

## 1. Introduction

Birth of macroeconometric structural modelling dates back to the days when Cowles Commission at the University of Chicago was the high point of the academic world for economists. Those associated with the commission included luminaries like Tjalling Koopmans, Kenneth Arrow, Trygve Haavelmo, T.W. Anderson, Lawrence Klein, G. Debreu, Leonid Hurwitz, Harry Markowitz, Jacob Marschak, Franco Modigliani and many others<sup>1</sup>. That was also the time when the Keynesian revolution was centre-stage even though its micro foundations were far from clear either to its supporters or to its detractors. Like macroeconomic theory structural macroeconomic models were based on stylised facts and contextual in their character.

The compelling need for activist macroeconomic policies for short run stabilisation and long run growth in the post world war economy gave a fillip to both Keynesian macrotheory as well as to policy oriented empirical macroeconomic research. What Paul Samuelson was later to christen as consensus macroeconomics was round the corner. The profession resumed the work that Jan Tinbergen had begun even before the second world war broke out. The first indications of what was later to blossom into a major branch of academic pursuit, namely, macroeconometric modelling, came with Klein's book *Keynesian Revolution* which had grown out of the first doctoral dissertation in economics at MIT and the first one supervised by Paul Samuelson. This was followed up by the first model by Klein in 1950 and another coauthored by him and Goldberger in 1955.

The work that followed was initially addressed to the academic profession and intended to provide a forum for meaningful discussions on macroeconomic policy issues. Testing alternative macroeconomic theories does not appear to have been explicitly on the agenda<sup>2</sup>. This could very well be due to the wide consensus in favour of the Keynesian framework that prevailed in the profession, at least as far as the developed western economies were concerned. Subsequently, macroeconometric modelling outgrew its parsimonious academic orientation so as to be able to handle

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<sup>1</sup> See Jansen (2000) and Diebold (1998).

<sup>2</sup> This was, in their own way, taken up later on by Friedman and Meiselman (1963) in terms of multiplier versus velocity.

the real world policy issues in a pragmatic manner. In particular forecasting and counterfactual policy simulations assumed wide popularity. This development also saw the emergence of larger models and a wide spectrum of theoretical and econometric compromises.

## 2. Cowles Commission Methodology<sup>3</sup> (CCM)

Though Tinbergen constructed the first macroeconometric model in 1939 a proper articulation of the CCM began only with the seminal 1944 paper of Haavelmo entitled "The probability Approach in Econometrics". He claimed,

"Theoretical models are necessary tools in our attempts to understand and explain events in real life. But whatever explanations we prefer, it is not to be forgotten that they are all our own artificial inventions in the search for an understanding of real life; they are not hidden Truths to be discovered."

Haavelmo saw economic modelling almost purely in probabilistic terms (Jansen, 2000). This is in some quarters being once again stressed as an essential return to the roots<sup>4</sup>.

Quoting Tinbergen's *Selected papers* published in 1959 Charemza and Deadman (1997) identify five major assumptions underlying CCM. These are as follows. *First*, "**Causal ordering**" which permits the specification of an interdependent system of equations with variables entering the model classified into two types : endogenous and exogenous. *Second*, imposition of "**zero restrictions**" by means of which specific variables, endogenous or exogenous are excluded from specific equations to ensure identifiability.

*Third*, "**time invariance**" of relationships which rules out autonomous changes over time. *Fourth*, "**Structural invariance**", which means that parameters are invariant to movements in variables included in the model though they may

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<sup>3</sup> This is some times also referred so as the systems of equations approach (SEA).

<sup>4</sup> Concepts like weak and strong exogeneity are rooted in this approach. Also see Eichenbaum (1991).

change due to movements in variables not included in the model. In other words the model, as it stands, is subject to structural changes only due to shocks which are exogenous to the model. *Finally*, "**model validation**" is best that can be done, and relies on diagnostics like goodness of fit, students' t, prior restrictions on signs and size of parameters and above all ex-post performance of models as tools for forecasting and policy analysis. It does not appear that explicit testing of one model against another was emphasized. All the same, this could turn out to be an implicit exercise. How far this methodology has remained in place and how far it has responded to new developments shall be taken up later.

