The Methodology of Econometric Research

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In the 1970s many of the macroeconometric relationships which economists had previously taken for granted broke down. One consequence of this was the backlash against "Keynesian" economics and the re-emergence of previously discredited economics disguised as new theories of "expectations". But in narrower circles there was a concern that it was the econometrics rather than the economics that had been at fault. This prompted a methodological debate which is still with us. The length of this debate is due to the fact that it has come to embrace some fundamental issues in the methodology of economics as well as that of econometrics. There was (and is) a growing disillusionment at the inability of econometrics to perform what should be its basic function - to empirically corroborate or otherwise competing economic theories. It is perhaps ironic that this same malaise is present in the methodological dispute - the competing views are partly incomparable because they are stated in different terms. In economics this has meant that theories are only tested on the basis of internal logic, and the only weapon an economist has is to attempt to discover logical flaws in rival theories and so it was that a central controversy in the world of economics in the 1950s and 1960s was the "Cambridge controversy" a rather esoteric dispute about the nature of capital. Realism, explanatory power etc. all go out the window in the quest for an internally consistent theory. This esssay attempts to draw together the various opinions on what is the methodology of econometrics. I will discuss each approach on its own terms, and where possible compare it with other approaches. Departing from the tradition in economics, I will come down firmly in favour of one particular approach.

An obvious question to ask at this stage is why bother discussing methodology at all? Econometrics will reach certain conclusions based on enquiries conducted in accordance with a definite policy for obtaining and assessing evidence. As Nagel says, the rationale for confidence in those conclusions must be based on the merits of that policy. He says that understanding the logic by which conclusions are established is the task of the philosophy of science. It could be argued that he has to say this to justify his writing a rather long book on the topic. At times philosophers of science such as Nagel tend to go to extraordinary lengths to establish what seems like an insignificant point and at such times one tends to agree with Feyerabend's comment that philosophy of science is

> "one of those bastard subjects ... which have not a single discovery to their credit".

However, given the fact that no one believes anyone else's econometrics any more, I believe that methodology is worth discussing. The best place to start is with a series of objections, which were raised almost 50 years ago to 'conventional' econometrics, which is the current textbook approach to the subject - Koutsoyiannis's book is a classic example. She outlines a step by step approach to econometrics, which I believe is fundamentally flawed every step of the way. These flaws were amply aired by Keynes in a review of a book by Jan Tinbergen in 1939. A contrast of these two approaches is particularly revealing.

Stage I of the textbook approach to econometric research is specification of the model. This involves deciding on your dependent and explanatory variables, the mathematical form of the model and stating a priori expectations regarding the sign and size of the parameters. This stage brings with it a corresponding assumption - that your model is correctly specified. This assumption has major implications for the procedure followed from here or in the research. Keynes asks

> "Am I right in thinking that the method of multiple correlation analysis essentially depends on the economist having furnished not merely a list of the significant causes ... but a complete list?"

If you do in fact leave out an important variable you don't obtain estimates of what you think you are estimating - as Keynes puts it

> "The method is only applicable where the economist is able to provide beforehand a correct and indubitably complete analysis of the significant factors."

It is at the specification stage that most attention has focused and I will return to this topic later.

However, the applied econometrician spends most of his time estimating relationships, which in practice means evaluating computer printouts. Here, Keynes identifies a host of problems which have increased in significance since Keynes' time. He mentions

> "the frightful inadequacy of most of the statistics making spurious correlations from proxy variables being unable to separate the distinct effect of multicollinear variables, assuming linear forms, confusing cause and correlation ... and confusing statistical with economic significance."

He then tops off this list of failings with a damning question:

> "If the method cannot prove or disprove a qualitative theory and if it cannot give a quantitative guide to the future, is it worth while?"

All in all, his article is a fairly comprehensive destruction of the "average economic regression" (AER) approach to econometrics, which he sees as "statistical alchemy" and in this respect worse than black magic. Having dealt rather tersely with the AER approach to econometric research I will now turn to the proposed alternatives. In particular I will analyse the contributions of Edward Leamer and David Hendry.

As I mentioned earlier the crucial issue is specification. Leamer contends that economic theory will never generate a complete specification, and therefore the actual variables used in your model will depend on what you believed beforehand. The basic problem facing researchers is that you can never be sure what variables you have left out, and what bias is emerging in your estimates as a result - this is the problem of specification uncertainty. The data will give you no information about the size of the bias, which means you must decide independently of the data how good the 'non experiment' is. For Leamer, the crucial difference between experiments and non experiments, between the natural and social sciences, is that the specification bias is larger in the non experiment. The only way (in principle) the problem can be overcome is to include all the relevant variables in the regression - but as he shows, you can always find a set of observations that will make the inferences implied by a model with one less variable seem silly.

> "There is no formal way to know what inferential monsters lurk beyond our immediate field of vision." (McAleer, Pagan and Volker).

In (necessarily) limiting your field of vision, you will make what are essentially "whimsical" assumptions. It is therefore no good simply to report the particular regression that resulted from your arbitrary assumptions. Your inference should be robust, i.e. it should be able to withstand changes in the assumptions. As an alternative to reporting a single inference, he suggests "Extreme Bounds Analysis" (EBA). The researcher explicitly states his prior beliefs in the specificaiton process and then attempts to evaluate the validity of these beliefs in the light of his data. The role of econometrics is to determine the range of inferences implied by a closely related range of models. The goal is to have a narrow range of inferences implied by a broad family of models. The centre of attention should not be the regression equation itself but the mapping from assumptions to inferences - "the mapping is the message". You must show how you arrived at the inference and examine its sensitivity. In short, critical attention to the words "whimsy" and "fragility" would be the salvation of econometrics.

