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Reflections on the LSE Tradition in Econometrics: a Student's Perspective

Aris Spanos*

Since the mid 1960s the LSE tradition, led initially by Denis Sargan and later by David Hendry, has contributed several innovative techniques and modeling strategies to applied econometrics. A key feature of the LSE tradition has been its striving to strike a balance between the theory-oriented perspective of textbook econometrics and the ARIMA data-oriented perspective of time series analysis. The primary aim of this article is to provide a student's perspective on this tradition. It is argued that its key contributions and its main call to take the data more seriously can be formally justified on sound philosophical grounds and provide a coherent framework for empirical modeling in economics. Its full impact on applied econometrics will take time to unfold, but the pervasiveness of its main message is clear: statistical models that account for the regularities in data can enhance the reliability of inference and policy analysis, and guide the search for better economic theories by demarcating 'what there is to explain'.

Keywords: LSE econometrics, textbook econometrics, ARIMA, model validation, model selection, error correction model, statistical adequacy, substantive adequacy, instrumental variables, model misspecification, Haavelmo (Trygve), Hendry (David F.), Sargan (J. Denis)

Réflexions sur la tradition de la LSE en économétrie : Le point de vue d'un étudiant

À partir du milieu des années 1960, une tradition économétrique à LSE, initiée et conduite par Denis Sargan puis par David Hendry, a fait des contributions innovantes nombreuses dans le champ de l'économétrie appliquée, tant sur le plan des techniques que des stratégies de modélisation. Un élément caractéristique de la tradition de la LSE a été son effort permanent pour trouver un équilibre entre la perspective théorique des manuels d'économétrie et une perspective orientée vers les

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données, telle qu'elle est mise en œuvre dans les modèles ARIMA pour l'analyse des séries temporelles. L'objectif principal de cet article est de présenter le regard rétrospectif d'un étudiant sur cette tradition. On soutient que ses contributions majeures et sa revendication principale de prendre plus au sérieux les données peuvent être justifiées sur des bases philosophiques solides et fournir un cadre cohérent pour la modélisation empirique en économie. Son impact sur l'économétrie appliquée mettra du temps à se déployer pleinement, mais son message est aussi clair qu'il est prégnant : les modèles statistiques qui rendent compte de régularités dans les données peuvent améliorer la fiabilité de l'inférence et l'analyse des politiques économiques, et ainsi guider la recherche de meilleures théories économiques en délimitant « ce qu'il y a à expliquer ».

Mots clés : économétrie de la LSE, économétrie des manuels, ARIMA, validation de modèle, sélection de modèle, modèle de correction d'erreur, adéquation statistique, adéquation substantive, variables instrumentales, mauvaise spécification du modèle, Haavelmo (Trygve), Hendry (David F.), Sargan (J. Denis D.)

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1 The LSE Tradition in Econometrics

The term 'LSE tradition in econometrics' is used to describe a particular perspective on econometric modeling developed by a group of econometricians associated with the London School of Economics (LSE) during the period 1963-1984¹.

The primary aim of the paper is to undertake a retrospective appraisal of this tradition viewed in the broader context of the post-Cowles Commission developments in econometric modeling. The viewing angle is one of a student of that tradition who was motivated by the aspiration to facilitate its framing into a coherent methodological framework. A framework in the context of which the key practices of that tradition could be formally justified on sound statistical and philosophical grounds.

The research agenda of the LSE tradition was influenced primarily by the experience of the protagonists like Sargan and Hendry in modeling time series data. This experience taught them that estimating a static theoretical relationship using time series data would often give rise to a statistically misspecified model. This is because most of the assumptions of the implicit statistical model, usually the Linear Regression model, are likely to be in-

1 This dating is based on the presence of the main protagonist, Denis Sargan, as a faculty member at the LSE; he arrived at the LSE in 1963 and retired in 1984. The other main protagonist, David Hendry, became faculty at the LSE in 1970 and left in 1982. Singling out Sargan and Hendry is not meant to lessen the role of other contributors like Grayham Mizon and Jean-François Richard to the LSE tradition.

valid, undermining the reliability of any inference based on such a model; see Sargan (1964), Hendry (1977).

The LSE econometricians knew that the theory-oriented perspective dominating econometrics journals at the time would publish applied papers only when the author could demonstrate that the estimated model is meaningful in terms of a particular theory. This presented them with a dilemma: follow the traditional curve-fitting approach of foisting the theory-model on the data, and 'correcting' the error term assumptions if you must, or find alternative ways to relate dynamic models with lags and trends to static theories. This, more than any other issue, gave the LSE tradition its different perspective and guided a large part of its research agenda.

1.1 A Student's Viewing Angle

I completed all my degrees (B.Sc., M.Sc. and Ph.D) at the LSE during the period 1973-1982, where I followed a programme called "Mathematical Economics and Econometrics", both at the undergraduate and graduate levels. I studied undergraduate statistics and econometrics with Ken Wallis, Alan Stuart, Jim Durbin, David Hendry and Grayham Mizon, and graduate time series analysis and econometrics with Jim Durbin and Denis Sargan. In addition to several courses in mathematical economics at different levels taught by Steve Nickell and Takashi Negishi, most crucial for my education at the LSE were a number of courses from the mathematics department, most of them taught by Ken Binmore, including analysis, set theory and logic, mathematical optimization, linear algebra, game theory, measure theory and functional analysis, which I attended with the encouragement of the economics department. The person who guided me toward econometrics during the final year of my undergraduate studies was Terence (W.M.) Gorman. After a short preamble on 'how a mathematical economist is likely to be up in the clouds for the entirety of one's career, in contrast to an econometrician who is forced to keep one leg on the ground due to dealing with real data', I agreed to meet with Denis Sargan and re-focus my graduate studies more toward econometrics rather than mathematical economics.

