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Economic Theory and Econometric Models

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The constitution of the Econometric Society states as the main objective of the society "the unification of the theoretical-qualitative and the empirical-quantitative approach to economic problems" (Ragnar Frisch, 1933, p. 1). Explaining this objective, Frisch warned that "so long as we content ourselves to statements in general terms about one economic factor having an 'effect' on some other factor, almost any sort of relationship may be selected, postulated as a law, and 'explained' by a plausible argument". Precise, realistic, but at the same time complex theory was required to "help us out in this situation" (*ibid*, p. 2).

Over fifty years after the foundation of the Econometric Society Hashem Pesaran stated, in an editorial which appeared in the initial issue of the *Journal of Applied Econometrics*, that "Frisch's call for unification of the research in economics has been left largely unanswered" (Pesaran, 1986, p. 1). This is despite the fact that propositions that theory should relate to applied models, and that models should be based upon theory, are not enormously controversial. The simple reason is that economic theory and econometric models

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are relatively awkward bedfellows – much as they cannot do without each other, they also find it very difficult to live in the same house. I shall suggest that this is in part because the proposed union has been over-ambitious, and partly for that reason, neither partner has been sufficiently accommodating to the other. What is needed is the intellectual analogue of a modern marriage.

One could look at this issue either in terms of making theory more applicable, or in terms of making modelling more theoretically-based. I shall start from the latter approach. What I have to say will therefore relate to controversies about the relative merits of different econometric methodologies, and it is apparent that increased attention has been given by econometricians to questions of methodology over the past decade. In particular, the rival methodologies associated respectively with David Hendry, Edward Leamer and Christopher Sims, and recently surveyed by Adrian Pagan (1987), generate showpiece discussions at international conferences. I do not propose here either to repeat Pagan's survey, or to advise on the "best buy" (on this see also Aris Spanos, 1988). A major issue, which underlies much of Leamer's criticism of conventional econometrics, is the status of inference in equations the specification of which has been chosen at least in part on the basis of preliminary regressions. I do not propose to pursue those questions here. I also note the obvious point that the tools we use may be in part dictated by the job to hand – hypothesis testing, forecasting and policy evaluation are different functions and it is not axiomatic that one particular approach to modelling will dominate in all three functions. This point will recur, but I wish to focus on two specific questions – how should economic theory determine the structure of the models we estimate and how should we interpret rejection of theories? I will argue that in the main we should see ourselves as using theory to structure our models, rather than using models to test theories.

As I have noted, Frisch was adamant that economic data were only interesting in relation to economic theory. Nevertheless, there was no consensus at that time as to how data should be related to theory. Mary Morgan (1987) has documented the lack of clear probabilistic foundation for the statistical techniques then in use. To a large extent this deficiency was attributable to Frisch's hostility to the use of sampling theory in econometrics. Morgan argues that econometrics was largely concerned with the measurement of constants in lawlike relationships, the existence and character of which were not in doubt. She quotes Henry Schultz (1928, p. 33), discussing the demand for sugar, as stating "All statistical devices are to be valued according to their efficacy in enabling us to lay bare the true relationship between the phenomena under question", a comment that is remarkable only in its premise that the true relationship is known. The same premise is evident in Lionel Robbins' strictures on Dr Blank's estimated demand function for herrings (Robbins,

1935) – Robbins does not wish to disagree with Blank on the form of the economic law, but only on the possibility of its quantification.

This all changed with the publication in 1944 of Trygve Haavelmo's "Probability Approach in Econometrics" (Haavelmo, 1944). Haavelmo insisted, first, that "no tool developed in the theory of statistics has any meaning – except, perhaps, for descriptive purposes – without being referred to some stochastic scheme" (*ibid*, p. iii); and second, that theoretical economic models should be formulated as "a restriction upon the joint variations of a system of variable quantities (or, more generally, 'objects') which otherwise might have any value or property" (*ibid*, p. 8). For Haavelmo, the role of theory was to offer a non-trivial, and therefore in principle rejectable, structure for the variance-covariance matrix of the data. In this may be recognised the seeds of Cowles Commission econometrics.

Haavelmo was unspecific with respect to the genesis of the theoretical restrictions, but over the post-war period economic theory has been increasingly dominated by the paradigm of atomistic agents maximising subject to a constraint set. It is true that game theory has provided a new set of models, although these game theoretic models typically imply fewer restrictions than the atomistic optimisation models which remain the core of the dominant neoclassical or neo-Walrasian "research programme". This research programme has been reinforced by the so-called "rational expectations revolution", and in Lakatosian terms this provides evidence that the programme remains "progressive".

I shall briefly illustrate this programme from the theory of consumer behaviour on the grounds that it is in this area that the econometric approach has been most successful; and also because it is an area with which all readers will be very familiar. The systems approach to demand modelling was initiated by Richard Stone's derivation of and estimation of the Linear Expenditure System (the LES; Stone, 1954). The achievement of the LES was the derivation of a complete set of demand equations which could describe the outcomes of the optimising decisions of a utility maximising consumer. We now know that the functional specification adopted by Stone, in which expenditure is a linear function of prices and money income, is highly restrictive and entails additive separability of the utility function; but this is in no way to devalue Stone's contribution.

Over the past decade, much of the work on consumer theory has adopted a more general framework in which agents maximise expected utility over an uncertain future. This gives rise to a sequence of consumption (more generally, demand) plans, one starting in each time period, with the feature that only the initial observation of each plan is realised. A common procedure, initiated in this literature by Robert Hall (1978), but most elegantly carried out by Lars

Peter Hansen and Kenneth Singleton (1983), is to estimate the parameters of the model from the Euler equations[1] which link the first order conditions from the optimisation problem in successive periods. The combination exhibited both in these two recent papers and in the original Stone LES paper of theoretical derivations of a set of restrictions on a system of stochastic equations, and the subsequent testing of these restrictions using the procedures of classical statistical inference, is exactly that anticipated both in the constitution of the Econometric Society and in Haavelmo's manifesto. It therefore appears somewhat churlish that the profession, having duly arrived at this destination, should now question that this is where we wish to go. However, in terms of the constitutional objective of unifying theoretical and empirical modelling, the Haavelmo-Cowles programme forces this unification too much on the terms of the theory party.

