos, 1986, pp. 670-671).

In an application of this methodology to Friedman and Schwartz's analysis of money demand Hendry and Ericsson say: "Modelling is seen as an attempt to characterise data properties in simple parametric relationships that are interpretable in the light of economic knowledge, remain reasonably constant over time, and account for the findings of pre-existing models." (Hendry and Ericsson 1991, p. 18). The methodology is based on the statistical theory of data reduction. They suggest six criteria, of which five are statistical and one is "Theory Consistency". To Hendry and Ericsson the most important rôle that theory can have in the empirical research is the specification of long-run relationships. The response to Hendry and Ericsson's (1991) criticisms by Friedman and Schwartz (1991, p. 49), emphasises the difference in purpose. "By HEs [Hendry and Ericsson's] standards, the prior 281 pages of our book were mostly worthless.... Those pages were not devoted à la HE, to "representing the joint density of [a limited set of variables] in terms of an autoregressive distributed lag model," then proceeding to simplify "[t]he conditional model to an ECM," and to evaluating it "in the light of the model design criteria" listed in their Table 2 (HE, pp. 22-23). Instead, the first 204 of those 281 pages present our theoretical framework, our statistical framework, the basic data, and an overview of the movements of money, income, and prices over the century our data cover." Friedman and Schwartz like Mayer (1980) emphasise the importance of explaining a wider range of observations than the particular sample being analysed.

Whereas the LSE tradition largely worked within a single equation framework, the other approach to combining econometric and time-series models was multivariate. Full multivariate vector ARIMA models tend to be intractable and Sims (1980), within an explicitly atheoretical approach, advocated a simplification of the VARMA model, the Vector Autoregression, VAR. The VAR is the fourth of the statistical models that has been widely adopted in econometrics. In this structure each variable (measured either in levels or first differences) is treated symmetrically, being explained by lagged values of itself and other variables in the system. There are no exogenous variables, no identifying conditions and the only rôle of theory is to specify the variables included. Cooley and LeRoy (1985) provide a critique of such atheoretical econometrics.

But the VAR was not necessarily atheoretical, it could provide a statistical framework within which the restrictions imposed by theoretical models could be imposed. One route was to use the VAR as the reduced form of a traditional structural model. Then the specification of the structural model could be tested by imposing the sequential restrictions necessary to generate it from a VAR: pre-determinateness of some variables; non-causality; exogeneity; and weak and strong over-identification conditions. For an implementation of such a sequential testing procedure in the context of the VAR, see Monfort and Rabemananjara (1990).

An alternative route used theoretical linear rational expectations or equilibrium models as a way of interpreting and imposing cross-equation restrictions on vector autoregressions. "Rational expectations modelling promised to tighten the link between theory and estimation, because the objects produced by the theorizing are exactly the terms of which econometrics is cast, e.g. covariance generating functions, Markov processes and ergodic distributions." (Hansen and Sargent 1991, p. 2). Within this framework the aim is to estimate the "deep" parameters (of taste and technology) by exploiting the cross-equation restrictions the theory imposes on the parameters of the VAR.

However, this could only be done for optimising models which take what Whittle (1982) calls the "LQ form": linear constraints with quadratic objective functions. The details of this optimisation problem have been extensively developed in the Operations Research Literature and widely applied in many fields beside economics. The decision rules take the form of a linear VAR. More complicated models of stochastic dynamic optimisation could not be solved analytically and real business cycle theorists had to face the difficulty that analytical solutions for the decision rules of their models under uncertainty were rare. Partly as a result of the difficulties involved in estimating these models they adopted the explicitly astatistical approach of calibrating and simulating the theoretical models, e.g. Kydland and Prescott (1982, 1991). Although Kydland and Prescott do not use methods of statistical inference, they regard these procedures as econometric, in the spirit of some of Frisch's exercises. Andersen (1991) provides a critique of the calibration approach. This astatistical response is strongly identified with the Lucas-Sargent research programme centred on a stochastic dynamic optimisation approach. This approach requires that all the behavioural relations of the model be obtained directly from the solutions to well defined dynamic optimisation problems faced by economic agents, usually taken to be representative agents. In order to make this approach operational a large number of very restrictive assumptions have to be made about preferences, technology, endowments and information sets. The proponents of this approach are forced to use very simple functional forms; rely almost exclusively on the concept of representative agents with homogeneous information (which as Arrow (1986) points out is odd in any explanatory model of decentralised markets where individual differences are the prime motivation for trade); and give little or no consideration to institutional constraints.

