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THE RUDIMENTS OF ECONOMETRIC METHODOLOGICAL DIVISION

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Significant differences exist between the econometric methods practiced by economists today. Colm McCaughley analyses the trade-offs between the key approaches to applied econometric methodology and is finally able to conclude that the field of study is enhanced by its diversity. This finding strengthens the case for augmenting traditional economics with psychology and neuroeconomics.

Introduction

Much of the division within modern applied econometrics originates in a famous debate which took place in the late 1940s. The debate concerned the role of data-led as opposed to theory-led econometric modelling. From this debate, an average economic regression (AER) approach had found its way into the mainstream of much econometric applied work during the 1960s. However, in the 1970s practioners became weary of critical problems within the AER and applied econometric methodology began to fragment. A fundamental division emerged between a British Tradition under the paladin of the LSE and David Hendry, as opposed to North American approaches led in separate dimensions by Ed Leamer and Christopher Sims. Such divisions remain to the present where noticeable differences often appear between econometric methods in prominent journals. This paper seeks to analyse the rudiments of these differing approaches. The methodologies presented are recognised as cornerstones of what has now become a diverse field of econometrics.

The Principles of Methodology

The essence of econometric methodology is the formulation of a framework which seeks an adequate "conjunction of economic theory and actual measurement, using the theory and technique of statistical inference as a bridge pier." (Haavelmo cited in Pesaran & Smith, 1992: 9)

To the unknowing economist such an interaction or mix of theory and observation may seem peculiar - the view that there always exists a strict dichotomy between theoretical and empirical activities, that 'theorists create theories and econometricians solely test', is not uncommon. In reality however, econometric modelling is viewed not as a simple test of theoretical relationships nor as a method simply to validate economic theories but as an "endeavour to understand observable economic phenomena," achieved through the use of "observed data in conjunction with some underlying theory in the context of a statistical framework." (Spanos cited in Pesaran & Smith, 1992: 12)

The basis of most methodological approaches are derived from ideas within economic philosophy.[1] Philosophers and economists have long debated the scientific status of economics. In doing so both are habitually concerned with a number of philosophical difficulties which plague econometric practice. Issues concerning falsification,[2] induction,[3] and recognition of the 'Duhem-Quine problem'[4] all affect the interplay between theory and data. Considering the words of Haavelmo above, if theory and statistical technique form the 'bridge pier' then these problems perhaps represent the icy water beneath.

Attempting to limit and control these difficulties may be seen as the underlying task of the econometric methodologist, in doing so he/she seeks implicitly to provide rigour to economic inference. In this regard, the principle division between all econometric methodologies is in how exactly they contend with the difficulties which plague the interaction between theory and observation.

[1] See Hausman (1989) for a review of the philosophical aspects of economic methodology.

[2] Falsification is "the contradiction of a general rule by particular observation" (Pesaran & Smith, 1992: 4).

[3] Induction is "the inference of general rules from particular observations," the philosophical difficulty being that past observation does not necessarily imply the existence of a general rule in future observation. (ibid.)

[4] The Duhem-Quine problem acknowledges that since any theory is inherently complex, to apply a theory to data requires auxiliary assumptions (regarding the disturbance term for example). In rejecting a theory based on observation one can never really be sure which component of theory or assumption has been falsified. (ibid.)

The AER Approach

The Cowles Commission for Research in Economics (1939-1955) is widely recognised as the first attempt to solidify general methodological foundations for applied econometrics. Although the strict ideas of the Cowles Commission were initially of limited influence on practical econometric techniques, it did set in motion a fundamental focus on methodological issues. By the late 1960s the methodological debate had arrived at one approach which emerged as the standard paradigm of applied econometric modelling. The so-called average economic regression (AER) will be familiar to many newcomers to econometrics as it is often used as a starting block in econometric pedagogy. (Christ, 1994; Keuzenkamp, 2000). The basis of the AER approach is that theory suggests both the "form and specification of the statistical model to be estimated" (Bond & Harrison, 1992: 316). A common example is a specification of the form,