This argument can be made in a number of respects, but almost invariably will revolve around aggregation. I shall consider two approaches to aggregation – one deriving from theoretical and the other from statistical considerations.

The three consumption-demand papers to which I have referred share the characteristic that they use aggregate data to examine the implications of theories that are developed in terms of individual optimising agents. This requires that we assume either that all individuals are identical and have the same income, or that individuals have the same preferences (although they may differ in the intercepts of their Engel curves) which, moreover, belong to the PIGL class.[2] In the latter and marginally more plausible case, the market demand curves may be regarded as the demand curves of a hypothetical representative agent maximising utility subject to the aggregate budget constraint. This allows interpretation of the parameters of the aggregate functions in terms of the parameters of the representative agent's utility function.

The representative agent hypothesis therefore performs a "reduction" of the parameters of the aggregate function to a set of more fundamental micro parameters. In this sense, the aggregate equations are "explained" in terms of these more fundamental parameters, apparently in the same way that one might explain the properties of the hydrogen atom in terms of the quantum mechanics equations of an electron-proton pair. This reductionist approach to explanation was discussed by Haavelmo in an important but neglected section of his manifesto entitled "The Autonomy of an Economic Relation" and which anticipates the Lucas critique (Robert Lucas, 1976).[3] A relation-

1. Or orthogonality conditions implied by the Euler equations. A difficulty with these "tests" is that they may have very low power against interesting alternatives – this argument was made by James Davidson and David Hendry (1981) in relation to the tests reported in Hall (1978).

2. The Price Independent Generalized Linear class – see Angus Deaton and John Muellbauer (1980).

3. See also John Aldrich (1989).

ship is autonomous to the extent that it remains constant if other relationships in the system (e.g., the money supply rule) are changed. Haavelmo explains "In scientific research – in the field of economics as well as in other fields – our search for 'explanations' consists of digging down to more fundamental relations than those which stand before us when we merely 'stand and look'" (*ibid*, p. 38).

There is however abundant evidence that attempts to make inferences about individual tastes from the tastes of a "representative agent" on the basis of aggregate time series data can be highly misleading. Thomas Stoker (1986) has emphasised the importance of distributional considerations in a micro-based application of the Linear Expenditure System to US data and Richard Blundell (1988) has reiterated these concerns. Richard Blundell, Panos Pashardes and Giullimo Weber (1988) estimate the Deaton and Muellbauer (Deaton and Muellbauer, 1980) Almost Ideal Demand System on both a panel of micro data and on the aggregated macro data. They find substantial biases in the estimated coefficients from the aggregate relationships in comparison with the microeconometric estimates. Furthermore, and, they suggest as a consequence of these biases, the aggregate equations exhibit residual serial correlation and reject the homogeneity restrictions. They suggest, as major causes of this aggregation failure, differences between households with and without children and the prevalence of zero purchases in the micro data.[4] This study in particular suggests that it is difficult to claim that aggregate elasticities correspond in any very straightforward way to underlying micro-parameters.

How does this leave the reductionist interpretation of aggregate equations? Pursuing further the hydrogen analogy, the demand theoretic "reduction" is flawed by the fact that we have in general no independent knowledge of the parameters of individual utility functions which would allow us to predict elasticities prior to estimation. It is therefore better to see utility theory in traditional studies as "structuring" demand estimates. In that case, the representative agent assumption is a convenient and almost Friedmanite simplifying assumption,[5] implying that the role of theory is primarily instrumental.

In the Stoker (1986) and Blundell *et al.* (1988) studies, by contrast, the presence of good micro data allow a genuine test of the compatibility of the macro and micro estimates, and this allows those authors to investigate the circumstances in which a reductionist interpretation of the macro estimates

4. This problem is even more severe in the recent study by Vanessa Fry and Panos Pashardes (1988) which adopts the same procedure in looking at tobacco purchases. Clearly, the prevalence of non-smokers implies that there is no representative consumer. But it is also the case that elasticities estimated from aggregate data may fail to reflect the elasticities of any representative smoker. This is because it is not possible to distinguish between the effect of a (perhaps tax-induced) price rise in inducing smokers to smoke less and the effect, if any, of inducing smokers to give up the habit.

5. See Milton Friedman (1953); and for a recent summary of the ensuing literature John Pheby (1988).

is possible. Both papers conclude that aggregate estimates with no corrections for distributional effects tend to suffer from misspecified dynamics and this does imply that they will be less useful in forecasting and policy analysis. There is also a suggestion that they may vary significantly, in a Lucas (1976) manner, because government policies will affect income distribution more than they will affect household characteristics and taste parameters. Although neither set of authors makes an explicit argument, the implication appears to be that one is better off confining oneself to microeconometric data. This view seems to me to be radically mistaken.

It has been clear ever since Lawrence Klein (1946a, b) and Andre Nataf (1948) first discussed aggregation issues in the context of production functions that the requirements for an exact correspondence between aggregate and micro relationships are enormously strong. Through the work of Terence Gorman (1953, 1959, 1968), Muellbauer (1975, 1976) and others these conditions have been somewhat weakened but remain heroic. In this light it might appear somewhat surprising that aggregate relationships do seem to be relatively constant over time and are broadly interpretable in terms of economic theory.