This means that a number of important problems, such as information heterogeneity, sectoral disaggregation and choice of functional forms that concern applied economists are either ignored or brushed aside.[1] They are ignored not because they are unimportant but because they cannot be readily accommodated within the optimisation framework. Thus the theory becomes a straitjacket rather than a flexible framework for enquiry. This approach shifts the emphasis to the model of the economy rather than the economic reality itself. As Sargent states "The internal logic of general equilibrium modelling then creates a difficulty in taking any of the model's predictions seriously" (Sargent, 1987, p. 7). Kydland and Prescott (1991, p. 169) say "Without some restrictions, virtually any linear stochastic process on the variables can be rationalised as the equilibrium behaviour of some model economy in this class. The key econometric problem is to select the parameters for an experimental economy". This is not the traditional definition of an econometric problem. The parameters of these models can be consistently estimated by Generalised Method of Moments (GMM) or Simulated GMM, conditional on the assumption that the model is correct. However, estimation, in itself, does not generate conditional predictions, time paths for the endogenous variables, which can be compared with the actuals to assess the explanatory power of the models. Canova, Finn and Pagan

1. The problem of information heterogeneity in econometric applications is discussed by Townsend (1983) and Pesaran (1987, 1990b).

(1992) solve a simple Real Business Cycle to provide such conditional predictions.

One important rôle of economic theory is to produce general, unifying insights which promote our understanding of the work of the economic system by abstracting from the complex mass of details which constitute the "reality", thus allowing the theorist to provide tractable analysis. The usefulness of any abstraction depends on whether it opens rather than closes doors, that is whether it enables the theorist to gain a deeper understanding of a wider range of interconnected phenomena. The theory also acts as a unifying framework within which new results can be related to what is already known. Continued adherence to the Rational Expectations Hypothesis is now closing rather than opening doors inhibiting for instance the study of how agents' learning processes may form part of a history-dependent process which allows a determinate equilibrium to be singled out from the multiplicity of the equilibria which obtain in general equilibrium models. When dynamic stochastic models are estimated, the assumptions of the model, required for a tractable solution, are so restrictive that the results are often uninformative. This is a criticism that Summers (1991) makes against the work of Hansen and Singleton (1982, 1983).

VI AN ALTERNATIVE APPROACH

The question is how can we devise a procedure that incorporates the precision of the modern dynamic stochastic theory and the flexibility of the traditional approach. The aim would be to develop a general econometric framework which enjoys the precision of the dynamic optimisation approach but does not suffer from its formal stricture when applied literally to economic problems. The straitjacket of the stochastic dynamic optimisation that, given current computer technology, only allows consideration of the simplest cases needs to be avoided if at all possible.

One possibility would be to articulate the use of shadow prices as an intermediate step to simplify empirical analysis of models derived from applications of the dynamic optimisation. The concept of shadow prices has a long history in economics and naturally arises in optimising problems subject to constraints. In many applications these shadow prices directly correspond to prices of goods or services in particular future or current markets that, for one reason or another, do not exist. A great deal of the complexity of the dynamic optimisation approach is due to missing markets that render the shadow prices unobservable. Consider the case of investment, which is set out more formally in the Appendix. The optimisation problem gives rise to a Lagrange multiplier, the shadow price of capital. Jorgenson's (1963) classic study made the assumption that there were complete second-hand markets for capital goods. Then the shadow price was the user cost of capital. Hayashi (1982) showed that the shadow price was directly related to what he defined as Tobin's marginal q. He then established the very strict conditions under which the marginal q would equal the average q. Abel and Blanchard (1986) tried to obtain measures of marginal q directly. Other examples where the use of shadow prices can be used to provide a bridge between theory and application are discussed in Pesaran and Smith (1992), where the analysis of consumption under liquidity constraint, and oil production are given.