### **3. The Flux in Content and Methodology**

Though it is not, in principle, the case that CCM was meant only for macroeconomic applications and within these only for Keynesian economics, yet given the historical context the two came to be seen as strongly embedded. It is not thus surprising that CCM came under a serious challenge almost at the same time as the Keynesian macroeconomics lost its universal appeal. It is interesting to note here that since macroeconomic judgements eventually turn out to be empirically verifiable, controversies in theory unavoidably spill over to econometric issues. Indeed new macroeconomic paradigms have given rise to associated econometric methodologies. Let us consider the new paradigms in macroeconomic theory first.

At the outset let it be noted that despite what Samuleson termed as Keynesian consensus the voice of dissent persisted all through and was frequently loud, thanks to Professor Friedman and his associates. Nonetheless, there was all through a common view that the issues are essentially empirical. Even in theory there was a mutually understood and commonly employed mode of discourse. Developments in the early seventies marked a major departure from this when the empirical content of macroeconomic debate was considerably eroded. On top of it, the very basis and motivation for policy debate died. This is true of New Classical Economics and even of New Keynesian Economics to some extent. In the latter because it does not add up to a substantive macro paradigm (Fair, 1994) and in the former because it provides no space for effective policy (Lucas and Sargent, 1981). In between these two we have the non-Walrasian macro-economics which provides the macro foundations to

Keynesian theory and at the same time presents a generalised model in which unemployment could be either Keynesian or classical. Correspondingly the effective policy prescriptions could be different in the two cases (Benassy, 1991). The prospects for empirical substantiation of Non Walrasian macrotheory, however, remain low due to the difficulties in the econometric implementation of models which allow for disequilibrium and incorporate rationing<sup>5</sup>.

It is interesting to note that nearly three decades of new paradigms in macroeconomic theory and policy have hardly gone beyond the ivory towers of the academic world. Raising this issue over ten years back Mankiw (1988) says:

".... The observation that recent developments have had little impact on applied macroeconomics is prima facie evidence that these developments are of little use to applied macroeconomists ....."

However, Mankiw hastens to add :

"Yet this conclusion is unwarranted ..... Just as Copernicus did not see his vision fully realised in his life time we should not expect these recent developments to yield high returns in the very near future".

Another 13 years have passed but the gap between the new macro theories on the one hand and how policies and forecasts are made on the other has not narrowed. My own premise is that theories will be used by practitioners only when their analytical and empirical foundations come to grips with the realities. A case in point is an evaluation of rational expectations by none other than Edmund Phelps who was, with Milton Friedman one of the earliest to attack the reigning Keynesian orthodoxy. His indictment runs as follows:

"The rational expectations movement is a kind of religion. It is not a scientific sort of enterprise.... Each recruit and each convert to the faith increases the power of the institution, the power to control the journals and control professional thought....*Neither theory nor evidence is on the side of rational expectations.....*"<sup>6</sup>

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<sup>5</sup> Some models have been built on these lines for Europe. See Sneessens (1981). For an attempt relating to India in this direction see Pandit (1995).

<sup>6</sup> Phelps (1988), italics mine.

Phelps goes on to illustrate how the new classical macroeconomics has been clueless on many persistent as well as sudden macroeconomic shocks that have characterised the 80's.

#### **4. Econometric Methodology**

The extent to which a scientific discipline is open to new ideas and approaches is a measure of its health. The enrichment of econometric methodology in recent decades leading to what is some times called "new econometrics" is not only natural but also welcome. The critical question however, is whether the new ideas are an outgrowth of earlier wisdom or a total negation of it. Since this is largely a matter of subjective assessment views would be considerably different. Diebold (1998) for example argues rather convincingly that the profession has learnt a lot over decades of macroeconometric modelling and the present methodologies are an outgrowth of this accumulated wisdom.