An interesting clue as to why this might be was provided by Yehuda Grunfeld and Zvi Griliches (1960) who asked "Is aggregation necessarily bad?" In his Ph.D. thesis, Grunfeld had obtained the surprising result that the investment expenditures of an aggregate of eight major US corporations were better explained by a two variable regression on the aggregate market value of these corporations and their aggregate stock of plant and equipment at the start of each period, than by a set of eight micro regressions in which each corporation's investment was related to its own market value and stock of plant and equipment (Grunfeld, 1958). In forecasting the aggregate, one would do better using an aggregate equation than by disaggregating and forecasting each component of the aggregate separately. Grunfeld and Griliches suggest that this may be explained by misspecification of the micro relationships. If there is even a small dependence of the micro variables on aggregate variables, this can result in better explanation by the aggregated equation than by the slightly misspecified micro equations.

This result has recently been rediscovered by Clive Granger (1987) who distinguishes between individual (i.e., agent-specific) factors and common factors in the micro equations. In the demand context, an example of a common factor would be the interest rate in a demand for a durable good. If we consider a specific agent, the role of the interest rate is likely to be quite small — whether or not a particular household purchases, say a refrigerator, in a particular period will mainly depend on whether the old refrigerator has finally ceased to function (replacement purchase) or the fact that the household unit has just been formed (new purchase). The role of the interest rate will be secondary. However, the individual factors are unlikely to be fully observed. If one is obliged to regress simply on the common factors – in this case the interest rate – the micro R^2 will be tiny (with a million households, Granger obtains a value of 0.001), but the aggregate equation may have a very high R^2 (Granger obtains 0.999) because the individual effects average out across the population.[6]

So long as the micro relationships are correctly specified and all variables (common and individual) are fully observed there is no gain through aggregation. However, once we allow parsimonious simplification strategies, the effects of these simplifications will be to result in quite different micro and aggregate relationships. Furthermore, it is not clear *a priori* which of these approximated relationships will more closely reflect theory. Blundell (1988) implied that when micro and aggregate relationships differ this must entail aggregation bias in the aggregate relationships. Granger's results show that theoretical effects may be swamped at the micro level by individual factors which are of little interest to the economist, and which in any case are likely to be incompletely observed resulting in omitted variable bias in the micro equations. Microeconometrics is important, but it does not invalidate traditional aggregate time series analysis.[7]

My concern here is with the methodology of aggregate time series econometrics so I shall not dwell on the problems of doing microeconometrics. The question I have posed is how economic theory should be incorporated in aggregate models. The naïve answer to this question is the reductionist route, in which the parameters of aggregate relationships are interpreted in terms of the decisions of a representative optimising agent. However, there is absolutely no reason to suppose that the aggregation assumptions required by this reduc-will hold. There is little point, therefore, in using these estimated aggregate relationships to "test" theories based on the optimising behaviour of a representative agent – if we fail to reject the theory it is only because we have insufficient data.[8]

6. Strictly, the variance of the household effects is of order n, where n is the number of households, and the variance of the common factors is of order n^2. Hence, as the number of household becomes large, the contribution of the individual effects becomes negligible. In the converse case in which we observe the individual factors but not the common factors the R^2s are reversed. See also Granger (1988).

7. Werner Hildenbrand (1983) arrives at a similar conclusion in a more specialised context. He remarks (*ibid*, p. 998) "There is a *qualitative difference* in market and individual demand functions. This observation shows that the concept of the 'representative consumer', which is often used in the literature; does not really simplify the analysis; on the contrary, it might be misleading". I am grateful to José Carbajo for bringing this reference to my attention.

8. I do not wish to claim that "If the sample size is large you reject everything" – see Peter Phillips (1988, p. 11).

It is now nearly ten years since Sims argued in his "Macroeconomics and Reality" (Sims, 1980) that the Haavelmo-Cowles programme is misconceived. Theoretically-inspired identifying restrictions are, he argued, simply "incredible". This is partly because many sets of restrictions amount to no more than normalisations together with "shrewd aggregations and exclusion restrictions" based on an "intuitive econometrician's view of psychological and sociological theory" (Sims, 1980, pp. 2-3); because the use of lagged dependent variables for identification requires prior knowledge of exact lag lengths and orders of serial correlation (Michio Hatanaka, 1975); and partly because rational expectations imply that any variable entering a particular equation may, in principle, enter all other equations containing expectational variables. In Sims' example, a demand for meat equation is "identified" by normalisation of the coefficient on the quantity (or value share) of meat variable to -1; by exclusion of all other quantity (or value share) variables; and by exclusion of the prices of goods considered by the econometrician to be distant substitutes for meat, or replacement of these prices by the prices of one or more suitably defined aggregates.

The VAR methodology, elaborated in a series of papers by Thomas Doan, Robert Litterman and Sims, is to estimate unrestricted distributed lags of each non-deterministic variable on the complete set of variables (Doan, Litterman and Sims, 1984; Litterman, 1986a; Sims, 1982, 1987). Thus given a set of k variables one models

$$x_{it} = \sum_{j=1}^{k} \sum_{r=1}^{n} \beta_{ijr} x_{j,t-r} + u_{it} \quad (i = 1, \ldots, k) \tag{1}$$

The objective is to allow the data to structure the dynamic responses of each variable. Obviously, however, one may wish to consider a relatively large number of variables and relatively long lag lengths, and this could result in shortage of degrees of freedom and in poorly determined coefficient estimates. Some of the early VAR articles impose "incredible" marginalisation (i.e., variable exclusion) and lag length restrictions – for example, Sims (1980) uses a six variable VAR on quarterly data with lag length restricted to four. But these restrictions are hardly more palatable than those Sims argued against in "Macroeconomics and Reality" and at least implicit recognition of this has pushed the VAR school into an adoption of a Bayesian framework. The crucial element of Bayesian VAR (BVAR) modelling is a "shrinkage" procedure in which a loose Bayesian prior distribution structures the otherwise unrestricted distributed lag estimates (Doan *et al.*, 1984; Sims, 1987).