$$y=X\beta + \varepsilon$$

The econometrician tests hypotheses relative to this structure but assumes the structure itself is correct. The econometrician will be concerned with the 'pathology' of the estimators when conducting such tests. Thus problems such as heteroscedasticity, serial correlation and multicollinearity will all be considered. In assessing the impact of these problems the Durbin-Watson statistic, coefficient t-ratios and so forth are all pivotal. The econometrician's response to the discovery of a pathological issue is to re-specify the model (usually by adding or subtracting variables) or to substitute for a more appropriate estimator (perhaps through a Cochrane-Orcutt transformation). Eventually the practioner will end up with a model which displays "all the correct signs, statistically significant coefficients, a Durbin-Watson statistic of around 2, a relatively high R^2 and so forth." (Gilbert, 1986: 283; Gilbert, 1998)

The fundamental problem with this approach is that two separate practioners each starting from a different theory may each be able to formulate a 'valid' model from an identical data set. Each will justifiably claim that the data supports their particular theory: the reader is left with no discernable scientific method to validate either claim. This problem stems from the very essence of the AER approach which was built around *estimation* rather than *testing*. The practitioner would typically interpret poor results as the need to re-estimate rather than a refutation of the model. Using

econometrics in this manner then becomes a data-mining[5] exercise where the data is thrashed until it eventually reveals a model with the pleasing characteristics described above. (Darnell & Evans, 1990)

The recognition of this significant problem led to much dismay in the early 1980s on the tenuous nature of econometric practice. Some practitioners[6] set about to revise the AER approach, others however considered its methodological foundations as damaging to credible applied econometrics. Work such as that by David Hendry (1980) 'Econometrics - Alchemy or Science?' and Edward Leamer (1983) 'Let's take the Con out of Econometrics' enunciated the formal pursuit of alternative methodological approaches.

The North American Approaches

The main methodological approaches in North America were led in separate directions by Sims (1980, 1982) and Leamer (1978, 1983, and 1986). Sims promoted the Vector Autoregressive (VAR) methodology which emphasised time-series, a theoretical modelling seeking to 'let the data speak'. Leamer, conversely, advocated more structurally based models which, like the Cowles tradition, placed strong emphasis on theoretical underpinnings.

The work of Sims was initially developed in line with the Friedman methodology[7] but diverged into a unique approach during the 1970s. VAR models were intended to reduce the scope for data-mining by including all available variables. The only role for theory was to suggest which variables would be included but there would be "no exogenous variables [and] no identifying conditions" (Pesaran & Smith, 1992:13). Rather estimation of vector autoregressions (VARs) and analysis of moving average representations[8] (MARs) were to determine model specification and exact formulation. In this manner, data not theory would provide the substance to the model.

Due to developments in co-integration and non-stationary series, Gilbert (1998) argues there has been considerable convergence between the VAR

[5] Data-mining consists of moulding or selecting models based only on ability to pass desired statistical tests rather than underlying theory. The result being misleading and biased research. (Hansen, 1996)

[6] Notably Darnell & Evans (1990: 64) who propose a 'variant of the traditional approach' built around a specified search strategy and a greater emphasis on testing.

[7] See Frazer & Boland (1983) for a detailed account of Friedman's methodology.