In an Arrow-Debreu world there are complete markets for all current, contingent and forward contracts. The widespread absence of forward contracts means that agents have to condition their decisions not on the known forward price but on their expectations of the price in the future. Economists have dealt with this problem by replacing the unobservable (to the econometrician) expectation of the future price by its observed determinants. The equally widespread absence of current (e.g. contracts for second-hand capital goods) and contingent (e.g. contracts conditional on the agent being liquidity constrained) markets means that agents have to condition on unobservable (to the econometrician) shadow prices. Such is the case with the shadow price of capital in the investment example.

Our proposed empirical approach set out with more mathematical details in Pesaran and Smith (1992), involves replacing the unobservable shadow prices by linear or simple non-linear functions of the observable state variables which determine them. This procedure maintains the structure of dynamic optimisation but allows other relevant institutional (taxation and ownership rights) and physical constraints (e.g. the exhaustibility of oil reserves as in Pesaran (1990a) or the constraints on installing or disposing of capital stock) to enter the problem through their influence on the shadow prices. Thus, it provides a consistent theoretical way to enter other relevant factors not explicitly treated by the theory, but which constrain the optimising behaviour of economic agents. This is in accord with a rich tradition of using shadow prices in economics to encapsulate the information needed by decision makers when markets do not exist. For an early example of this see Sen's (1960) work on the Choice of Techniques. This framework also opens an avenue of discourse with the theorist, since the empirical significance of problems such as liquidity constraints can be presented more readily in theoretical terms.

VII CONCLUDING REMARKS

Evaluation of theories necessarily involves confrontation of the theories' predictions with the evidence. In economics the relevant predictions concern the conditional distributions of the observables. This is not, however, a sufficient condition for at least two reasons. Firstly, there is the problem of inference, that there is no agreed method of judging whether the conditional predictions match the data and thus whether the evidence rejects the theory. Secondly, the conditional predictions result from the conjunction of the theory and the auxiliary assumptions required to produce an empirical model, and it is not clear which is being rejected. These two problems are sufficiently serious that it seems unlikely that economic theories can be tested. However, within an agreed procedure for inference it may be possible to judge whether the conditional predictions of a particular empirical model, which embodies the theory, do in fact match the data better than those of another rival model.

With theory that can be cast in the LQ form this is relatively straightforward and theory and evidence can be related in the traditional way. Linear constraints and quadratic objective functions provide a good approximation to a very wide variety of problems. However, there are a range of important cases where it does not. The adequacy of the LQ approximation depends on the relative stability of the underlying parameters and some notion of differentiability of the constraints and the objective functions. But there are many interesting economic phenomena where boundary conditions become operative, such as the non-negativity of prices for outputs and factor inputs, or where there are assymetric adjustment costs, bankruptcies and irreversibilities. For these problems the LQ form may provide a poor approximation. In addition, the approximation process involved in the linearisation means that the estimated parameters cannot be related directly to the deep parameters of the original, non-linear, structural model. With this form of theory, involving stochastic, non-linear dynamic optimisation with incomplete markets, comparing predictions of the theory with the evidence ceases to be a straightforward matter. The theory faces the danger of becoming a straitjacket because the models cannot be readily solved to provide predictions for observed data, except in the simplest cases, and also because the models cannot be easily extended to incorporate other prior information about physical or institutional features of the problem. As we described, the professional response to this tension between the theory and the evidence has been either in the direction of "atheoretical" empirical research using VAR's, or towards "astatistical" approaches by resorting to calibration techniques or reliance on simple "stylized" facts. This is unsatisfactory. Theory is essential in enabling us to organise our a priori knowledge about the problem in a consistent and coherent way. But the predictions of the theory must also be confronted with the data, at least indirectly via a particular empirical model, if it is to have any relevance and to enhance our understanding of the real life problems.