A prior distribution has two components – the prior mean and the prior variance. First consider the prior mean. If one estimates a large number of lagged coefficients, one will intuitively feel that many of them, particularly those at high lag lengths, should be small.[9] Doan *et al.* (1984) formalise this intuition by specifying the prior for each modelled variable as a random walk with drift.[10] This prior can be justified on the argument that, under (perhaps incredibly) strict assumptions, random walk models appear as the outcomes of the decisions of atomistic agents optimising under uncertainty (most notably, Hall, 1978); or on the heuristic argument that "no change" forecasts provide a sensible "naïve" base against which any other forecasts should be compared. More formally, one can argue that collinearity is clearly a major problem in the estimation of unrestricted distributed lag models and that severe collinearity may give models which "produce erratic, poor forecasts and imply explosive behavior of the data" (Doan *et al.*, 1984). A standard remedy for collinearity, implemented in ridge regression (Arthur Hoerl and Robert Kennard, 1970a, b) is to "shrink" these coefficients towards zero by adding a small constant (the "ridge constant") to the diagonal elements of the data cross-product matrix.[11]

Specification of the prior variance involves the investigator quantifying his/her uncertainty about the prior mean. The prior variance matrix will typically contain a large number of parameters, and this therefore appears a daunting task. Much of the originality of the VAR shrinkage procedure arises from the economy in specification of this matrix which, in the most simple case, is characterised in terms of only three parameters (Doan, Litterman and Sims, 1986). These are the overall tightness of the prior distribution, the rate at which the prior standard deviations decay, and the relative weight of variables other than the lagged dependent variable in a particular autoregression (with prior covariances set to zero). A tighter prior distribution implies a larger ridge constant and this results in a greater shrinkage towards the random walk model. The important feature of the Doan *et al.* (1984) procedure is that the tightness of the prior is increased as lag length increases. Degrees of freedom considerations are no longer paramount since coefficients associated with long

9. But note that this intuition may be incorrect if one uses seasonally unadjusted data. However, Kenneth Wallis (1974) has shown that use of seasonally adjusted data can distort the dynamics in the estimated relationships.

10. The exposition in Doan *et al.* (1948) is complicated and not entirely consistent. See John Geweke (1984) for a concise summary.

11. In the standard linear model

$$y = X\beta + u$$

where y and X are both measured as deviations from their sample means, the ridge regression estimator of β is

$$b = (X'X + kI)^{-1}X'y$$

where k is the ridge constant.

lag lengths, and with less important explanatory variables, are forced to be close to zero.

It is often suggested that the VAR approach is completely atheoretical (see, e.g., Thomas Cooley and Stephen LeRoy, 1986). This view is given support by those VAR modellers whose activities are primarily related to forecasting and who argue that relevant economic theory is so incredible that one will forecast better with an unrestricted reduced form model (Litterman, 1986a, b; Stephen McNees, 1986).[12] However, this position is too extreme. Most simply, theory may be tested to a limited extent by examination of block exclusion (Granger causality) tests, although I would agree with Sims that, interpreted strictly, such restrictions are not in general credible. It is therefore more interesting to examine the use of VAR models in policy analysis since in this activity theory is indispensable.

Suppose one is interested in evaluating the policy impact of a shock to the money supply. One will typically look for a set of dynamic multipliers showing the impact of that shock on all the variables of interest. An initial difficulty is that in VAR models all variables are jointly determined by their common history and a set of current disturbances. This implies that it does not make sense to talk of a shock to the money supply unless additional structure is imposed on the VAR. To see this, note that the autoregressive representation (1) may be transformed into the moving average representation

$$x_{it} = \sum_{j=1}^{k} \sum_{r=0}^{\infty} \alpha_{ijr} u_{j,t-r} \quad (i = 1, \ldots, k) \tag{2}$$

where each variable depends on the history of shocks to all the variables in the model. There are two possibilities. Take the money supply to be variable 1. If none of the other k-1 variables in the model Granger-causes the money supply (so that $\beta_{1jr} = \alpha_{1jr} = 0$ for all $j>1$ and r) we may identify monetary policy with the innovation u_1 on the money supply equation and trace out the effects of these innovations on the other variables in the system. It is more likely, however, particularly given Sims' views, that all variables are interdependent at least over time. In that case analysis of the effects of monetary policy requires the identifying assumption that the monetary authorities

12. For example, Litterman (1986b, p. 26) writes in connection with business cycles, ". . . there are a multitude of economic theories of the business cycle, most of which focus on one part of a complex multifaceted problem. Most economists would admit that each theory has some validity, although there is wide disagreement over the relative importance of the different approaches." And in conjunction with the Data Resources Inc. (DRI) model investment sector, he states, "Even if one accepts the Jorgenson theory as a reasonable approach to explaining investment, the empirical implementation does not adequately represent the true uncertainty about the determinants of investment."

choose x_1 independently of the current period disturbances on the other equations. In an older terminology, this defines the first link in a Wold causal chain with money causally prior to the other variables (Herman Wold and Radnar Bentzel, 1946; Wold and Lars Jureen, 1953). It can be implemented by renormalisation of (2) such that x_{1t} depends only on the policy innovations v_{1t} while the remaining variables depend on v_{1t} and also a set of innovations $v_{2t}, \ldots, v_{kt}$ which are orthogonal to v_{1t}. In the limiting case in which all the innovations are mutually orthogonal, we may rewrite (2) as

$$x_{it} = \sum_{j=1}^{i} \sum_{r=0}^{\infty} \gamma_{ijr} v_{j,t-r} \qquad (i = 1, \ldots, k) \tag{3}$$

This expression is unique given the ordering of the variables, but as Pagan (1987) notes, it is not clear *a priori* how the innovations $v_{2t}, \ldots, v_{kt}$ should be interpreted. The policy multipliers will depend on the causal ordering adopted, and the ordering of variables 2 . . k may in practice be somewhat arbitrary. We find therefore that, although in estimation VAR modellers can avoid making strong identifying assumptions, policy interpretation of their models, including the calculation of policy multipliers, requires that one make exactly the same sort of identifying assumption that Sims criticised in the Haavelmo-Cowles programme. This is the basis of Cooley and LeRoy's (1985) critique of atheoretical macroeconometrics.