As a full-time student during the period 1973-1979, it gradually became clear to me that there was something exceptional about studying econometrics at the LSE. This 'special' atmosphere would become manifest during the lively econometric seminars given by faculty and students, as well as a number of visitors, including Ted Anderson, Peter Phillips, Clive Granger, Rob Engle, Tom Rothenberg and Jean-François Richard. I became aware of the difference between what I was being taught as 'textbook econometrics' and the LSE oral tradition in applied econometrics when I worked for Gorman during the summers of my 2nd and 3rd years as an undergraduate (1974-5) and I had the opportunity to interact with faculty members like Steve Putney, Tony Shorrocks and Meghnad Desai. I became so eager to find out more that as an undergraduate I decided to attend (informally) Durbin's graduate course on Time Series, which I thoroughly enjoyed. I

had to retake the course formally as a M.Sc. student, but I welcomed the opportunity to learn more from that enlightening course.

For my Ph.D. I had David Hendry as my main advisor, and Denis Sargan as a secondary advisor when Hendry was on leave, which was quite often during the period 1980-1981. A crucial part of my thesis on "Latent Variables in Dynamic Econometric Models", was to bring out the key differences between the LSE and the textbook econometrics traditions as they pertain to two broad methodological problems:

[A] How to bridge the gap between theory and data in a way that avoids attributing to the data the subordinate role of quantifying theories presumed true.

[B] How to address the twin problems of model validation and model selection using coherent strategies that account for the probabilistic structure of the data.

I began my academic career at Birkbeck College (another college of London University) in the autumn of 1979. After a disillusioning attempt to teach textbook econometrics at a graduate level jointly with Tom Cooley and Ron Smith in 1979-80, I decided to undertake a recasting of textbook econometrics in the spirit of the LSE tradition by writing extensive lecture notes. These notes provided the basis of my graduate econometrics course at Birkbeck College for several years. The students used to complain (with good justification) that the corresponding econometrics course at the LSE, based on the textbook approach, was considerably less challenging, both at the technical and conceptual levels, than the course they had to endure with me. With a lot of encouragement from John Muellbauer, these lecture notes were eventually published in Spanos (1986), with a foreword by Hendry. It was the first 'unofficial' textbook inspired by the LSE tradition during its formative phase.

My initial aim was to justify the LSE tradition's use of models with trends and lags in an obvious attempt to account for the statistical information in time series data on sound statistical and philosophical grounds. I immersed myself in philosophy of science by focusing primarily on the LSE philosophers, Popper and Lakatos and the related literature, in an attempt to find some answers, but to no avail. The problem with the philosophical accounts of *confirmation* and *falsification* is that they take *trustworthy evidence* e and *testable claims* h as readily available (Chalmers, 1999). In empirical modeling, however, the real problem is how to construct e and h .

After reflecting on these issues I concluded that the best way to construct e and h , with a view to bridge the gap between theory and data, was to devise a sequence of interconnecting models: *theory* (that might include latent variables), *structural* (estimable theory model), *statistical* (accounting for the regularities in the data) and *empirical models* (the blending of substantive and statistical information); see Spanos (1986, 1988). Shortly afterwards, I discovered that Haavelmo (1944) expressed similar ideas that went largely unnoticed by the subsequent econometric literature.

2 The Historical Context

2.1 Revisiting Haavelmo's Neglected Insights

Trygve Haavelmo was a Norwegian econometrician held in high esteem by the LSE econometricians because of his key contributions in puzzling out the 'simultaneity bias' problem and framing the Simultaneous Equations Model (SEM) in a way that largely shaped the Cowles Commission research agenda during the 1940s. I had studied Haavelmo (1943, 1947), but I was unaware of Haavelmo (1944) upon which I stumbled while going through the early volumes of *Econometrica*. The effect of that monograph on me was stunningly revelatory. Haavelmo (1944) articulated very clearly most of the problems I was grappling with, and his monograph provided many valuable insights on how to address them; see Spanos (1989; 1995; 2014).

[i] His distinction between 'theoretical', 'observational', 'true' variables and the observed data was most perceptive, and his discussion on how one might be able to bridge the gap between theory and data by contrasting 'artificial isolation designs' and those of 'Nature' was awe inspiring. He warned practitioners against assuming that the variables envisaged by a theory always coincide with particular data series, and encouraged them to pose certain key questions (1944, 16):

- (a) Are most of the theories we construct in "rational economics" one for which historical data and passive observations are *not* adequate experiments? This question is connected with the following:
- (b) Do we try to construct theories describing what individuals, firms, etc. *actually* do in the course of events, or do we construct theories describing schedules of alternatives at a given moment? If the latter is the case, what bearing do such schedules of alternatives have upon a series of decisions and actions actually carried out?

Haavelmo (1944, 7) articulated most perceptively the answer to the dilemma faced by the LSE tradition, by arguing that, in the case of observational data:

... [one] is presented with some results which, so to speak, Nature has produced in all their complexity, his task being to build models that explain what has been observed.

