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ON MODELLING THE LONG RUN IN APPLIED ECONOMICS

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I. OBJECTIVES

Every month or so any developed economy will produce large quantities of data which attempt to summarise the major features of the macro economy. The process that generates this data will be called the data generating process (DGP). The fact that new data keeps appearing will be taken to be sufficient evidence that the DGP exists and thus can be studied. I will assume that the objective of theoretical macroeconomics and of macroeconometrics is to determine the DGP, or at least segments of it. However, the actual economy appears to be very complicated, partly because it is the aggregation of millions of non-identical, non-independent decision-making units, such as households, corporations, and various financial and government institutions (cross-sectional aggregation). A further practical problem is that the observation period of the data does not necessarily match the decision making periods (temporal aggregation). It can be argued that even though the quantity of data produced by a macroeconomy is quite large, it is still quite insufficient to capture all of the complexities of the DGP. The modelling objective has thus to be limited to providing an adequate or satisfactory approximation to the true DGP. Hopefully, as modelling technology improves and data increases, better approximations will be achieved, but actual convergence to the truth is highly unlikely.

It is interesting to compare the situation in macroeconomics with that in some parts of finance, where the amounts of data is truly immense, relative to the previous experience of econometricians, whereas financial theory suggests that the DGP will be uncomplicated. Even here, surprises can occur when the data are analysed. Partly due to actuality of aggregation the macro economy is either stochastic or, if deterministic, it is of such a form that is most effectively described and investigated using probabilistic concepts. It follows, I believe, that methods based specifically on chaos will not have an advantage over truly stochastic methods, although this by no means eliminates the use of non-linear stochastic models (see Granger (1994)).

Even at this early stage of consideration it is possible to get into discussions of philosophical questions, such as: does probability exist, is it different from randomness, does the DGP exist, and so forth. Some of these questions are interesting and perhaps even important but I doubt if they are of immediate relevance to applied economics. To wait for their resolution would paralyse all modelling activity and I suspect that whatever final solution is eventually agreed upon will have virtually no impact on every-day applied work.

As concentration is being given to modelling the long-run this means that particular attention has to be paid to time series techniques (T large, M, N small, where T is the amount of data in the time dimension, M is the number of variables, N the number of 'regions' or 'agents') rather than to cross-section ($T = 1, M, N$ large) or panel (T small, M small, N large) data. Doubtless, in the future data sets with other types of dimensions will become available. Using the strategies proposed by Box and Jenkins (1970) a time series analysis contains three major components: pre-analysis of the data to help with model specification (sometimes called 'pre-testing'), model estimation and possibly a re-specification (including 'in-sample' evaluation), and finally the model evaluation ('post-sample' evaluation). Cross-section and panel data analysis typically excludes the third step and I think that it can be argued that this consequently produces a weaker methodology.

II. PRE-TESTING

The major object of the prior analysis of the data is to attempt to determine the main features of each individual series. It is important to be aware of these features to ensure that the model being built is properly balanced, that is the main features of the dependent varibles are represented somewhere amongst the independent variables. For example, if the series contains a strong seasonal component then in a linear model some of the explanatory series must contain a seasonal and together produce the same phase as in the dependent variable's seasonal.

The feature that is of particular interest for the topic being discussed might be called 'persistence'. Essentially this is a basic smoothness in the data which equates to the long-run component. There are a variety of ways that persistence can be defined. The classical definition in terms of deterministic trends was never properly founded, in my opinion, as 'trend' was never rigorously defined. To say that a series has a deterministic trend component $\mu(t)$ plus a non-persistent component, one needs to put constraints on the form of $\mu(t)$. Virtually everyone would agree that cos (t) is not a trend but log (t) can be one. One could constrain $g(t)$ to be monotonic but that may be considered to be too strong, as it excludes many polynomials. It is also clear that to only use linear trends or exponential trends, which is the general current practice, is far too constrained a group to represent the growth patterns seen in actual series.

A definition of persistence, called extended memory in Granger (1995), considers the conditional expected mean over a horizon h, or forecast with horizon h

$$f_{t,h} = \mathrm{E}(X_{t+h} \mid I_t) \tag{1}$$

where I_t: $X_{t-j}, j \geqslant 0$ consists of just current and past values of the series being forecast. If $f_{n,h}$ does not tend to a constant as h becomes large, so that current information helps one forecast into the indefinite future, the process is said to have 'extended memory'. ('Long-memory' would be a better phrase but it is already used up describing a different property of some time series.) An example is the random walk

$$X_{t+1} = X_t + \epsilon_{t+1} \tag{2}$$

where ϵ_t is iid with zero mean, so that $f_{t,h} = X_t$, all h. The advantage of the definition (1) is that it does not just apply to linear processes.