It must be noted that in many quarters disenchantment is with entire econometric methodology rather than just with structural modelling. The reasons for such disenchantment are not always the same. Some believe that the data bases are usually too fragile to warrant sophisticated statistical treatment. A more fundamentalist view would even argue that since economic data are not the result of controlled repetitive experiments, the basic laws of statistics e.g., Central Limit Theorems are not even applicable. At another end, economic theorists view econometric applications merely as caricatures of oversimplified economic theory and thus uninteresting if not misleading.

A fairly large part of the profession, however, is not against the use of econometrics as such. But many are genuinely put off by mindless and mechanical applications of the available econometric techniques. Much of the outcry against such work has indeed come from practicing econometricians themselves<sup>7</sup>. Criticism has relatively more often been directed against structural modelling because this is the area of applied econometrics which has occupied the centre stage for a long time and

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<sup>7</sup> Leamer (1983).

is even a kind of soft target. However, the disenchantment with macroeconomic modelling is neither uniform nor similar. At one end of the spectrum some feel that the CCM methodology has been misleading and has put empirical macroeconomic research on a wrong trail. On the contrary many think that the methodology is basically sound though there is a real need to incorporate new ideas. In between we have the LSE methodology which accepts structural models as being useful but wish to adopt a substantially different and rigorous methodology for model specification, estimation, hypothesis testing and simulation<sup>8</sup>. The LSE methodology may be summed up as “general to specific” in contrast to CCM which is specific to general. The concept of nested models is relevant in this context

The LSE group strongly feels that macro econometric model builders have increasingly deviated from the original thrust of the CCM, in particular, where hypothesis testing and model specification are concerned. This has in turn been attributed to the fact that since the seminal six equation model of Klein (1950) every successive model has been larger than its predecessor. Since most economy wide models today run into hundreds of equations many rules of the rigorous methodology are hard to implement. The basic question here is one of choice between, on the one hand, small models which can be analytically elegant and capable of being subjected to rigorous econometric methodology and on the other larger models which are far more useful to model users and forecasters. This has been forcefully brought home by Klein (1999) who cites examples of how small models can lead to misleading conclusions<sup>9</sup>. Let us now turn to specific problems.

## **5. Some Specific Issues**

In its original formulations CCM indeed had a somewhat purist and limited agenda addressed primarily to an academic audience. But the growing demands on it from policy makers, corporates as well as the wider professional community of economists has considerably widened the scope and need for structural modelling. In

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<sup>8</sup> See Hendry (1993) and Favero (2000).

<sup>9</sup> See also Phelps (1988).

response a four step methodology has got articulated and widely used by macroeconometricians (Klein 1971). These are<sup>10</sup>:

- a) Specification and identification of analytical models keeping in mind the objectives of the model and many other structural features relevant to the economy under consideration.
- b) Estimation of the model as best as one could with the available data base and diagnostics as per the state of the art.
- c) Validation of the estimated structural model by examining its ability to reproduce observed movements in key variables within and outside the sample period.
- d) Application of the model for forecasting and policy analysis by means of simulation techniques.

Clearly, taking each step is contingent upon a satisfactory outcome in the preceding step. The recent challenge to structural modelling has been concerned with each of the steps (a) through (d).

## **6. Model Specification**

The first and in a way a basic issue here though not often explicitly stated has been about the size of the model. Many economists who are in principle in favour of structural modelling have not been enthusiastic about excessively large models. Crudely put the question has been, "Do we really need to generate so many parameters to track the structure of the economy?" A related question is the cost of increased size in terms of the simplicity of the model and the efficiency with which it can be estimated. A possible way out is suggested as the four lettered dictum: "KISS" (Keep it sophisticatedly simple) by Zellner<sup>11</sup>.