[ii] His embracing of the Fisher, Neyman-Pearson approach to statistical inference, led him to argue convincingly in favor of employing parsimonious parametric statistical models (74-75) in learning from data about phenomena of interest. This is in contrast to the *curve-fitting perspective* adopted by the textbook tradition; see Spanos (2014). Haavelmo (1943) warned against the perils of 'curve-fitting' by attaching random error terms to deterministic theory models:

Without further specification of the model, this procedure has no foundation ... First, the notion that one can operate with some vague idea about "small errors" without introducing the concepts of stochastic variables and probability distributions, is, I think, based upon an illusion. (Haavelmo, 1943, 5)

His recommended alternative strategy on page 7:

to avoid inconsistencies,..., all formulae for estimating the parameters involved should be derived on the basis of this joint probability law of all the observable variables involved in the system. (This I think is obvious to statisticians, but it is overlooked in the work of most economists who construct dynamic models to be fitted to the data.)

In this sense, Haavelmo foreshadowed the emphasis by the LSE tradition on modeling the observable process $\{\mathbf{Z}_t := (\mathbf{y}_t, \mathbf{X}_t), t \in \mathbb{N}\}$ underlying data \mathbf{Z}_0 , by advocating a probabilistic foundation for inference based on the joint distribution $D(\mathbf{Z}_1, \mathbf{Z}_2, \dots, \mathbf{Z}_n; \phi)$, as well as the crucial role of assessing the validity of models before drawing any inferences. Indeed, the focus on $D(\mathbf{Z}_1, \mathbf{Z}_2, \dots, \mathbf{Z}_n; \phi)$ in conjunction with his SEM framing, provided the key on how to disentangle the statistical from the structural model.

[iii] Haavelmo (1940, 1958) warned practitioners that accounting for the regularities in the data (statistical adequacy) is *not* equivalent to either (a) the model ‘fits the data well’ or (b) the model can simulate ‘realistic-looking data’: “It has become almost too easy to start with hard-boiled and oversimplified “exact” theories, supply them with a few random elements, and come out with models capable of producing realistic-looking data.” (Haavelmo, 1958, 354)

The above methodological insights from Haavelmo will be used in section 5 to address some of the key methodological problems raised by the LSE tradition.

2.2 The Origins of the LSE Tradition

The origins of the LSE tradition go back to the early 1960s when Jim Durbin (statistics department) and Bill Phillips, of the Phillips curve (1958) fame (economics department) played a crucial role in creating the econometrics group at the LSE with two key appointments. As described by Durbin (Phillips, 1988, 135):

Bill Phillips and I cooperated in getting two new posts at the readership level at the school: one in the economics department and one in the statistics department, both in econometrics. Rex Bergstrom took the post in the economics department for a time and we persuaded Denis Sargan to come from Leeds to the post in the statistics department.

Gilbert (1989, 127-128) describes the initial development of econometrics at the LSE:

The reason econometrics developed at LSE and not elsewhere in Britain was because of the close links between the Economics Department and an independent but economics-oriented Statistics Department, links which did not exist elsewhere. These close links promoted a fertilization process in which the LSE econometricians took over elements from time series analysis. The intellectual problem was how to benefit from the data-instigated time series approach to specification (identification) while at the same time being able to make structural inferences in the Cowles tradition.

Indeed, one can make a strong case that the LSE group sought to find common ground between the ARIMA-type modeling of time series (promoted by statisticians), and the simultaneous equations modeling (favored

by traditional econometricians), with a view to reconcile the two perspectives; see Spanos (2010a).

Phillips (1988, 125), described Jim Durbin's role in the 'LSE tradition':

By the 1960s it was apparent to many that the LSE was the place where it was all happening in econometrics, not only in research but also in teaching programs. Indeed, successive waves of students graduated with a special LSE pedigree that stood for the best in econometric training combined with a special interest and understanding of statistical time series. This combination has endured to the present and one of Jim's distinct legacies to the LSE has been the establishment and continuity of this intellectual tradition.

It is particularly interesting that both authors bring out the same two factors as being instrumental for the development of the LSE tradition in econometrics.

The *first* was the close collaboration between the statistics and economics departments at the LSE in fostering times series modeling and econometric theory, with Durbin and Sargan the protagonists; see Sargan (2003). It is worth noting that Durbin and Sargan were contemporaries at St. John's College, Cambridge, following similar undergraduate courses in mathematics (Phillips, 1988).

The *second* contributing factor was the concerted effort to reconcile the experience of these protagonists in modeling time series data with the theory-oriented post-Cowles tradition in econometrics. The first factor is discussed next.

2.3 Synergies Between Statistics and Economics

During the 1950s the statistics department at the LSE had a strong group of statisticians including Durbin, Kendall, Stuart and Quenouille, who were also interested in time series modeling and econometrics. Kendall and Stuart (1969, 1973, 1968) was the three volume magnum opus for advanced level statistics courses. Kendall (1953) was a highly influential paper on the statistical modeling of speculative prices; see Andreou et al (2001). Quenouille (1957) was the first monograph to provide a coherent statistical treatment of Vector Autoregressive (VAR) models as well as ARMA(p,q) models. In the early 1960s Durbin introduced a course on 'Advanced Statistical Methods for Econometrics' that began a new era for econometrics at the LSE; see Phillips (1988).