An alternative definition by Lee (1992) emphasises the future impact of current shocks on the process. It should be noted that these definitions of persistence all apply just to a single series. They are based on the idea that whether or not a series is persistent can be determined just from its own data, without reference to other series.

The simplest form of persistence occurs with the unit root process X_t, where

$$X_t = X_{t-1} + Y_t \tag{3}$$

and Y_t is stationary, with mean m. If $m = 0$, the unit root process has no 'drift', otherwise X_t contains a drift term which is the linear trend mt. These are examples of processes that are integrated of order one, denoted I(1). Strictly an I(1) process X_t is given by (3) where Y_t is I(0), integrated of order zero. However, this just raises the question of how I(0) should be defined? A stationary series will be I(0) but not all I(0) series are stationary. An example is the AR(1) model with coefficient $\alpha \cos wt$ with α less than one in magnitude. (It is also true that I(1) series may be non-stationary but not all non-stationary series are I(1) as the example just given illustrates. Unfortunately, it has become commonplace for economist and econometricians to incorrectly call I(1) processes 'non-stationary'. An equivalent mistake would be having a variable with a t-distribution but calling it 'non-Gaussian'.)

An important part of the pre-analysis phase of modelling is the application of unit root tests. The reason is that many macroseries seem to be persistent and so a null hypothesis of I(1) seems reasonable, against an alternative if I(0). For just a few series a null of I(2) seems to be a possibility, this is discussed later. There are a number of tests available with I(1) as the null and I(0) the alternative, including several due to Dickey and Fuller (1979). It is often forgotten by researchers that if the null is not rejected this does not mean that the process has a unit root. The reason is these unit root tests have a great deal of difficulty in differentiating between many near unit root processes, including a process called 'stochastic unit root' by Granger and Swanson (1994) where

$$X_t = \alpha_t X_{t-1} + Y_t.$$

Y_t is stationary and α_t is a stochastic process not caused by X_{t-1} and with $E(\alpha_t) = 1$. Such a process will be sometimes explosive, other times stationary and occasionally will have a unit root, but simulations find that a null of I(1) is rejected only at about the standard rates by the Dickey Fuller tests. What is important is that these alternative models have different properties than pure unit root processes. For example, the variance of a unit root processes increases proportionally to t but for a stochastic unit root process the variance increases exponentially with t. (Further discussion of these processes can be found in Leybourne *et al.* (1996).) Another class of models that have been found to not be rejected by these I(1) tests are I(0) series with broken linear trends (see for example Perron (1989)). This observation resulted into some rather fatuous literature in which the question asked was whether a given economic series seemed most likely to be either a unit root process or stationary with a broken straight line trend. The question is uninteresting because only these two possibilities are considered whereas there are many other possible models that are not rejected by an I(1) test, including stochastic unit roots, unit roots with sub-linear deterministic trends, processes with stochastic non-linear trends (as discussed by Granger *et al.* (1995)), some fractionally integrated processes plus deterministic trends and so forth. The question that matters here is whether a series appears to be persistent or not, and if it does not reject a null of I(1) it can be taken to be persistent, even if it is not necessarily I(1). I will not get into a discussion of transformations of variables here, but it seems to be generally true that if $g(x)$ is a monotonic function, then if x_t is persistent so will be $g(x_t)$. It is shown in Granger and Swanson (1994) that if $\log x_t$ has a pure unit root then x_t will have a stochastic unit root.

III. COINTEGRATION

The original theory of cointegration concentrated on a pair of series, each of which was I(1), but for which there was a linear combination that was I(0). The modelling procedure that corresponds to this is to find series that did not individually reject a null of I(1) but for which there is a combination that does reject this null, in the direction of I(0). The various implications of the theory, such as the existence of a attractor and the idea of some form of equilibrium are often then taken over to the empirical application. Unfortunately, the theory is based on a number of assumptions which are not tested in practice, such as that the variance of the cointegrating vector, $\mathbf{z}_t$, is constant or, at least not growing with time. It is unclear what it means to say that there is long-run equilibrium when the equilibrium error has a variance that is exploding over time, for example, if the process is I(0) in mean but I(1) in variance.