[8] See Darnell & Evans (1990:118) or Gilbert (1998: 113) for an explanation of VARs and MARs.

methodology and the British tradition.[9] In this respect, greater emphasis shall be given in this section to the 'sensitivity' approach of Leamer. Leamer (1978, 1983) argued that the AER methodology with its combination of iterative estimation and specification search was arbitrary and thus yielded invalid inferences. Leamer (1986) advocated a Bayesian approach whereby the econometrician would specify a priori beliefs[10] then confrontation with the data would mould these into posterior beliefs. The difference between the two would then enable the econometrician to assess the sensitivity of parameters to changes in model specification which occurred through confrontation with the data. It seems Leamer's principal justification for this approach was a view that the only way to make econometrics credible is by showing how inferences change as assumptions (i.e., specifications) change. This seems to be supported by Keuzenkamp (2000:112),

> *"An interesting economic question is rarely whether a parameter differs significantly from zero and should be deleted or not. The magnitude of a particular (set of) parameter(s) of interest, and the sensitivity to model changes, is of much more importance."*

Leamer's proposed method for assessing this sensitivity was through extreme bounds analysis (EBA). EBA "replaces point coefficient estimates with ranges defined by the maximum and minimum estimates across specifications." (Gilbert, 1998:112) The size of the range would determine a level of confidence in the nature of the results, also known as the fragility of the results. Thus a very large range would be interpreted as a 'fragile' inference. As Darnell & Evans (1990:109) note this range is not to be confused with a confidence interval, "The EBA range reflects model specification uncertainty in the construction of alternative point estimates; the range from a confidence interval reflects sampling uncertainty within a given specification."

Many opponents of Leamer's approach criticised the use of EBA by highlighting that this procedure does not allow the data to suggest fundamentally different alternatives from those anticipated in the prior. (Gilbert 1998:112) Failing to let the data speak in this manner may frequently fail to incorporate all relevant information in model formulation

[9] Nevertheless, the reader should appreciate that these methodologies are still fundamentally distinct. See Keuzenkamp (2000) for a greater discussion of this issue.

[10] Achieved through specification of a "prior distribution over the joint density of a complete list of parameters unconstrained by paucity of degrees of freedom." (Gilbert, 1998: 112)

and estimation. In retaliation, proponents of this approach argue that such constraints are necessary to address the possibility of data-mining accusations which plague alternative methodologies.

Keuzenkamp (2000) details further possible concerns with this approach, in particular that the R^2 statistic is often used as a specification selection criterion. Major statistical problems have been highlighted concerning the use of R^2 in this way, particularly in small samples. Furthermore, the real problem of Leamer's EBA seems to be that "whatever the bound, it is not clear whether the resulting specification is plausible." (Keuzenkamp 2000:169) There seems to be a general recognition that although the idea of sensitivity analysis is revealing to econometric practice, the lack of a cogent framework regarding selection and specification criteria is a significant weakness of Leamer's methodology.

The British Tradition

The British approach to econometrics was developed in the 1950s and 1960s at the London School of Economics. Its most famous proponent has been David Hendry to the extent that this methodology is also commonly referred to as the Hendry School. The Hendry methodology is grounded in the philosophical arguments of Lakatos and Popper who both had strong links to the LSE. Lakatos's arguments were based on the principle that "when a theory is rejected a more general theory is needed which explains both the original theory and why the implication was rejected" (Bond & Harrison, 1992: 398). This idea seems to be the philosophical justification for the 'general-to-simple' approach (GtoS) which characterizes the Hendry methodology. The GtoS method is to be contrasted by the 'simple-to-general' approach explicit in the AER.

The GtoS approach seeks to address and counteract the fundamental objectivity problem[11] of the AER by starting with a very general parameterisation that is acceptable to a range of plausible theoretical positions. The model is then marginalized through simplifications that are amenable to the data. In this way the general form of the model is derived from theory but the exact empirical form is determined only via estimation and testing procedures (Bond & Harrison, 1992). In this respect the Hendry methodology is positioned towards the centre of the data-theory spectrum. It may be considered as a type of middle ground between Leamer's approach

[11] That different investigators can arrive at conflicting or unique theories based on the same data set and same initial intentions.

which promotes a strong theoretical orientation, in contrast to Sim's VAR methodology which emphasises a theoretical, time-series analysis (Gilbert, 1986:305).