Denis Sargan began his career at the LSE as a Reader of econometrics in the statistics department in 1963 and took over the teaching of graduate econometrics courses. He became a professor of econometrics in the economics department in 1964. After that Durbin focused his graduate teaching in a time series course which was amazingly attuned to the latest developments in that field. Almost immediately upon its publication, Durbin recognized the path-breaking potential of Box-Jenkins (1970) "Time Series Analysis", and gave it center stage in his graduate time series course. He strongly emphasized the iterative nature of the Box-Jenkins time-series

modeling strategy involving several stages (identification, estimation, diagnostic checking, forecasting) with special emphasis on *graphical techniques* and *diagnostic checking*. Durbin was a strong advocate of diagnostic checking that evolved into the current Mis-Specification (M-S) testing. After proposing the first such test for autocorrelation (Durbin and Watson, 1950), he put forward several additional M-S tests (Durbin, 1975), including tests for the constancy of the parameters using recursive least-squares (Brown, Durbin and Evans, 1975). Indeed, he was a strong advocate of thorough M-S testing for model validation purposes:

I've always thought it was really quite important to carry out diagnostic tests. Certainly in econometric applications and other applications of regression analysis to time series data, I think it is important to check out whether the assumptions on which inference is based are satisfied. (Phillips, 1988, 132-133)

He went on to elaborate on the crucial importance of M-S testing:

In the many fields of interest to me such as time series and applications in econometrics and the social sciences, one now has the possibility of calculating a large number of different diagnostic test statistics. Of course, I have a special interest in tests of autocorrelation, but one thinks of tests of normality, one thinks of tests of heteroskedasticity, and so on. ... And if we find these assumptions are invalid we can make modifications and then do some more diagnostic tests. (Phillips, 1988, 151)

Indeed, he dismissed the charge against M-S testing known as 'multiple hypotheses', calling it 'theoretical' in a derogatory sense: "I think it's quite right and proper for an applied worker to look at a wide variety of diagnostic tests and, especially, I like the idea of graphical procedures." (Phillips, 1988, 151)

The close collaboration between the statistics and economics departments at the LSE was particularly crucial for reconciling the theory-oriented perspective of the Cowles Commission with the data-oriented perspective of time series modeling. The high level grounding in statistical theory for the M.Sc. in Econometrics contributed significantly to the effort because it gave the students the necessary background and enough confidence to pursue this reconciliation both at a technical as well as a methodological level. The programme "Mathematical Economics and Econometrics" at both undergraduate and graduate levels, was jointly taught by the economics and statistics departments, with several key courses taught by the mathematics department. Indeed, the lines between the two departments were so blurred that as an undergraduate I didn't realize that Ken Wallis and Graham Mizon, two well-known econometricians, were faculty members of the statistics department. Peter Robinson, who succeeded Denis Sargan in the Tooke Chair, did his M.Sc. in the Statistics department.

The main textbooks in statistics during my undergraduate studies were Kendall and Stuart (1969, 1973, 1968), Cox and Hinkley (1974) and Rao (1973). For Durbin's graduate course on 'Time Series' the main textbooks were Hannan (1970) and Anderson (1971), with occasional references to Box and Jenkins (1970). For Sargan's 'Advanced Econometric Theory' course the main statistics textbook was Cramer (1946). My initial thoughts about the

book being out-of-date turned out to be completely unfounded; an early lesson in appreciation of Sargan's wisdom on statistical issues.

An issue related to the high level grounding in statistical theory aimed at by the LSE courses pertains to Sargan as a teacher. According to Hendry (Ericsson, 2004, 748-749):

Denis was always charming and patient, but he never understood the knowledge gap between himself and his students. He answered questions about five levels above the target, and he knew the material so well that he rarely used lecture notes. I once saw him in the coffee bar scribbling down a few notes on the back of an envelope—they constituted his entire lecture. Also, while the material was brilliant, the notation changed several times in the course of the lecture: α became β , then γ , and back to α , while γ had become α and then β ; and x and z got swapped as well.

In light of that, how did Sargan advise so many distinguished econometricians (Maasoumi, 1988a)? Hendry went on to answer that question: "Sorting out one's notes proved invaluable, however, and eventually ensured comprehension of Denis's lectures." (Ericsson, 2004, 749)

The selected group of 12-15 M.Sc. students attending that course viewed the deciphering of Sargan's lecture notes as a personal challenge, and they had (or could acquire from other LSE courses) the technical background needed to do just that. It is no coincidence that Hendry was able to publish several technical papers within half a dozen years of arriving at the LSE in 1967 without any background in statistical theory; see Ericsson (2004).

3 Textbook Econometrics

The key difference between the mainstream post-Cowles and the LSE perspectives stems primarily from their view of the role of theory and data in empirical modeling.

3.1 Pre-Eminence of Economic Theory

The 'pre-eminence of theory' perspective, dominating economic modeling since Ricardo (1817), attributes to data the subordinate role of 'quantifying theories presumed true'. In this conception, data do not so much test as facilitate the instantiation of theories. Econometric methods offer sophisticated ways 'to bring data into line' with a particular theory. Since the theory has little chance to be falsified, such instantiations do not constitute genuine tests of the theory as such; see Spanos (2010a).

Cairnes (1888, 72-94), articulated the most extreme version of the 'pre-eminence of theory' by pronouncing data irrelevant for appraising economic theories. His argument in a nutshell was that economic theories are far superior to those of physics because the premises of economic theories are deductive in nature. They are derived from 'self-evident truths' established by introspection via direct access to the ultimate causes of economic

phenomena, rendering them infallible. In contrast, the premises of Newtonian Mechanics are based on mere inductive generalizations based on experimentation and inductive inferences, which are known to be fallible.