The difficulty, however, is that once you step out of the textbook situation into the real world environment one faces complexly structured economies which cannot be captured by simple and small models. Dynamics, nonlinearity and a good measure

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<sup>10</sup> For a detailed exposition see Intrilligator, Bodkin and Hsiao (1996).

<sup>11</sup> Quoted by Diebold (1998).

of disaggregative treatment of different phenomena are hard to avoid if the model has to cater to different needs and to capture different cross currents in the economy (Klein, 1999). The level of disaggregation and the degree of comprehensiveness of a structural model has thus got to be a compromise between the demands that are made on the model and the neatness of the final outcome.

Historically, monetarist or, more widely, neoclassical models have involved fewer variables and fewer relationships relative to Keynesian models. But they have seldom been used effectively for forecasting or policy analysis. The burden of adjustment has typically been placed on a couple of variables. This has largely been possible due to the excessively high degree of aggregation and the assumption of perfectly competitive markets and sometimes even limited policy options that are permissible. The consideration that Model X gives you a more detailed analysis than does model Y cannot be dismissed *prima facie*.

The literature on real business cycles (RBC) which is regarded as a high point of non Keynesian macroeconomic theory is relevant in this context. Here we do have a system of relationships in term of observables and non observables which is by no means simple. The most celebrated model in this context (Kydland and Prescott, 1982) eventually involves the solution of a system of 15 equations to explain the interaction between technology, tastes, investment and information. This is despite the fact that the economy is treated as one homogeneous entity. Yet, treating the economy as a systems of simultaneous equations is dubbed to be misleading presumably because of its Keynesian flavour. In any case the dispute is by no means entirely new. Herman Wold argued long back that real world economies were recursive in their structure rather than simultaneous.

It has been recognised over the last few decades that macroeconomic phenomena are driven by the state of expectations. On this there cannot be any disagreement. But how these expectations are formulated and aggregated is a wide open issue. As argued by Phelps<sup>12</sup> rational expectations cannot be an undisputed answer. Be that as it may, it is not true that structural modelling rules out the role for

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<sup>12</sup> Op.cit. See also Pesaran (1987).



expectations or for that matter, rational expectations. In my view a major thrust of Keynesian revolution has indeed been its emphasis on the role of expectations. For many years model builders relied on adaptive expectations mechanism, or more directly observable anticipatory data. Obviously, there may be a scope for doing better including the use of REH. In fact, many of the existing large structural models do already incorporate rational expectations in some of their segments, e.g., financial markets<sup>13</sup>.

Finally, a major controversy regarding structural modelling under the CCM has been related to the variables in the model being classified as endogenous (or interdependent) and predetermined. The latter would typically include policy variables, international market developments for small open economy situations, natural factors and lagged endogenous variables. The classification is important in so far as it determines whether the model is properly identified or not. It is also crucial to the estimation procedure that one may adopt and also how the model may be solved. Needless to add here that whether a variable may be treated as predetermined or not would depend on how the model is structured and what the purpose of the model is. The latter would dictate what closure rules are followed.

An early and powerful attack on this part of CCM (What has been termed as “**causal ordering**” in section 2 above) came from Sims (1980) in a widely quoted paper. His charge was not only that the classification is ad-hoc, but also that zero restrictions on parameters in different equations is equally arbitrary; intended only to ensure that each equation in the model is identified (usually over - identified). While Sims' first charge cannot be dismissed outright, his second charge is either exaggerated or misplaced. That individual researchers may use arbitrary restrictions to ensure identifiability cannot be ruled out. Some researchers certainly are sloppy and much of empirical research turns out to be ad-hoc and thus useless. Since this is true in all areas of research, a methodology cannot be hanged because it is abused by some. Sim's first charge has since given rise to better and testable concepts of exogeneity namely weak, strong and super exogeneity with a statistical foundation (Engle, Hendry and Richard, 1983). The third of these concepts of exogeneity is

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<sup>13</sup> See Fair (1994).

relevant in the context of policy simulations. A battery of tests to check exogeneity is now indeed available and there is no reason why a model builder should not use the available diagnostics. It is in this contest that structural modelling has returned to Haavelmo's ideas<sup>14</sup>.