The layout of the Hendry methodology begins with the formulation of a very general specification representing the Data Generating Process as much as degrees of freedom permit. The DGP is "nothing more than the joint probability of all the sample data" (Gilbert 1986:285). Having formulated this general specification from theory the first step of the testing procedure begins. One uses classical F and χ^2 tests to search for simplifications that are *congruent* [12] with the data. In this way, moving from general to simple carries only specifications which Gilbert (1986:283) refers to as 'F-acceptable'. Testing then continues using appropriate diagnostic results. Hendry regards such mis-specification testing as central to credible econometric practice. Such views are clearly expressed in Hendry's tenet of econometric modelling, namely 'test, test and test.' (Hendry, 1980: 403)

At this point the GtoS approach does not overcome the problem that a large number of alternative specifications may be data amenable. The Hendry method addresses this issue through 'encompassing'. The basic idea of encompassing is that the true DGP should be able to explain any empirical results since by definition the DGP 'created' the results. Thus, since our aim is to model the true DGP in simplified form, our chosen general model should encompass any competing specifications which may simply be regarded as sub-sets of the true DGP (Hansen, 1996, Gilbert, 1986). Encompassing is carried out in practice through a series of tests, the exact formulation of which will depend on whether the competing models are nested or non-nested, linear or otherwise.[13]

Although the concept of encompassing is widely considered as enlightening and innovative its practical implementation has not received such praise. Hansen (1996: 1411) notes the idea that all chosen models must survive iterated encompassing tests against a range of alternative and conceivable models simply fails to make statistical sense. There seems a realisation in the critique of the Hendry methodology that *full* implementation of the encompassing principle is close to unattainable.

More general concerns regarding the Hendry methodology have been accusations of data-mining and questions as to whether this methodology is simply another form of standard statistical 'exploratory data

[12] The idea of congruence is very important to the Hendry methodology, the principle is that "models are not right or wrong but are useful or misleading for particular purposes; non-congruent models are open to constructive improvement" (Gilbert, 1986: 286)

[13] See Hansen (1996: 1410) for a greater discussion of encompassing tests.

analysis'. The grounding of data-mining accusations is quite clear in that Hendry (1993) openly advocates data-based searches for model selection. Hendry does however oppose that such actions conform to data-mining. He argues this point by emphasising that the criteria used to *select* the model are not used to *test* the model which is the real indication of validity. Hendry contends that the method of selection cannot really affect a model's validity; validity can only be determined through confrontation with empirical evidence (Hansen, 1996; Bond & Harrison, 1992). In essence, Hendry's methodology argues that one should never assume a specification is valid (as is common practice in the AER approach) rather one should *test* whether the specification is valid and congruent with the data.

This argument seems to reveal the spirit of the Hendry methodology - one cannot simply rely on economic theory to form specific empirical models for theory is rarely rich enough to specify any sort of detail. Rather we must use data and testing to provide the substance and body to model specification which theory alone cannot portray. Acknowledging that some form of specification search is necessary the real question then becomes whether Hendry's method represents the most appropriate way to provide such substance? Hansen (1996:1402) indicates via formal argument that it does indeed lead us closer to the 'true' specification.

Hendry's guidelines regarding specification searching are linked to one of the major and unique strengths of this approach - the recognition of the fundamental importance of model design. Unlike other approaches Hendry's methodology provides a coherent and competent set of criteria for both model design and acceptance.[14] Other approaches frequently explain how to recognise a poor model but rarely elucidate how to rebuild a model into a more robust design. In this respect, Hendry has addressed an important facet of empirical model formulation neglected by many alternative methodologies.

Conclusion

The choice between data-led or theory-based modelling may be thought of as a spectrum. In finding a position on the spectrum inherent trade-offs often exist: models which are theoretically rigorous frequently become unstuck when applied to data; strong empirical models are often little use to theory;

[14] See Gilbert (1986: 289-299) for a description and explanation of these criteria.

whilst models which locate towards the centre are often criticised on both accounts.