As a criticism of Sims, this is too strong. Note first that in his applied work, Sims does not restrict himself to orthogonalisation assumptions as in (3), but is willing to explore a wider class of identifying restrictions which are not dissimilar to those made by structural modellers (see Sims, 1986). Moreover, he allows himself to search over different sets of identifying assumptions in order to obtain plausible policy multipliers. However, the sets of assumptions he explores all generate just identified models with the implication that they are all compatible with the same reduced form. This permits a two stage procedure in which at the first stage the autoregressive representation (1) is estimated, and at the second stage this representation is interpreted into economic theory by the imposition of identifying assumptions on the moving average representation. The identifying assumptions may be controversial, but they do not contaminate estimation.

Although it is not true that VAR modelling is completely atheoretical, the philosophy of the VAR approach may be caricatured as attempting to limit the role of theory in order to obtain results which are as objective as possible and as near as possible independent of the investigator's theoretical beliefs or prejudices. An alternative approach, associated with what I have called elsewhere (Gilbert, 1989) the LSE (London School of Economics) methodology is to use theory to structure models in a more or less loose way so as to obtain a model whose general interpretation is in line with theory but whose detail is determined by the data. The instrument for ensuring coherence with the data is classical testing methodology.

This immediately prompts the question of what constitutes a test of a theory which we regard as at best approximate? I have noted that it does not usually make much sense to suppose that we can sensibly use classical testing procedures to attempt to reject theories based on the behaviour of atomistic optimising agents on aggregate economic data, since there is no reason to suppose that those theories apply precisely on aggregate data.[13] There are in practice two interesting questions. The first is whether a given theory is or is not too simple relative both to the data and for the purposes to hand. The second question is whether one theory-based model explains a given dataset better than another theory-based model.

The issue of simplification almost invariably prompts the map analogy. For example, Leamer (1978, p. 205) writes "Each map is a greatly simplified version of the theory of the world; each is designed for some class of decisions and works relatively poorly for others". Simplification is forced upon us by the fact that we have limited comprehension, and, more acutely in time series studies, by limited numbers of observations. As the amount of data available increase, we are able to entertain more complicated models, but this is not necessarily a benefit if we are interested in investigating relatively simple theories since the additional complexity may then largely take the form of nuisance parameters. Frequently, the increased model complexity will take the form of inclusion of more variables[14] – i.e., revision of the marginalisation decision – and this can be tested using conventional classical nested techniques. The important question is whether omission of these factors results in biased coefficient values and incorrect inference in relation to the purposes of the investigation. The tourist and the geologist will typically use different maps, but the tourist may wish to know if there are steep gradients on his/her route, and questions of access are not totally irrelevant to the geologist.

The obvious trade-off in the sort of samples we frequently find ourselves analysing in time series macroeconometrics is between reduction in bias through the inclusion of additional regressors and reduced precision through the reduction in degrees of freedom and increase in collinearity. Short samples of aggregate data can only relate to simple theories since they only contain a limited amount of information. Macroeconometric models will therefore be more

13. Heterogeneity may imply that these theories also fail to hold on micro data.

14. Phillips (1988, p. 28) notes that it is implicit in the Hendry methodology that the number k or regressors grows with the sample size T in such a way that $k/T \to 0$ as $T \to \infty$.

simple than the world they purport to represent. This does not particularly matter, but it does imply that we must always be aware that previously neglected factors may become important – an obvious example is provided by the role of inflation in the consumption function.

Two strategies are currently available for controlling for structural non-constancy. VAR modellers advise use of random coefficient vector auto-regressions in which the model coefficients all evolve as random walks (Doan *et al.*, 1984). In principle, this leads to very high dimensional models, but imposition of a tight Bayesian prior distribution heavily constrains the co-efficient evolution and permits estimation. This procedure automates control for structural constancy, since the modeller's role is reduced to choice of the tightness parameters of the prior. A disadvantage is that it cannot ever prompt reassessment of the marginalisation decision – i.e., inclusion of previously excluded or unconsidered regressor variables.

An alternative approach which is gaining increasing support is the use of recursive regression methods to check for structural constancy. In recursive regression one uses updating formulae, first worked out by Timo Terasvirta (1970), to compute the regression of interest for each subsample [1,t] for $t=T_1, \ldots, T$ where T is the final observation available and T_1 is of the order of three times the number of regressor variables (see Hendry, 1989, pp. 20-21). This produces a large volume of output which is difficult to interpret except by graphical methods. Use of recursive methods had therefore to wait until PC technology allowed easy and costless preparation of graphs. It is now computationally trivial to graph Chow tests (Gregory Chow, 1960) for all possible structural breaks, or for one period ahead predictions for all periods within the $[T_1, T-1]$ interval. Also one can plot coefficient estimates against sample size. Although these graphical methods do not imply any precise statistical tests, they show up structural non-constancy of either the break or evolution form in an easily recognisable form, and prompt the investigator to ask why a particular coefficient is moving through the sample, or why a particular observation is exerting leverage on the coefficient estimates. These questions should then prompt appropriate model respecification. I am not aware that Leamer has ever advised use of recursive methods, but they do appear to be very much in the spirit of his concern with fragility in regression estimates (see Leamer and Hermann Leonard, 1983), even if the proposed databased "solution" is not one he would favour.