Turning to Sims' first charge it is not hard to see that while economic theory may often be open ended as regards the variables which should or should not enter a particular relationship this is not always the case. In either case the inclusion or exclusion of many variables is a matter of the context of the model. Thus, in any given situation the problem can in general be reasoned out either way on analytical basis or on specific contextual basis. The accumulated professional wisdom is usually there to provide guidance. But if the model builder ignores this and is allowed to do so this is a case of collective professional failure.

## **7. Estimation and Validation**

With regard to estimation the original CCM had strongly recommended maximum likelihood estimation either on an equation by equation basis (limited information ML) or, on the system of equations basis (full information ML). However, when computing facilities and the required software were not easily available the use of ML estimation procedures was rather cumbersome. Thus, perforce, models had to be small and preferably linear. The late fifties and early sixties saw the emergence of two stage and three stage least squares (2SLS and 3SLS) estimation procedures which proved to be very convenient particularly for linear models. Unbiasedness having been given up fairly early, attention was mainly focussed on ensuring consistency and efficiency<sup>15</sup>. Almost all inference came to be asymptotic in nature. Also, asymptotically 2SLS was as good as LIML and 3SLS as good as FIML. For small samples, however difference would persist.

Recent developments have raised two sets of problem as far as estimation is concerned. First, how to estimate models which include expectations variables particularly rational expectations. One of the early and not so difficult solutions

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<sup>14</sup> See Eichenbaum (1995).

<sup>15</sup> See chapters 4 and 7 in Fair (1984).

suggested by McCallum (1976) is based on an application of the 2SLS procedure. It has now been possible to improve upon this by using the generalised method of moments (GMM). Fair (1984) outlines how 2SLS procedure can be modified to estimate rational expectations in a single equation, with increased efficiency. The procedure, however, is somewhat flexible in the sense that one can choose a positive definite weighting matrix in different ways. To arrive at such a matrix is not always easy. Fair also explains how rational expectations can be handled under a system of equations framework using FIML together with the so called “extended path” (EP) method. Such a procedure is now also built into software packages like TROLL.

The second set of issues has arisen in the wake of new literature on time series analysis during the eighties. The main points that have been made are that regression equations involving nonstationary time series will yield “spurious” results. Moreover, if the residual error term is not stationary then the usual diagnostics are not valid. A variable  $x_t$  is said to be integrated of order  $k$ , where  $k$  is a positive integer, if  $\Delta^k x_t$  is stationary. Clearly stationary variables are  $I(0)$ . Further, a set of variables  $x_t$  said to be cointegrated if there exists a vector  $\beta$  such that  $x_t\beta$  is stationary.

Like most methodologies the currently popular time series methodology has got fairly standardised. Testing for unit roots (null hypothesis being that the series is nonstationary), presence of drift, existence of trend stationarity etc have become quite familiar steps in identifying what are known as data generating processes (DGP). Once this is done one moves on to testing for cointegration, separation of long run and short run relationships etc. The nice thing is that in all this one hardly needs to go beyond OLS. Thus, OLS enjoys a measure of renewed respectability under this methodology. All the same problems arise once one goes beyond two variables. Treatment of multiple time series is more complex and would in fact pave the way for modelling in terms of systems of equations. For now, let us return to the two issues raised earlier.

As far as stationarity of the residual is concerned there cannot be two opinions. Most of the statistical inference particularly the large sample results which rest on asymptotic normality are based on the assumption of stationarity. Hence the need to ensure that variables in a given equation are cointegrated, though they need not be

integrated of the same order. But once this is done does one get back to structural modelling. The answer to this is an emphatic “yes” on both economic as well as econometric grounds. Let us consider these as follows.