Having briefly analysed three of the major approaches the question then becomes which is the most constructive for econometric practice? If we are to use the quality and quantity of applied research generated by a methodology as a guide to its value then the Hendry methodology is certainly one of the most powerful. Nevertheless, it must be conceded that even Hendry's approach fails to be applicable to all purposes. As such, a unique, defining approach does not seem to exist. Perhaps then, a more informative question is whether we need such a unique approach?

The theory of comparative advantage would suggest there is no need for all researchers to specialise on one specific approach. As Hansen (1996:1408) notes, "It is perfectly acceptable for some researchers to specialise in data description, others in pure theory, and others bridging the gap." The current state of applied econometrics would seem to support this assertion, there has been a move towards greater fragmentation of econometric methodology rather than convergence. The wealth of current possible approaches including a revised AER methodology, new calibration approaches and most recently neuroeconomic methods would seem to suggest that alternative views on the subject are more beneficial than a solitary approach. As Hansen (1996:1409) points out, "I suspect that we can learn from many different approaches, not just one 'correct' path." In this light, the debate over econometric methodology looks set to continue for the foreseeable future.

Bibliography

Bond, D. and Harrison M. J. 1992. 'Some recent developments in econometric model selection and estimation.'. *The Statistician* 41: 315-325

Christ, C. F. 1994 'The Cowles Commission's Contributions to Econometrics at Chicago, 1939-1955'. *Journal of Economic Literature* 32:4: 30-59

Darnell, A. C. and Evans, J. L. 1990. *The Limits of Econometrics.* Aldershot: Edward Elgar Publishing.

Frazer, W. J. and Boland, L. A. 1983. 'An essay on the Foundations of Friedman's Methodology'. *The American Economic Review* 73:1: 129-144

Gilbert, C. 1986. 'Professor Hendry's Econometric Methodology'. *Oxford Bulletin of Economics and Statistics* 48: 283-307

Gilbert, C. 1998. 'Econometric Methodology' in Davis J. B., Wade D. & Mäki U., (eds). *The Handbook of Economic Methodology.* Edward Elgar Publishing, 211-216.

Hansen, B. E. 1996. 'Review Article, Methodology: Alchemy or Science?'. *The Economic Journal* 106:9:1398-1413

Hausman, D. M. 1989. 'Economic Methodology in a Nutshell'. *Journal of Economic Perspectives* 2: 2:115-127

Hendry, D. 1980. 'Econometrics - Alchemy or Science'. *Economica*, 47:11:387-406

Hendry, D. 1993. *Econometrics: Alchemy or Science? Essays in Econometric Methodology.* Oxford, UK and Cambridge, USA: Blackwell.

Keuzenkamp, H. 2000. *Probability, Econometrics and Truth: The Methodology of Econometrics.* Cambridge: Cambridge University Press.

Leamer, E. E. 1978. *Specification Searches: Ad Hoc Inference With Non-Experimental Data*. New York: Wiley.

Leamer, E. E. 1983. 'Lets take the con out of econometrics'. *American Economic Review* 73:31-43

Leamer, E. E. 1986. A Bayesian analysis of the determinants of inflation in*:* Belsley, D. A. and Kuh, E. (eds), *Model Reliability.* Cambridge, MA: MIT Press.

Pesaran, M. and Smith, R. 1992. 'The Interaction between Theory and Observation in Economics'. *Economic and Social Review* 24:1-24

Sims, C. A. 1980. 'Macroeconomics and Reality'. *Econometrica* 48:1:1-48

Sims, C. A. 1982. Scientific Standards in Econometric Modelling in Hazewinkel, M. and Rinnooy-Kan, A. H. G. (eds) *Current Developments in the Interface: Economics, Econometrics, Mathematics*. Dordrecht, Boston, and London: D. Reidel 317-37