Level of complexity is therefore primarily a matter of sample size. The more interesting questions arise from comparison of alternative and incompatible simple theories which share the same objectives. Although maps may differ only in the selection of detail to represent, they may also differ because one or other map incorrectly represents certain details. In such cases we are required to make a choice. There is now a considerable body of both econometric theory and of experience in non-nested hypothesis testing. Suppose we have two alternative and apparently congruent models A and B. Suppose initially model A (say a regression of y on X) gives the "correct" representation of the economic process under consideration. This implies that the estimates obtained by incorrectly supposing model B (regression of y on Z) to be true will suffer from misspecification bias. Knowledge of the covariance of the X and Z variables allows this bias to be calculated. Thus if A is true, it allows the econometrician to predict how B will perform; but if A does not give a good representation of the economic process, it will not be able to "explain" the model B coefficients. Furthermore, we can reverse the entire procedure and attempt to use model B to predict how A will perform.

These non-nested hypothesis tests, or encompassing tests as they are sometimes called, turn out to be very simple to perform. One forms the composite but quite possible economically uninterpretable hypothesis $A \cup B$ which in the case discussed above is the regression of y on both X and Z (deleting one occurrence of any variable included in both X and Z), and then performs the standard F tests of A and B in turn against $A \cup B$. Four outcomes are possible. If one can accept the absence of the Z variables in the presence of the X variables, but not vice versa (i.e., $E[y|X,Z] = X\alpha$), model A is said to encompass model B; equally, model B may encompass model A ($E[y|X,Z] = Z\beta$). But two other outcomes are possible. If one cannot accept either $E[y|X,Z] = X\alpha$ or $E[y|X,Z] = Z\beta$ neither hypothesis may be maintained. Finally, one might be able to accept both $E[y|X,Z] = X\alpha$ and $E[y|X,Z] = Z\beta$ in which case the data are indecisive.[15] This relates to Thomas Kuhn's view that a scientific theory will not be rejected simply because of anomalies, but rather because some of these anomalies can be explained by a rival theory (Kuhn, 1962).

The LSE procedure may be summarised as an attempt to obtain a parsimonious representation of a general unrestricted equation.[16] This representation should simultaneously satisfy a number of criteria (Hendry and Jean-Francois Richard, 1982, 1983). First, it must be an acceptable simplification of the unrestricted equation either on the basis of a single F test against the unrestricted equation, or on the basis of a sequence of such tests.[17] Second, it should have serially independent errors. Third, it must be structurally constant.

I will return to the error correction specification shortly. The model dis-

15. This is "coefficient encompassing". A more limited question ("variance encompassing") is whether we can explain the residual variances. Coefficient encompassing implies variance encompassing, but not *vice versa* (Mizon and Richard, 1986).

16. Usually this will involve OLS estimation of single equations, but the same procedures may be adopted in simultaneous models using appropriate estimators.

17. See Grayham Mizon (1977).

covery activity takes place in part in the parsimonious simplification activity, which typically involves the imposition of zero or equality restrictions on sets of coefficients, and also importantly in reviewing the marginalisation (variable exclusion) decisions. Parsimonious simplification may be regarded as in large measure a tidying up operation which does little to affect equation fit, controls for collinearity and thereby improves forecasting performance, and at worst results in exaggerated estimates of coefficient precision (since coefficients which are approximately zero or equal are set to be exactly zero or equal).[18] Pravin Trivedi (1984) has coined the term "testimation" to describe the "empirical specification search involving a blend of estimation and significance tests". Importantly, parsimonious simplification conserves degrees of freedom and in this respect it is not dissimilar to the shrinkage procedure adopted in VAR modelling, the difference being mainly whether one imposes strong restrictions on a set of near zero coefficients (LSE), or weaker restrictions on the entire set of coefficients (VAR). It does not seem to me that there is any strong basis for suggesting that one method has superior statistical properties than the other. VAR modellers argue that their models have superior forecasting properties, but LSE modellers would reply that their methods tend to be more robust with respect to structural change. This is not an argument that can be settled on an *a priori* basis.

Opening up the marginalisation question is of greater importance. If a variable which is of practical importance is omitted from the model, perhaps because its presence is not indicated by the available theory, this omission is likely to cause biased coefficient estimates and either serially correlated residuals or over-complicated estimated dynamics. In the former case, one might be tempted to estimate using an appropriate autoregressive estimator, which is tantamount to regarding the autoregressive coefficients as nuisance parameters; while in the latter one will obtain the same result via unrestricted estimates of the autoregressive equation. The alternative, which is familiar to all of us, is to take the residual serial correlation as prompting the question of whether the model is well-specified, and in particular, whether important variables have been omitted. Subsequent discovery that this is indeed the case may either indicate a need to extend or revise the underlying theory, or more simply suggest the observation that the theory offers only a partial explanation of the data. In the latter case, the additional variables introduced into the model may perhaps be legitimately regarded as nuisance variables, but in the former case the two way interaction between theory and data will have a clear positive value.

18. Contrast Leamer (1985) who describes the LSE methodology as "a combination of backward and forward step-wise (better known as unwise) regression . . . The order for imposing the restrictions and the choice of significance level are arbitrary . . . What meaning should be attached to all of this?"