Robbins, a leading professor at the LSE during the period 1930-1965 (see Sargan, 2003), articulated an almost identical view:

In Economics, ..., the ultimate constituents of our fundamental generalizations are known to us by immediate acquaintance. In the natural sciences they are known only inferentially. There is much less reason to doubt the counterpart in reality of the assumption of individual preferences than that of the assumption of the electron. (Robbins, 1935, 105)

Indeed, Robbins (1935) dismissed the application of statistics to theory appraisal in economics claiming that such techniques are only applicable to data which can be considered as 'random samples'. Since there were no such data in economics, statistical analysis of economic data had no role to play in theory assessment. Robbins (1971, 149) recanted these claims describing them as: "exaggerated reactions to the claims of institutionalists and 'crude' econometricians like Beveridge".

The current version of this perspective sounds almost as extreme:

Unlike the system-of-equations approach, the model economy which better fits the data is not the one used. Rather currently established theory dictates which one is used. (Kydland and Prescott, 1991, 174)

3.2 The Framing of Textbook Econometrics

The prevailing view among applied economists in the early 1930s was that the statistical methods associated with Fisher-Neyman-Pearson, although applicable to experimental data, are inapplicable to economic data because: (i) the 'experimental method' is inappropriate for studying economic phenomena, (ii) there is always an unlimited number of potential factors influencing economic phenomena—hence the invocation of *ceteris paribus* clauses—, (iii) economic phenomena are intrinsically heterogeneous (spatial and temporal variability), and (iv) economic data are vitiated with errors of measurement; see Frisch (1934). Hence, Frisch rejected the Fisher-Neyman-Pearson approach to statistical inference and proposed his *confluence analysis* as an alternative method that could account for the perceived features (i)-(iv).

This standpoint led to a different approach to statistical modeling and inference which was based on the Gauss-Laplace curve-fitting perspective in conjunction with Quetelet's scheme based on:

(C1) a *systematic* (deterministic) *component* (constant causes) determined by substantive information, and

(C2) a random part which represents the *non-systematic error* (accidental causes) *component* (see Desrosières, 1998).

Econometric modeling, with a theory-oriented structural modeling providing the premises for statistical inference, was initiated in the 1940s and was formalized by the Cowles Commission (see Koopmans, 1950, Hood and Koopmans, 1953) into the Simultaneous Equations Model (SEM); see

Morgan (1990), Qin (1993). Modern econometrics, however, was initially framed in the early 1960s by two highly influential textbooks, Johnston (1963) and Goldberger (1964). They successfully demarcated the intended scope of modern econometrics for the next half century and beyond. Their success is largely due to two crucial factors.

The *first* was their embrace of the Pre-Eminence of Theory perspective on empirical modeling. This perspective had great appeal to economic theorists because it gave econometrics an instrumental role with very narrow scope. They achieved this by adopting the *curve-fitting perspective* where a deterministic theory-model is transformed into a statistical model by attaching white-noise error term(s) that often represent errors of measurement, errors of approximation, omitted effects or stochastic shocks; see Marshack (1953, 12), and Johnston (1963, 5-7).

Pagan (1984, 103) offered a succinct description of the textbook approach as follows:

Four steps almost completely describe it: a model is postulated, data gathered, a regression run, some t-statistics or simulation performance provided and another 'empirical regularity' was forged.

Although this reads like a caricature, it is very similar to the description offered by Johnston (1972, 6).

The *second* reason for the success of textbook econometrics was its demystifying of the Cowles SEM by presenting a system of interdependent equations as a natural extension of the Linear Regression model. The blueprint of this textbook econometrics tradition was simple and coherent. The Linear Regression model:

$$y_t = \beta_0 + \beta_1 x_t + \varepsilon_t, \quad t=1, 2, \dots, n,$$

in conjunction with the *Gauss-Markov theorem*, became the cornerstone of econometric theory. The latter provided the key assumptions for the error term:

- | | | | |
|---------------------------|---|---|-----------------------|
| (i) Zero mean: | $E(\varepsilon_t) = 0,$ | } | $t, s=1, 2, \dots, n$ |
| (ii) Homoskedasticity: | $E(\varepsilon_t^2) = \sigma^2,$ | | |
| (iii) No-autocorrelation: | $E(\varepsilon_t \varepsilon_s) = 0, \quad t \neq s,$ | | |
| (iv) No-collinearity: | $\sum_{t=1}^n (x_t - \bar{x})^2 \neq 0, \quad \bar{x} = \frac{1}{n} \sum_{t=1}^n x_t$ | | |

that would yield Best Linear Unbiased Estimators (BLUE) of (β_0, β_1) . Moreover, all other models of interest in econometrics could then be viewed as variations/extensions of this basic recipe. The rest of this textbook econometrics blueprint is a sequence of chapters that discuss inference methods relating to departures from the above assumptions (i)-(iv). These chapters are given titles indicating that departures from assumptions (i)-(iv) are viewed as 'problems' to be fixed.

The same blueprint, but with unremittingly accumulating additional material, has dominated all the traditional textbooks in econometrics to this day; see Johnston (1972), Theil (1971), Maddala (1977), Judge et al (1985), Greene (2011) inter alia.

3.3 Textbook Econometrics and the DAE

Part of the success of this textbook blueprint was due to the fact that the above simple Linear Regression model could be extended to the $k > 1$ regressors case by using a carefully designed matrix notation that made the extension seem intuitive and straight forward. That notation was provided by the Department of Applied Economics (DAE) at Cambridge, England, under the directorship of Richard Stone; see Gilbert (1991). In 1948 Stone offered a job to Durbin upon completion of his diploma in mathematical statistics. The papers by Durbin and Watson (1950, 1951) influenced the framing of textbook econometrics in several crucial respects.