It is easy to extend the theory to the case of a vector of m I(1) variables, having $r < m$ cointegrations between them, and thus $m-r$ common stochastic trends, which are necessarily I(1) but need not be random walks. The empirical implementation of this theory, due to Johansen and using maximum likelihood procedures followed fairly quickly. There are several books discussing this work, including Engle and Granger (1991), Banerjee *et al.* (1993), Johansen (1995) and Hatanaka (1992). The formulation of the likelihood estimators and their related tests is a substantial achievement but they are not without problems for the inexperienced user. The number of lags to use in the basic VAR is at the users choice, and different choices can affect the results and interpretations. There are also quite unrealistic expectations amongst users who seem to forget that information about long-run components accumulates slowly, that results about 'super-efficiency' of estimates concern asymptotics and may not be dominating in our typical sample sizes, that if many lags are used in the VAR the model being estimated is far from parsimonious, and that it is ridiculous to try to interpret each coefficient in these models, particularly when the cointegrations are not identified, in the classical sense. Attempts to produce identification by relying heavily on information that is not in the sample but is in some theory, for example, may give interesting results but historically econometric models that have been forced to obey some economic theory have usually performed less well than unconstrained models. When it works well, the applied economist is left with an estimate of the number of cointegrations, estimates of r linearly independent combinations of the original variables (assuming they all do not reject the null of I(1)) that are I(0) and the error-correction model. It is possible to directly derive an estimate of the $m-r$ common stochastic trends ($\mathbf{W}_t$) in the system, using the identifying criterion suggested by Gonzalo and Granger (1995), which is that the disequilibrium measures ($\mathbf{z}_t$) do not cause these trends at very low frequencies. They suggest that $\mathbf{z}_t, \mathbf{W}_t$ have a parallel interpretation for the system that the classical 'transitory' and 'permanent' components do for a single series, such as income.

From this basic approach there are a number of natural generalisations. Johansen allows the vector of variables to include some that are I(0), which just makes these individually 'cointegrations', makes the analysis more complicated but adds little to the interpretation. The I(1) assumption can be replaced by I(d) where d is a fraction, so that some of the variables are fractionally integrated. Outside of some special cases in finance, I do not think that this is an especially plausible class of models to consider and am not convinced that they have been widely discovered in practice in macrodata. A lot of attention recently has been paid to the possibility that economic variables are I(2). In fact Harvey often specifies that the common stochastic trend terms is I(2), although this option can be removed in the program that he coproduced. I find this class of models unlikely to occur in economics because if they are strictly believed they have unappealing impulse response functions. If the second difference of a series x_t is the white noise of ϵ_t then

$$x_t = \epsilon_t + 2\epsilon_{t-1} + 3\epsilon_{t-2} + 4\epsilon_{t-3} + 5\epsilon_{t-4}, \text{etc}$$

so that a unit shock to the economy in the First World War has bigger impact on the current value of the variable than one in the Second World War which itself has bigger impact than one in the Gulf War. If something does not feel right it probably is not right. One explanation for the occasional classification of variables as being I(2) rises from Perron's (1989) observation that an I(0) series plus a broken line trend is not rejected by a test having I(1) null, then similarly it is presumably true that an I(1) series plus broken trends of some kind will have a difference that is not rejected by this same test and null.

At this level of analysis and if one is considering the relationships between a fairly small number of series which do not contain strong seasonal components, the suggested steps in the analysis are:

(i) Plot the series against time.

(ii) Plot the series against each other in pairs, X_t on one axis, Y_t on another.

(iii) Do some formal pretesting to classify as I(0), I(1), or I(2), where I(1) means 'does not reject a null of I(1)' by some test. If an I(2) classification is suggested, plot the change of the series against time and perform a test for a broken line trend.

(iv) For series that are I(1), do a preliminary analysis in pairs using the Engle–Granger (1987) test procedure to find possible evidence of simple cointegrations.

(v) For these same series, use the Johansen maximum likelihood procedure to find cointegrations and the error-correction model for these variables.

(vi) Use the Gonzalo–Granger (1995) technique to estimate the common stochastic trends in the system.

(vii) If there are important I(0) variables in the original set of variables, expand the error-correction model by adding equations for the levels of these variables and add lagged levels of them in the other equations.

(viii) If the classification suggested that there were some I(2) variables, include their first differences in dependent variables and their lagged first differences in the error-correction equations and also add a broken line trend as a further explanatory variable in all relevant equations. Usually this step will not be required in the analysis.

I favour use of a general to specific modelling strategy in constructing the error-correction equations, starting with either a predetermined number of included lags and explanatory variables, or possibly the lag length could be chosen by use of the BIC model-selection criterion which can be found defined in most modern econometrics texts, including Granger and Teräsvirta (1994). Insignificant coefficients are then dropped and the equations re-estimated. It is important to emphasise that, at best, the resulting model will be an approximation to the generating function, that individual coefficients cannot be interpreted in general and that changing the list of valuables under consideration will change the results. The model can be used for forecasting purposes, for (Granger) causality testing (with the usual care taken with interpretation, as explained in Granger (1980)) and for impulse response investigations. It is strongly suggested that the model not just be specified and estimated, but that some post-sample tests be applied to it, such as comparing its forecasting ability to alternative models. This is, of course, standard practice in time-series modelling.