The feature of the LSE approach on which I wish to concentrate is the role of cointegration and the prevalence of the error correction specification. The error correction specification is an attempt to combine the flexibility of time series (Box-Jenkins)[19] models in accounting for short term dynamics with the theory-compatibility of traditional structural econometric models (see Gilbert, 1989). In this specification both the dependent variable and the explanatory variables appear as current and lagged differences (sometimes as second differences or multi-period differences), as in Box-Jenkins models, but unlike those models, the specification also includes a single lagged level of the dependent variable and a subset of the explanatory variables. For example, a stylised version of the Davidson *et al.* (1978) consumption function may be written as

$$\Delta_4 \ln c_t = \beta_0 + \beta_1 \Delta_4 \ln y_t + \beta_2 \Delta_1 \Delta_4 \ln y_t - \beta_3 (\ln c_{t-4} - \ln y_{t-4}) \quad (4)$$

where, on quarterly data, annual changes in consumption are related to annual changes in income and a four quarter lagged discrepancy between income and consumption. It is these lagged levels variables which determine the steady state solution of the model.[20] It will frequently be found that augmentation of pure difference equations by lagged levels terms in this way has a dramatic effect on forecasts and on estimated policy responses.

It is always possible to reparameterise any unrestricted distributed lag equation specified in levels (e.g., a VAR) into the error correction form, so it may appear odd to claim any special status for this way of writing distributed lag relationships. Note however that the LSE procedure implicitly prohibits parsimonious simplification of the unrestricted equation into a Box-Jenkins model in which the lagged level of the dependent variable is excluded, even if this exclusion would result in negligible loss in fit. In this sense, the specification is non-trivial. That it is an interesting non-trivial specification depends on the claim that economic theory implies a set of comparative static results which are reflected in long-run constancies, and is reinforced by the logically independent but incorrect claim that economic theory tells us little about short-term adjustment processes.[21]

The earliest error correction specification was Denis Sargan's (1964) wage

19. George Box and Gwylym Jenkins (1970).

20. In the steady state solution all the differenced variables are set to zero. The steady state growth solution, in which all the differenced variables are set to appropriate constants, is often more informative – see James Davidson, David Hendry, Frank Srba and Stephen Yeo (1978), and Gilbert (1986, 1989).

21. See for example Phillips (1988, p. 19): "In macroeconomics, theory usually provides little information about the process of short run adjustment".

model in which the rate of increase in wage rates was related to the difference between the lagged real wage and a notional target real wage. Here there is a straightforward structural interpretation of the error correction term. More recently, however, the generality of the specification has received support from the Granger representation theorem (Robert Engle and Granger, 1987) which states that if there exists a stationary linear combination of a set of non-stationary variables (i.e., if the variables are "cointegrated") then these variables must be linked by at least one relationship which can be written in the error correction form. (If this were not the case, the variables would increasingly diverge over time.) Since most macroeconomic aggregates are non-stationary (typically they grow over time) any persisting (autonomous) relationship between aggregates over time is likely to be of the error correction form.

Cointegration therefore provides a powerful reason for supposing that there will exist structural constant relationships between macroeconomic aggregates. If economic time series are non-stationary but cointegrated there are strong arguments for imposing the error correction structure on our models, and it is an advantage of the LSE methodology over the VAR methodology that it adopts this approach. A major role for economic theory in the LSE methodology is to aid the specification of the cointegrating term. Unsurprisingly, short samples of relatively high frequency (quarterly or monthly) data are often relatively uninformative about the long-run relationship between the variables, so that theoretically unmotivated specification of these terms gives little precision or discrimination between alternative specifications. One possibility, suggested by Engle and Granger (1987), is a two stage procedure where at the first stage one estimates the static ("cointegrating") regression ignoring the short-term dynamics, and at the second stage one imposes these long-run coefficients on the dynamic error correction model. However, Monte Carlo investigation suggests that this procedure has poor properties (Anindya Banerjee, Juan Dolado, David Hendry and Gregor Smith, 1986) and that it is preferable to attempt to estimate the long-run solution from the dynamic adjustment equation as in the initial Sargan (1964) wage model and the Davidson *et al.* (1978) consumption function model. Nevertheless, the long-run solution may still be poorly determined, implying that theoretical restrictions are unlikely to be rejected.

The theoretical status of the short-run dynamics in the LSE parsimoniously simplified equations is more problematic and here economic theory has as yet been less helpful. Hendry and Richard (1982, 1983) describe the modelling exercise as an attempt to provide a characterisation of what they call the "Data Generating Process" (the DGP) which is the joint probability distribution of the complete set of sample data (endogenous and exogenous variables).

Actual DGPs, they suggest, will be very complicated, but the combination of marginalisation (exclusion of variables that do not much matter), conditioning (regarding certain variables as exogenous[22]) and simplification which together make up the activity of modelling can give rise to simple and structurally constant representations of the DGP.

The DGP concept derives from Monte Carlo analysis where the investigator specifies the process which will generate the data to be used in the subsequent estimation experiments. This suggests an analogy in which we suppose a fictional statistician choosing the data that we analyse in applied economics. In a pioneering contribution to the Artificial Intelligence literature, Alan Turing (1950) asked whether an investigator could infallibly distinguish which of two terminals is connected to a machine and which operated by a human. Hendry dares us to claim that we can distinguish between Monte Carlo and real world economic data. If we cannot, the DGP analogy carries over, and we can hope to discover structural short-term dynamics.

This argument appears to me to be flawed. If macroeconomic data do exhibit constant short-term dynamics then one might expect any structural interpretation to relate to the parameters of the adjustment processes of the optimising agents. But we have seen that the aggregation conditions required for the aggregate parameters to be straightforwardly interpretable in terms of the microeconomic parameters are heroic. With Monte Carlo data, by contrast, we can be confident that there does exist a simple structure since the structure has been imposed by a single simple investigator. There are no aggregation issues, and the question of reduction does not arise.