Consider first the questions raised by Sims (1980) and his solutions which I believe are simpler to deal with. He suggests that since we pay enough attention to economic theory and also impose zero restrictions on parameters in an ad hoc manner, we may assume that all variables of our interest are dynamically interdependent on each other. This leads to his vector autoregression modelling (VAR) which has now become quite popular. It is not denied that theory is not always strictly adhered to nor that exclusion of variables is often ad-hoc. Nonetheless there is no case for abandoning both economic theory as well as the belief that some variables can genuinely be treated as extraneous to the model. This is like throwing out the baby along the tub water, as it were. One only has to be guided by careful reasoning on both counts.

Moreover there are also some serious econometric difficulties. First, the number of parameters that need to be estimated in a VAR model gets very large if one works with only five or six variables and lag lengths of three or four. This puts one up against a serious degrees of freedom problems. Consequently, there are three way out. Either reduce the number of variables or cut down on lag lengths both of which seriously jeopardise the accuracy and relevance of the model. However, if one is lucky to have a good quality of high frequency data sets on all relevant variables, the degrees of freedom problem disappears Quarterly data for twenty years may be fine for a variety of small models. Last, but not the least, one of the major criticisms against structural modelling was “Do we need to generate so many parameters ?” What happens to that question? One is indeed back to square one. Besides, if one were only interested in forecasting VAR models would perform well though lack the detail that structural models possess. But if one is equally or more seriously interested in policy analysis VAR models have not proved to be very helpful.

Here, it must be noted that VAR modelling requires that all variables be stationary. Testing for stationarity itself is contingent upon the existence of a long enough time series. The power of these tests, as they stand, being rather low, it is not

surprising that many times one gets either indefinite or contradictory one inferences depending on which test is used. Getting a little deeper into the econometric issues, it has become common these days to argue that problems arising from nonstationarity nullify the methodology associated with structural modelling. This is indeed not true. In a recent paper Hsiao (1997a) proves that in a model with all variables integrated of order 1 OLS estimates of structural parameters may be inconsistent when regressors are cointegrated. On the other hand 2SLS estimators are consistent though their convergence in probability may vary across situations. This result indeed knocks out the basic premise of what time series modellers invariably do, namely use OLS.

In yet another paper Hsiao (1997b) shows that many concerns like simultaneity bias expressed by structural modellers do not disappear in the wake of time series methodology. Hsiao also shows how time series methodology can be usefully utilised in dealing with structural modelling. More specifically Hsiao deals with the following questions:

- a) Relationship between multiple time series models and structural equations models with or without cointegration,
- b) Concept of identifiability for nonstationary variables,
- c) Need for separate sets of identification conditions, arising in case long run and short run relationships are separated,
- d) Irrelevance of the simultaneity bias for models involving integrated regressors under superconsistency,
- e) Need for a new method of estimation under cointegration and the speeds of convergence of OLS, 2SLS, 3SLS estimation under cointegration,
- f) The limiting distribution of Wald type statistics under cointegration.

Without going into these issues in detail we only need to note that the bottom line in Hsiao's paper is that the issues raised under CCM remain legitimate and standard structural estimation techniques and testing procedures remain valid.

As far as spurious regressions are concerned these are, in principle, possible under all situations. The best way to deal with these is to bring into focus more of theory as well as structural factors which are based on widely held empirical

judgements rather than abandon both. To let the data speak for themselves is right upto the point that theory permits clear alternatives or is noncommittal. But in the absence of a maintained hypothesis it may lead to results which are hard to interpret. Hendry's simple rule, "to test, to test and to test" is an excellent one to follow.