In the previous section we suggest an approach which might help bridge the current gulf between theory and evidence. This approach allows the theoretical analysis to be conducted outside the LQ framework, but at the same time aims to avoid the computational and estimation problems involved, by replacing the unobserved Lagrange multipliers or shadow prices by functions of the variables that determine them. The resulting models although non-linear, can be solved to give conditional predictions of the observables. They can be solved because although they maintain the intrinsic non-linearity (e.g. associated with whether a constraint binds or not) they approximate the incidental non-linearity in the determination of the shadow prices with linear or other tractable functions. As a result they can be estimated without too much difficulty. This approach, furthermore, we would hope has the potential not only to improve estimation and prediction but also to improve the dialogue between theory and evidence. As we discussed above, a major source of tension between theory and evidence arises because the theorist and the econometrician have different purposes in mind. The prime objective of the empirical worker is to explain the data, albeit within a theoretical framework which provides consistency and coherence. This is not the prime objective of the theorist. Being able to present the evidence in theoretically coherent terms — shadow prices — rather than as the parameters of conditional distributions may aid the dialogue by delivering "facts in a form where they can be apprehended by theory" to adopt the phrase used by Summers.

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APPENDIX

The Shadow-Price of Capital

Consider a firm acting to maximise the present value of its expected net receipts

$$V_t = E_{t-1}\left\{\sum_{\tau=0}^{\infty}\beta^{\tau}[p_{t+\tau}Y_{t+\tau} - w_{t+\tau}L_{t+\tau} - \tilde{p}_{t+\tau}\, I_{t+\tau}\right\}, \tag{A.1}$$

where E_{t-1}, stands for the expectations operator conditional on information available at t–1, $\beta=1/(1+r)$, with r, the discount rate, assumed to be fixed, p_t is the output price, Y_t the quantity of output produced by firm during period t, w_t is the wage rate and L_t employment. $\tilde{p}_t$ is the price and I_t the quantity of investment. For expositional simplicity all taxes are ignored. The value function V_t is maximised subject to the production function

$$Y_t = F(K_t, L_t, t), \tag{A.2}$$

and the state equation determining capital stock, K_t:

$$K_t = G_t(I_t, K_{t-1}) + (1 - \delta)K_{t-1}. \tag{A.3}$$

The function G_t models installation, disposal and other related adjustments costs which are involved in translating real investment expenditures into net additions to the capital stock, and δ is the rate of capital depreciation. This formulation can be viewed as a discrete time analogue of Hayashi's (1982) continuous time formulation with explicit treatment of uncertainty and expectations. The present formulation does not, however, take account of the non-negativity constraint on capital stock, but is capable of capturing asymmetric response of capital stock to investment and the non-negativity of real investment, an important feature of the investment decision that the Jorgenson model does not take into account.

Invoking the Maximum principle discussed, for example, in Whittle (1982), the constrained maximisation problem (A.1)–(A.3) can be solved by the unconstrained maximisation of

$$H_t = E_{t-1}\left\{\sum_{\tau=0}^{\infty}\beta^{\tau}\, h_{t+\tau}\right\}, \tag{A.4}$$

with respect to the decision and state variables, $L_{t+\tau}$, $I_{t+\tau}$,, $K_{t+\tau}$, τ = 0,1,2,..., where

$$h_t = p_t Y_t - w_t L_t - \tilde{p}_t I_t - \lambda_t [K_t - (1-\delta)K_{t-1} - G(I_t, K_{t-1})], \tag{A.5}$$

where λ_t, as it will become clear below, can be interpreted as the shadow price of capital. Maximisation of (A.4) gives the following first order conditions for the current period variables (i.e. where $\tau = 0$).

$$E_{t-1}\left\{\frac{\partial Y_t}{\partial L_t} p_t - w_t\right\} = 0, \tag{A.6}$$

$$E_{t-1}\left\{-\tilde{p}_t + \lambda_t \frac{\partial G_t}{\partial I_t}\right\} = 0, \tag{A.7}$$

$$E_{t-1}\left\{p_t \frac{\partial Y_t}{\partial K_t} - \lambda_t + \beta\left[(1-\delta) + \frac{\partial G_{t+1}}{\partial K_t}\right]\lambda_{t+1}\right\} = 0. \tag{A.8}$$

Equation (A.6) says that the expected value of the marginal product of labour equals the expected wage rate.