This sounds fine in theory. But the acutal methodology of EBA is guilty of exactly the same flaws that Leamer criticises in conventional econometrics. It involves you stating what you believe to be your "important" and "doubtful" variables. You then manipulate the "doubtful" variables and hope that this will lead a reasonably stable value for the co-efficient you are interested in - the "focus variable". Leamer points out that opinions are whimsical

> "sometimes I take the error term to be correlated, sometimes uncorrelated, sometimes normal and sometimes non normal ... does it depend on what I had for breakfast?"

But we can just as well ask - does his choice of what is an important variable and what is a doubtful variable depend on what he had for breakfast? It has been pointed out that in a model with 9 variables there are 181,440 conceivable partitions of important and doubtful variables! Thus even if one were to consider the ludicrous idea of a fragility analysis of your fragility analysis, this task would in practice be impossible. The same article has drawn attention to the fact that EBA assumes that the error terms are normally distributed, non-autocorrelated and homoscedastic - precisely the conditions which do not arise in applied econometric research. These are obviously major flaws in his approach. However it has to be said that the property which he seeks of an inference is obviously desirable. On his own example where the inference is not robust, practical applicaiton of a single reported inference could have literally lethal consequences! This is why Kennedy correctly chooses it as a desirable general principle for model evaluation. I do not believe however that you derive a general philosophical approach to econometrics from just one principle. The Hendry approach has a rather more extensive basis, it is to it what I will now turn.

Nagel points out that for a social science to be 'scientific' does not mean that it must be able to carry out controlled experiments. What is required is "controlled empirical enquiry", a clearly defined method of analysing non experimental data. The form of enquiry that is pursued in economics is the "ex-post facto experiment". What distinguishes this from a natural science experiment is that the relevant factors cannot be overtly manipulated. Control is achieved if sufficient information about these factors can be secured. The crucial point is that the subjects manipulated are the data of observation on relevant factors, and not the factors themselves. This is probably the single most forgotton fact in econometrics - that in the strictest sense econometrics is just 'number crunching'. The data we actually collect were generated by a certain data generating process (DGP) which we can never hope to know. The best we can do is design a model which approximates as closely as possible to the DGP, which "adequately characterises the data". The simplified representation of the DGP cannot be strictly valid; the best it can be is adequate - in the jargon, we look for a "tentatively adequate conditional data characterisation". A model is never right or wrong, but useful or useless for a particular purpose. This contrasts with the AER approach where the model is treated as axiomatically corrct. As Gilbert points out this has major implications for the procedure following specification, because it means that poor test statistics imply problems in consistently and efficiently estimating the parameters of the model. It does not imply problems with the model itself. On this view, the econometrician must worry about the pathology of his estimates - Part II of Koutsoyianis is called "Econometric Problems", and the chapters are entitled "Autocorrelation", "Multiple Collinearity" etc. Each chapter follows the same format - the assumption, its plausibility, the consequences, the tests and crucially the 'solution'. This 'solution' usually involves adding in extra variables and generally tampering with the form of the model. It is a movement from the simple to the general. It is what she herself describes as the 'experimental' approach, but I find that a label that has been applied to Hendry's approach is more accurate - 'kitchen sink econometrics'.

As far as Hendry is concerned however, poor test statistics imply model misspecification. He starts with a very general hypothesis and then looks for simplifications that are acceptable based on the data. This process has been described as 'testimation'. This model must conform to certain previously laid down criteria - it must be data admissable (it must be logically possible for the model to have generated the data), theory consistent etc. An example of the difference in approach is provided by the autocorrelation problem - for Hendry this implies a systematic ofrecasting error, therefore your model must be respecified. The AER view says you correct for autocorrelation by re-estimation. One of the most crucial requirements of the model is the encompassing principle - that your model should be able to predict the results of alternative models, their successes and failures. The main argument against all this is that the general to simple approach essentially involves 'data mining'. It can lead to complex looking empirical models containing a wide variety of variables and lags without any theoretical basis for their inclusion. Hendry's reply is that theory will never provide a guide to all situations that will arise, and that the interaction between theory and data can be two way.

Apart from coping better with the limitations imposed by ex post facto experiments, this methodology has a second major advantage, related to the testing of economic theory. Economic theory is usually stated in terms of "latent variables" i.e. unobservables such as expectations, equilibrium etc. The choice of proxy variables for these is just as crucial as the normal specification process. Hendry explicitly concerns himself with this "mapping from unobservables into observables" and has developed techniques for coping with the problem which I do not propose to outline (because I have not a clue what is actually involved). I shall give him credit for trying! In general philosophical terms, also his approach is much closer to economics - in particular Friedman's idea that all models are false and that economics is a process of 'as if' theorising. It is also particularly well suited to the rational expectations revolution which should be another factor in its favour.

This arises because Hendry deals explicitly, as previously stated, with "latent variables", expectations being the most latent variable of all. A final advantage of the Hendry approach is that it is reasonably well grounded in existing techniques and so would not require a dramatic revision of econometric methods for its implementation.

In conclusion, therefore, I have argued that the conventional methodology of econometric research as presented by, for example, Koutsoyiannis is flawed, and is generally recognised as such. Despite difficulties in comparing proposed alternatives directly, it was concluded that the approach offered by David Hendry was the best way forward. Indeed, there are signs that this view is gaining wider acceptance within the profession. A sure way of judging this is by the content of more recent textbooks. For example, Kennedy, takes a distinctly Hendry type view of what econometrics is about, and he incorporates the criteria put forward by both Hendry and Leamer in stating what constitutes a 'good' model. If the methodological debate has prompted a general move in this direction then it will not have been a waste of time.

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References:

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