## **8. Model Validation and Application**

As far as model validation is concerned structural modellers have conventionally depended on two criteria namely, Root Mean Square percent Error and Theil's U-statistic. While RMSPE measures an overall goodness of fit U will ensure that turning points are adequately tracked. Every careful researcher has to ensure that both of these are acceptable for the set of major variables in the model. Here it needs to be highlighted that thanks to the vast improvement in computing facilities simulation techniques have considerably advanced. Simulations can be either deterministic or stochastic. In the latter case we have a mean solution as well as a standard deviation which permits one to set confidence intervals for the mean solutions or mean forecasts. The deviation between deterministic solutions and mean of the stochastic solutions has, however, turned out to be small for moderately nonlinear and dynamic models, as expected. Clearly the deviation would increase with the degree of nonlinearity and the extent of dynamics (Fair, 1994). Here one is reminded of the work on real business cycles which has earlier been described as the crowning climax of the new macroeconomics and the associated new econometrics. How do Kydland and Preccott (1982), in their path breaking paper, test their model? Not by testing it against any alternative model or even an alternative set of assumptions/ parameters, but simply by calibrating the model for the observed time series for the US economy with what appear to be a reasonable set of parameters<sup>16</sup>.

Let me now turn to the celebrated Lucas (1976) critique of policy simulations in the CCM tradition. At the outset it needs to be noted that Lucas critique is not a matter of econometric issues alone but also a matter of macroeconomic theory. Yet, I must hasten to add that the basic principle enunciated by Lucas is valid, The central point of his critique is that models should identify the underlying behavioural

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<sup>16</sup> See Eichenbaum (1991).

functions. An implication of this in keeping with the REH is that as policy rule changes the parameters of the model elsewhere do not remain the same. While this view has been accepted by structural modellers there are two points that have emerged in response.

*First*, implementation of the Lucas agenda is difficult except for very simple models, which may at best be illustrative academic exercises. *Second*, it has been found that the inaccuracy arising from overlooking the problem is rather small. What macroeconometric model builders typically do is to alter the magnitudes of policy variables within feasible limits and not change the policy rules. Moreover the issue that Lucas raises was indeed not unfamiliar to the CC tradition. Only, it was set aside as a compromise. Clearly, this continues to be the case. The concept of super exogeneity developed by Henry et.al. can be utilised in this context.

Before I conclude this section it would be important to take a stock of the prevailing situation with regard to structural econometric modelling for forecasting and policy analysis. As discussed earlier structural modelling in the CCM tradition has been subjected to criticism on several grounds. While some of this is fair and well taken, some of it is unwarranted and / or excessively exaggerated. In any case three alternative methodologies, overwhelmingly focussed on macroeconomic problems, have emerged so far. These include vector autoregression or VAR modelling, general to specific or LSE modelling and GMM based modelling, applied largely to business cycle analysis. However, none of these has been able to dislodge the structural modelling methodology in the CCM tradition. This can be attributed to three factors.

*First*, many of the valid issues raised against the CCM have evoked a positive and constructive response. A large proportion of the structural modelling work today is not on the same lines on which it was in the sixties or even the seventies. It has been able to take account of all major developments in macroeconomic theory as well as in econometric methodology. *Second*, in these days of marketisation survival of a paradigm will eventually depend on its ability to fight for survival on the strength of what it can deliver. A lot of users – be they governments, central bankers, corporates

or international agencies seem to find structural models useful<sup>17</sup>. This is well illustrated by the world Project LINK which has been in existence for nearly three decades – much longer than any comparable project has survived, as far as I am aware.

*Third*, the alternative methodologies have not yet been able to establish a clear superiority either with regard to the macrotheory with which they are associated or with the econometric theory they use or their final outcomes as regards forecasting or policy conclusions. Mankiw's analogy of new methodologies with Copernican theory mentioned earlier does not seem to have been borne out by developments so far. Whether two to three decades is not long enough has yet to be seen<sup>18</sup>. Sooner or later ivory tower academic pursuits must come to fruition at the grass roots level. One thing we learn from the REH is that economic agents cannot repeatedly make systematic errors! *Finally*, we see considerable hope in the LSE methodology which, in a way takes the CCM back to its roots and does not question the potential of structural modelling as tools for policy analysis after correct model specification is established on the basis of rigorous testing procedures.