The most promising route for rationalising the dynamics of LSE equations is in terms of the backward representation of a forward looking optimising models. In simple models, optimising behaviour in the presence of adjustment costs will give rise to a second order difference equation which can be solved to give a lagged (partial) adjustment term and a forward lead on the expected values of the exogenous variables. But these future expected values may always be solved out in terms of past values of the exogenous variables giving a backward looking representations. Stephen Nickell (1985) showed that in a number of cases of interest, this backward representation will have the error correction form, and this suggests that it may in general be possible to rationalise error correction models in these terms (Keith Cuthbertson, 1988). An implication of this view, via the Lucas (1976) critique, is that if the process followed by any of the exogenous variables changes, the backward looking relationship will be structurally non-constant while the forward looking representation

22. Strictly "at least weakly exogenous" – see Engle, Hendry and Richard (1983).

will remain constant. Current experience, however, is that in these circumstances it is the forward looking equation that is non-constant (Hendry, 1988; Carlo Favero, 1989).

An alternative approach is to regard the short-term dynamics in LSE relationships as nuisance terms. Direct estimation of the cointegrating relationships is inefficient because of the residual serial correlation resulting from the omitted dynamics and may be inconsistent because of simultaneity. It is possible that in part these omitted dynamics arise from aggregation across heterogeneous agents (Marco Lippi, 1988). In principle, one could estimate using a systems maximum likelihood (ML) estimator taking into account the serial correlation (Soren Johansen, 1988; Soren Johansen and Katarina Juselius, 1989), but there is advantage in using a single equations estimator since this localises any misspecification error. The single equations estimator must correct both for the simultaneity and for the serial correlation. In recent work Phillips (1988) has argued that LSE dynamic equations often come very close to and sometimes achieve optimal estimation of the cointegrating relationship. On this interpretation, the short-run dynamic terms in those equations are simply the simultaneity and Generalised Least Squares (GLS) adjustments, in the same way that one can rewrite the familiar Cochrane-Orcutt autoregressive estimator (Donald Cochrane and Guy Orcutt, 1949) in terms of a restricted OLS estimation of an equation containing lagged values of the dependent variable and the regressor variables (Hendry and Mizon, 1978). An implication is that we have come full circle back to pre-Haavelmo econometrics where the concern was the measurement of constants in lawlike relationships which in modern terminology are simply the cointegrating relationships.

However, this is to miss much of the point of the methods generated by Sargan, Hendry and their colleagues. Routine forecasting and policy analysis in econometrics is as much or more concerned with short-term movements in key variables than with their long-term equilibria. Furthermore, short-term (derivative) responses are generally very much better determined than long-term relationships. I argued in Gilbert (1989) that a substantial part of the motivation of the LSE tradition in econometrics was the perceived challenge to "white box" econometric models from "black box" time series (Box-Jenkins) models (Richard Cooper, 1972; Charles Nelson, 1972). The same points are true in relation to the development of VAR methodology. Practitioners of the LSE approach are unlikely, therefore, to recognise themselves in Phillips' description.

At the start of his famous 1972 survey "Lags in Economic Behavior", Marc Nerlove quoted Schultz (1938) as saying "Although a theory of dynamic economics is still a thing of the future, we must not be satisfied with the *status quo* in economics". Nerlove then went on to remark that "dynamic economics is still, in large part, a thing of the future" (Nerlove, 1972, p. 222). The rational expectations optimising models, examples of which I have already discussed, have constituted a major attempt to provide that dynamic theory. They have not been wholly successful, for the reasons I have indicated. Neither have they been wholly unsuccessful. A possible criticism of both the VAR and LSE approaches to modelling aggregate macro-dynamics is that they do not make any attempt to accommodate these theories. An alternative possibility is to argue that the problem is on the theorists' side; and that the rational expectations atomistic optimising models deliver models which are too simple even to be taken as reasonable approximations. The problem is, nevertheless, that however much the anomalies multiply, we are likely to abandon these theories until an alternative paradigm becomes available. Sadly, I do not see any indication that such a development is imminent.

I started this lecture by recalling a commitment to unify theory and empirical modelling. That programme has recorded a measure of success, but to a large extent that success has been in the modelling of long-term equilibrium relationships. When Nerlove surveyed the methods of dynamic economics, the contribution of theory was relatively new and relatively slight. We now have much better developed and more securely based theories of dynamic adjustment but these theories have been too simple to inform practical modelling. It is obviously possible to argue that this is the fault of the econometricians, and the level of discord among the econometricians might be held as evidence for this view. My suspicion is, however, that the current disarray in the econometric camp is the consequence of the lack of applicable theory. Where we have informative and detailed theories, as for example in demand analysis or the theory of financial asset prices, methodological debates are muted. If the theorist can develop realistic but securely based dynamic theories, then the competing approaches to econometric methodology could coexist quite happily throughout macroeconometrics.

I have made a number of different arguments in the course of this paper, so a brief summary may be useful.

1. I agree with the currently widely held view that it is not possible in general to estimate parameters of micro functions from aggregate data.
2. I disagree with the implied view that aggregate relationships cannot be interpreted in terms of microeconomic theory. The appropriate level of aggregation will depend both on the purpose of the modelling exercise and on the questions being asked.
3. Theoretical restrictions should not be expected to hold precisely on aggregate data. This implies that classical rejections cannot *per se* be taken to imply rejection of the theories in question.
4. Classical techniques of non-nested hypothesis testing provide a method

for discriminating between alternative imprecise theories.

5. It is difficult to argue *a priori* that Bayesian shrinkage procedures have either superior or inferior statistical properties to the pseudo-structural methods associated with the British approach to dynamic modelling. An advantage of the latter approach is however that it gives a central role to model discovery, which may allow a beneficial feedback from data to theory.
6. Cointegration provides a powerful reason for believing that macroeconomic aggregates will be linked by structurally stable relationships, and it is an important advantage of the British approach that it embodies this feature or economic time series through error correction. However, the argument that the British approach to dynamic modelling should be seen as simply a method of efficiently estimating these equilibrium relationships is misconceived.
7. The progress in estimating relationships has not been matched by comparable progress in estimating dynamic adjustment processes, where theory and data appear to be quite starkly at odds. A possible response is that the existing optimising theories are just too simple.

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