Equation (A.7) can be written as

$$E_{t-1}(\lambda_t) = E_{t-1}(\tilde{p}_t) \Big/ \frac{\partial G_t}{\partial I_t}, \tag{A.9}$$

giving the expected shadow price of capital. In the Jorgenson model, Equation (A.3) is simply

$$K_t = I_t + (1-\delta)K_{t-1},$$

thus $\frac{\partial G_t}{\partial I_t} = 1$ and $\frac{\partial G_t}{\partial K_{t-1}} = 0$. Therefore, the expected shadow price of capital is just the expected price of investment goods, namely:

$$E_{t-1}(\lambda_t) = E_{t-1}(\tilde{p}_t) = \tilde{p}_t^e. \tag{A.9a}$$

In the present case, (A.8) simplifies to

$$E_{t-1}\left\{p_t \frac{\partial Y_t}{\partial K_t} - \lambda_t + \beta(1-\delta)\lambda_{t+1}\right\}, \tag{A.8a}$$

$$E_{t-1}\left(p_t \frac{\partial Y_t}{\partial K_t}\right) = E_{t-1}\{c_t\},$$

where

$$c_t = \lambda_t - \beta(1-\delta)\lambda_{t+1}.$$

Using (A.9a) and noting that

$$E_{t-1}(\lambda_{t+1}) = E_{t-1}(\tilde{p}_t),$$

we have

$$E_{t-1}(c_t) = E_{t-1}\left[\tilde{p}_t\left(1-\beta(1-\delta)\frac{\tilde{p}_{t+1}}{\tilde{p}_t}\right)\right]$$

$$= E_{t-1}\left\{\tilde{p}_t\left[1-\frac{1-\delta}{1+r}(1+\tilde{\pi}_{t+1})\right]\right\}$$

where $\tilde{\pi}_{\tau+1}$ is the rate of inflation of investment goods prices. Hence

$$E_{t-1}(c_t) = E_{t-1}\left\{\tilde{p}_t\left(\frac{r+\delta+(1-\delta)\tilde{\pi}_{t+1}}{1+r}\right)\right\}.$$

This corresponds to what Jorgenson (1963, p. 249) describes (in the certainty case) as the shadow price, or implicit rental of one unit of capital services per period of time and refers to it as the "user cost of capital".

Hayashi (1982) defines Tobin's "marginal q" as $q_t = \lambda_\tau/\tilde{p}_t$, which under uncertainty can be written as $q_t = \lambda_t \,/\, E_{t-1}(\tilde{p}_t) = \lambda_t \,/\, \tilde{p}_t^e$. Tobin's average q is defined by

$$Q = \frac{V_t}{\tilde{p}_t K_{t-1}}.$$

Now divide both sides of Equation (A.8) by $\tilde{p}_t^e$

$$E_{t-1}\left[\frac{p_t}{\tilde{p}_t^e}\frac{\partial Y_t}{\partial K_t} - \frac{\lambda_t}{\tilde{p}_t^e} + \beta\frac{\lambda_{t+1}}{\tilde{p}_t^e}\left(1-\delta+\frac{\partial G_{t+1}}{\partial K_t}\right)\right] = 0,$$

which under certainty corresponds to Equation (A.9′) of Hayashi (1982, p. 217). Once the installation function is known, the optimal rate of investment is merely a function of the expected shadow price of capital and the expected price of investment goods. Hayashi then goes on to show that under certain very strong conditions, the marginal q is related to the average Q. However, an alternative approach would be simply to approximate $E_{t-1}(\lambda)_t)$ in terms of the observables of the system and substitute these for the expected shadow prices as discussed in the text.