## 9. Some Concluding Observations

It is quite well known that India has had a long and respectable history of macroeconometric modelling<sup>19</sup>. Unfortunately, it is also a fact that until recently most of the models were a one time effort (Pandit, 1999). A new beginning has, however, been made recently with the emergence of models that are maintained and used for forecasting and /or policy analysis at institutional levels<sup>20</sup>. There is a considerable variation across the available models as regards their analytical basis, size and focus. Thus, if we go by the number and vintage of maintained models i.e., those which are regularly revised, updated and frequently used for forecasting and for policy evaluation, India is by no means over researched as far as macromodelling is

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<sup>17</sup> Calling this phenomenon as " Commercialisation" begs the question. A large number of researchers who still go by CCM with incorporated refinements includes academics of repute and competence.

<sup>18</sup> To assess how long is long enough, note that the Keynesian paradigm was widely accepted and well established within no more than two decades of the publication of the *General Theory*!

<sup>19</sup> This is also true of other forms of modelling particularly those related to planning.

<sup>20</sup> Mention may be made of the Centre for Development Economics (Delhi School of Economics), National Council of Applied Economic Research, Institute of Economic Growth, Reserve Bank of India and The Indira Gandhi Institute of Development Research.



concerned. On the other hand the need for such modelling activity has increased in the wake of India's new policy regime. It is also my view that it is econometric models which are likely to be more appropriate for policy analysis under the emerging era of market oriented economic systems.

In any case a large segment of the economy remains an uncharted territory. Even with regard to segments that have been explored we need to crystallise major hypotheses in keeping with the prevailing state of the art. Thus, the first task which awaits researchers is to bring about a greater measure of analytical rigour into macroeconometric modelling. This may possibly be done best by developing smaller well specified sectoral models with clear policy focus and rigorous econometric methodology. *Second*, with sophisticated software widely available it is no longer justified to avoid rigorous diagnostics in estimation and hypothesis testing nor to avoid more insightful simulations. Clearly, this should enormously improve the endproduct and enable us to produce better forecasts and more credible policy conclusions. *Third*, there are many problems which cannot be adequately dealt with by using only macroeconometric models either due to data problems or other analytical difficulties. Imaginative and selective use of CGE models in conjunction with macroeconometric models can be fruitfully explored. This would particularly be helpful if we want to infer microeconomic consequences of macroeconomic developments.

*Fourth*, it appears to me that future research and application in this area will have to move on two tracks. Of these one would develop smaller models which can be subjected to greater sophistication and help us to have a clearer understanding of the overall functioning of the economy. The other would pursue the present practice of developing medium to large models which may be used for more detailed forecasts as well as for comprehensive policy analysis. The two sets of models can be used also to serve as cross checks on each other.

*Fifth*, quite similar to the earlier suggestion we shall have to make a beginning with models based on high frequency data. Without abandoning those based on annual data. Limited exercises have already been made with monthly data. But given

the frequency and accuracy of the data on important variables it is not advisable to go beyond the quarterly data sets and that too in setting up relatively simpler models.

*Sixth*, an issue relating to the second point made above is the rivalry, bordering on to antagonism between time series modelling and structural modelling. We feel that the problems here are misperceived. It cannot be anybody's case that theory does not matter and therefore structural models must be abandoned. Nor is it justifiable that a number of genuine statistical issues brought up by the work on time series can be ignored. While structural modelling must pay attention to problems raised by non stationarity and or lack of cointegration there is no need to make a mechanical switch over to VAR modelling. Thus, I do not rule out a proper blending of structural modelling and time series methodology. The LSE methodology appears to be a development in the right direction and can be utilised effectively.

*Last but not the least*, macroeconometric models can serve a useful purpose if they are continuously reviewed, scrutinised and updated in the light of new data, new theories, new policy issues and new perceptions about how the economy functions. Meaningful life of a model is perhaps not more than three to four years.

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