IV. GENERALISATIONS

Anyone who looks at real economic data would probably agree with the statement that the simple classification involving just the choice between I(0), I(1) or I(2) is insufficiently subtle to capture the many types of persistence or non-persistence that occur in actual series. However, it is also clear that many series are persistent. If this persistence is locally linear a unit root process will provide an adequate approximation as it adapts well around all slow changes or even rapid changes. Personally, I learned this lesson well in the mid 1970s when I was comparing the forecasting performance of a well respected simultaneous equation econometric model, developed at the St Louis Federal Reserve Bank, with simple univariate Box–Jenkins models. For a time they performed roughly equally well but when the first oil price shock occurred in 1973/4, the econometric model was completely unable to cope and produced unhelpful forecasts whereas the Box–Jenkins model, which included a unit root, was disturbed during the actual shock but quickly adapted and started to produce forecasts of the same quality as those prior to the shock. However, the fact that an adaptive model, that is one involving a unit root, provides a good fit to data does not mean that it is the correct model. I believe that a regime switching model, that is adaptive around times of structural change and is more structural and based on economic theory in calmer periods could be superior.

Regime-switching models are examples of a wide class of possible generalisations that include various time-varying Markov–Switching (see Hamilton (1994), chapter 22) and non-linear specification models (see Granger and Teräsvirta (1994)). A wide variety of non-linear error-correction models have been considered by Granger and Swanson (1996), in which the error-corrections (zs) and the common stochastic trends are each generated non-linearly, but there is still a linear cointegration between a pair of variables, X_t, Y_t. A great deal of work has to be done before the full potential of these models is realised. In my opinion, not a great deal of non-linearity will be found for most quarterly observed, macro variables, because of the effects of temporal and cross-sectional aggregation but important effects should be found in variables such as interest rates and, possibly, exchange rates.

The results so far available suggest two practical implications, that adding non-linear error-correction terms, such as $|z_t|$ or $z_t^+ (= z_t \text{ if } z_t > 0, = 0$ otherwise) can give improved fit and occasionally improved forecasts, and that even if a variable is not strictly I(1), but is persistent, use of the error–correction model construction procedure as discussed in the previous section will produce an excellent estimate of the common stochastic trends W_t, even though strictly the error–correction model is misspecified. Thus, using step (viii) followed by (vi) could produce a good estimate of the common broken line trend, if it is common.

V. WHAT CONTROVERSY?

In my writings I have usually been careful to avoid controversy, although occasionally it has found me. Attitudes towards modelling vary greatly and may therefore be considered contraversial. The extent to which a model uses, or is based on, an economic theory is an example of a question which produces substantially different answers from different workers, My own attitude is to build an unconstrained model, but one that is wide enough to encompass a theory or theories of interest, and then to test if the theories appear to be supported by the model. Use of non-numerical information, such as personal beliefs or experiences as suggested by the Bayesians should be considered, has also been known to produce some disagreement. I personally lack sufficient self confidence to be a formal Bayesian, stating a specific prior, but am happy that not everyone has this personal characteristic.

My strongly held conviction is that there is little or no controversy about how to model in economics, provided these models are being used in a real decision making process or can be evaluated as though such decisions are being made. A model can only be properly evaluated by comparing its output with the product of other models. For this we need many models built in different ways, it would be a disaster if for the empirical investigation of the economy every applied worker approached the modelling process in exactly the same way. It is impossible to decide between models on purely intellectual grounds, one individual can only say why he or she may have a preference. Given the choice between an apple, an orange, or a banana, one person could say that the public will not buy a fruit that is not yellow or another that he prefers fruit that is spherical but the only way to decide which consumers really prefer is to make all three fruit available and see which is purchased. The equivalence with models is to make several available and see which is selected and used in practice. This choice is made easier if a sequence of outputs, such as forecasts, are made and can be evaluated. The parallel concept of the price of the fruit in my example is the human capital required to use a modelling technique, if a method is very complex and needs a great deal of experience before it can be used with confidence it will be viewed as expensive compared to a simple or pre-packaged, automated procedure. There is therefore little controversy if we agree to try several modelling methods and let competition decide between them. We should expect evolution of the dominant model to occur, as we saw the simple non-dynamic contemporaneous equation replaced by the slightly dynamic simultaneous equation system which was itself replaced by vector autoregressions which evolved into linear error–correction models. There is every reason to believe that further evolution will occur in the future. This process of learning does involve decisions, a sequence of outputs from the models and an agreed procedure for evaluation. Unfortunately, it does not apply to many aspects of econometric modelling, such as impulse response estimates or models designed specifically for policy purposes.

The obvious difficulty with this 'selection by competition' belief when applied to long-run modelling is that as information about the long-run accumulates very slowly, it takes a long time for discrimination between the successful and the less successful models to occur. Whilst we wait, undoubtedly other ways to present opinions about preferences will be given, but should be treated with care.

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