The *first* major influence was the pertinent matrix notation for the Linear Regression model:

$$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon},$$

$$[\text{i}] E(\boldsymbol{\varepsilon}|\mathbf{X})=\mathbf{0}, [\text{ii}] Cov(\boldsymbol{\varepsilon}|\mathbf{X})=\sigma^2\mathbf{I}_n, [\text{iii}] \text{Rank}(\mathbf{X})=k < n, \quad (1)$$

$$\mathbf{y}: n \times 1, \mathbf{X}: n \times k, \boldsymbol{\beta}: k \times 1, \boldsymbol{\varepsilon}: n \times 1, \mathbf{I}_n: n \times n.$$

The conditioning on $\mathbf{X}=\mathbf{x}$ was added to render more general the original 'fixed in repeated samples' assumption; see Goldberger (1964). The role of notation in rendering certain procedures seem intuitive is often undervalued in science, but it was critical for the success of textbook econometrics. A strong case can be made that despite the fact that Malinvaud (1966) was a more esteemed textbook at both the technical and conceptual levels, its overall influence on econometrics was considerably less than that of Johnston (1963) and Goldberger (1964).

The *second* key influence on textbook econometrics by the Durbin-Watson papers was to provide the initial articulation of the *Gauss-Markov theorem* (1950, 410):

If in addition, $\varepsilon_1, \varepsilon_2, \dots, \varepsilon_n$ can be taken to be distributed independently of each other with constant variance, then by Markov's theorem² the least squares estimates of $\beta_1, \beta_2, \dots, \beta_k$ are best linear unbiased estimates whatever the form of the distribution of the ε 's."

The *third* key influence was the way Durbin-Watson (1950) framed the textbook econometrics perspective on *M-S testing*, using the *non-correlation* assumption in [ii]. Their contribution can be described in the following two steps.

Step 1. They postulated an Autocorrelation-Corrected Regression:

$$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon}, \quad \boldsymbol{\varepsilon}=\rho\boldsymbol{\varepsilon}_{-1} + \mathbf{u}, \quad |\rho| < 1,$$

$$[\text{i}] E(\boldsymbol{\varepsilon}|\mathbf{X})=\mathbf{0}, [\text{ii}]^* Cov(\boldsymbol{\varepsilon}|\mathbf{X})=\sigma_0^2\mathbf{V}_n, [\text{iii}] \text{Rank}(\mathbf{X})=k < n, \quad (2)$$

² Ironically, the initial attribution of the theorem to Markov by David and Neyman (1938) is misplaced. As Plackett (1949) argues: "Markoff ... may perhaps have clarified assumptions implicit there but proved nothing new." Neyman's (1952, 228), correction: "the theorem that I ascribed to Markoff was discovered by Gauss", was never noticed by the econometrics literature.

that *parametrically nests* the original Linear Regression model (1). This involved *particularizing* the generic departure from independence:

$$E(\varepsilon_t \varepsilon_s | \mathbf{X}_t = \mathbf{x}_t) \neq 0, \quad t > s, \quad t, s = 1, 2, \dots, n, \quad (3)$$

in the form of the AR(1) model, i.e. (3) has been particularized to:

$$E(\varepsilon_t \varepsilon_s | \mathbf{X}_t = \mathbf{x}_t) = \left(\frac{\rho^{|t-s|}}{1-\rho^2} \right) \sigma_0^2, \quad t > s, \quad t, s = 1, 2, \dots, n. \quad (4)$$

Note that this particularization has reduced the unknown parameter of \mathbf{V}_n from $\frac{1}{2}n(n+1)$ and increasing with n , to just one ρ .

Step 2. Testing independence is now parameterized in (2) using the hypotheses:

$$H_0: \rho = 0, \text{ vs. } H_1: \rho \neq 0. \quad (5)$$

In terms of the OLS residuals $\hat{\varepsilon}_t = y_t - \mathbf{x}_t^\top \hat{\beta}$, where $\hat{\beta} = (\mathbf{X}^\top \mathbf{X})^{-1} \mathbf{X}^\top \mathbf{y}$ is the OLS estimator, the D-W test for (5) is defined by:

$$D-W(\mathbf{y}) = \left[\sum_{t=1}^n \hat{\varepsilon}_t^2 \right]^{-1} \sum_{t=2}^n (\hat{\varepsilon}_t - \hat{\varepsilon}_{t-1})^2, \quad C_1 = \{ \mathbf{y}: d_U(\alpha) < D-W(\mathbf{y}) < d_L(\alpha) \}, \quad (6)$$

When the observed test statistic $D-W(\mathbf{y}_0)$ is smaller (bigger) than the lower (upper) bound $d_L(\alpha)$ ($d_U(\alpha)$), H_0 is rejected.

What is especially remarkable, and worth bringing out, is that Durbin and Watson (1950, 409) did not recommend a respecification strategy, by declaring that: “We shall not be concerned in either paper with the question of what should be done if the test gives an unfavorable result.”

A third influential paper written by the DAE group, Cochrane and Orcutt (1949), provided the answer to this *respecification* question for textbook econometrics.

Step 3. When the D-W test rejects H_0 adopt H_1 . That is, replace the original model (1) with the alternative model (2). This respecification is traditionally presented as replacing $\hat{\beta}$, which is inefficient under [ii]*, with the relatively more efficient GLS estimator $\check{\beta} = (\mathbf{X}^\top \mathbf{V}_n^{-1} \mathbf{X})^{-1} \mathbf{X}^\top \mathbf{V}_n^{-1} \mathbf{y}$ (Greene, 2011). The justification stems from its affinity to the Pre-Eminence of Theory perspective because it retains the original *theory-model* and ‘fixes’ assumption [ii] of the error term.

This form of ‘error-fixing’, i.e. adopting the particular alternative in a M-S test, has been extended to other assumptions, including homoskedasticity and linearity; see Greene (2011). In section 5 it is argued that this respecification strategy is fallacious and invariably leads to unreliable inferences.

4 The Framing of the LSE Tradition

The key differences between the LSE and textbook traditions were primarily methodological. The protagonists were sceptical about the pertinence of

the Pre-Eminence of Theory perspective because they knew first hand that 'quantifying theoretical models presumed true' doesn't work in practice; see Mizon (1995a). Sargan (1957) criticized the simplistic way of bridging the gap between theory and data, encouraged paying particular attention to the nature of the information in the data (cross-section vs. time series), and warned against treating the choice of the data as an afterthought.

In their attempt to avoid both extreme practices, the Pre-Eminence of Theory modeling perspective on one hand, and the data-driven ARIMA modeling, on the other, the LSE tradition set out a 'third way', aspiring to account for the regularities in data without ignoring pertinent theory information. As argued by Hendry (2009, 56-57):

This implication is not a tract for mindless modeling of data in the absence of economic analysis, but instead suggests formulating more general initial models that embed the available economic theory as a special case, consistent with our knowledge of the institutional framework, historical record, and the data properties. ... Applied econometrics cannot be conducted without an economic theoretical framework to guide its endeavours and help interpret its findings. Nevertheless, since economic theory is not complete, correct, and immutable, and never will be, one also cannot justify an insistence on deriving empirical models from theory alone.

The LSE econometricians found themselves recasting econometric modeling by inventing new concepts and methods while striving to find or adapt a suitable foundation in one or another philosophy of science (Kuhn, Popper, Lakatos); see Hendry (1980), Hendry and Richard (1982). While loosely reflecting on a Popperian conception to criticize and a Lakatosian demand for 'progressiveness', these philosophical approaches did not provide an appropriate framework wherein one could repudiate the misleading charges leveled against the LSE tradition; see Spanos (2010a).

4.1 Textbook Econometrics at the LSE

The LSE courses in econometrics during my full-time student days [1973-1979] were based on traditional textbooks; Johnston (1963/1972), Malinvaud (1966/1970) and Theil (1971) for undergraduate courses, and Schmidt (1976) and Hood and Koopmans (1953) for Sargan's graduate courses. Although the material taught in econometric courses was largely traditional (Sargan, 1988b), it had several distinct *differences in emphasis*. The *first* difference was the broader and more balanced grounding in statistical theory, including estimation, testing and prediction, well beyond the definitions and summaries found in the recommended textbooks. The emphasis on Maximum Likelihood Estimation (MLE), likelihood ratio and related frequentist procedures associated with Fisher, Neyman and Pearson, was engendered by the synergy and close collaboration between the economics and statistics departments. In 1963 Durbin wrote a paper entitled: "Maximum Likelihood Estimation of the Parameters of a System of Simultaneous Regression Equations", that provided the motivation for Hendry (1976). The *second* difference was a special emphasis placed on certain modeling is-

sues arising from time series data such as modeling temporal dependence/heterogeneity. The *third* difference in emphasis was the presentation of empirical modeling as an iterative process instead of a one-shot model-fitting routine. One could also discern a certain critical perspective on textbook econometrics that encouraged the students to obviate excessive respect for the authority of the textbook and develop a more critical perspective.

These crucial differences in the teaching of econometrics made the LSE students both aware of the path-breaking nature of the research agenda of the LSE econometricians (Leamer, Hendry and Poirier, 1990; Pagan, 1987), and confident enough in their technical background to pursue such topics in their research. The majority of the Ph.D students, following Sargan's lead (Maasoumi, 1988b), pursued mainly technical issues arising in both time series as well as simultaneous equations modeling. In this sense, the LSE tradition participated fully in the development of technical tools in addressing crucial inference problems on the mainstream post-Cowles agenda. Indeed, there had been crucial interactions between the LSE protagonists with the North American post-Cowles tradition when Sargan spent several visits in the United States in the late 1950s (Phillips, 1985, 125): "It certainly was very stimulating to have not only long stays at Minnesota [1958-9] and Chicago [1959-60], but also to spend some time on the West Coast in the summer of 1959 and visit the East Coast, particularly the Cowles Foundation in 1960."

A smaller number of students, including myself, decided to grapple with the methodological issues raised by the different perspectives. This is not unrelated to the fact that Hendry interacted with various groups of econometricians at CORE, San Diego, Yale, Berkeley and Australian National University during the period 1980-81, and had to defend the LSE methodology; see Ericsson (2004, 767).

4.2 Key Elements of the LSE tradition

According to Hendry (2003), the methodological issues raised in Sargan (1964) largely defined the research agenda for the LSE tradition for the next 20 years or so. In a paper entitled "J. Denis Sargan and the Origins of LSE Econometric Methodology", he summarizes the key contributions of Sargan (1964):

In this paper, Denis laid out the conceptual foundations of what has become the "LSE approach." The essential elements that he formalized included:

- (1) the use of "long-run" economic analysis to specify the equilibrium of the model;
- (2) the introduction of "equilibrium-correction" mechanisms into behavioral dynamic econometric models;
- (3) the development of a new interpretation of autoregressive errors in time-series models;
- (4) the construction of valid misspecification tests after estimating dynamic models;
- (5) the use of model comparison procedures for linear against logarithmic specifications;

- (6) the investigation of the impact of data transforms on the selection of models;
- (7) a nonlinear in parameters instrumental variables estimator for measurement errors;
- (8) the development of operational computer programs to implement the new econometric methods;
- (9) a proof that his iterative computations would converge with near certainty; and
- (10) matching the econometric theory to the substantive empirical modeling problem.

In *elements* (1)-(2) Sargan proposed innovative ways to bridge the gap between dynamic statistical models and static structural models in terms of the long-run equilibrium and the error-correction term due to Phillips (1957). In his reply to Ball's criticisms that his wage equation does not accord well with the theoretical demand and supply functions for labor Sargan (1964, 60), argued:

it is usual to think of the type of wage equation that I have been estimating as a price-adjustment equation, and also that a more complete model of this type would treat unemployment as an endogenous variable. To do this would require an equation explaining the actual number employed, and the actual number retaining their names on the employment exchange registers. But can these be considered the same as the demand and supply of labour? In the body of the paper I give reasons for doubting this.

The 'error-correction' formulation had its roots in Phillips (1957), but it was popularized by Davidson *et al.* (1978) and used widely in empirical modeling because of its success in improving a model's forecasting ability. It also proved instrumental in initiating the extensive literature on 'cointegration'; see Granger (1981), Hendry (1986), Engle and Granger (1987), Johansen (1991; 1995).

The initial seeds for the notion of cointegration were garnered in a discussion between Hendry and Granger in 1980 after a seminar given by Hendry at the monthly meeting of the SSRN Econometrics Workshop. Granger called into question the 'validity' of the basic error-correction model:

$$\Delta y_t = \beta_0 + \beta_1 \Delta x_t + \beta_2 (y_{t-1} - x_{t-1}) + u_t, \quad (7)$$

on the grounds that it is 'unbalanced'; see Granger (1990, 12). In the terminology of cointegration developed later, Granger was arguing that if the time series (y_t, x_t) were integrated of order 1, denoted by $y_t \sim I(1)$ and $x_t \sim I(1)$, then $\Delta y_t \sim I(0)$ and $\Delta x_t \sim I(0)$, but the error-correction term $(y_{t-1} - x_{t-1}) \sim I(1)$. After further discussion at the end of the seminar, which I witnessed, they agreed to disagree on the cogency of dynamic specifications like (7), and I was asked by Hendry to run some simulations to see if they shed any light on the disagreement. The initial simulations that evening seemed to support Granger's doubts because the recursive estimator of the coefficient β_2 did not seem constant, and I relayed that information to Hendry the next day. It turned out that under different conditions they were both right.

In element (3) Sargan proposed to view dynamic models in terms of the observable process $\{\mathbf{Z}_t, t \in \mathbb{N} := (1, 2, \dots, n, \dots)\}$ underlying the data $\mathbf{Z}_0 := (\mathbf{z}_1, \mathbf{z}_2, \dots, \mathbf{z}_n)$, where $\mathbf{Z}_t := (y_t, \mathbf{X}_t)$, by viewing the Linear Regression model with an AR(1) error (see (2)) as a restricted form of a Dynamic Linear Regression model:

$$y_t = \mathbf{x}_t^\top \beta_0 + \mathbf{x}_{t-1}^\top \beta_1 + \beta_2 y_{t-1} + v_t, \quad (8)$$

with the restrictions taking the form of the (non-linear) *common factors*:

$$\beta_1 + \beta_0 \beta_2 = 0. \quad (9)$$

These restrictions stem from the fact that:

$$\{y_t = \mathbf{x}_t^\top \beta + \varepsilon_t, \varepsilon_t = \rho \varepsilon_{t-1} + u_t\} \rightarrow y_t = \mathbf{x}_t^\top \beta - \mathbf{x}_{t-1}^\top \beta \rho + \rho y_{t-1} + u_t.$$

This departure from the textbook viewpoint was very important for several reasons.

(a) It placed the observable process $\{\mathbf{Z}_t, t \in \mathbb{N}\}$ and its probabilistic structure at center stage, and unveiled the distributional reduction yielding the parameterization implicit in different statistical models. The reduction for (8) is: $D(\mathbf{Z}_t | \mathbf{Z}_{t-1}; \varphi) = D(y_t | \mathbf{X}_t, \mathbf{Z}_{t-1}; \varphi_1) \cdot D(\mathbf{X}_t | \mathbf{Z}_{t-1}; \varphi_2)$, with $D(y_t | \mathbf{X}_t, \mathbf{Z}_{t-1}; \varphi_1)$ the distribution underlying (8) and $\varphi_1 = (\beta_0, \beta_1, \beta_2, \sigma_v^2)$. The reduction is primarily due to Hendry's collaborative work with Richard (Richard, 1980), and provided the key to elucidating the notion of *weak exogeneity*; see Hendry and Richard (1982), Engle, Hendry and Richard (1983).

(b) It brought out the fact that by modeling the error term one imposes implicit restrictions on statistical models like (8) when specified directly in terms of the observable processes $\{(y_t | \mathbf{X}_t, \mathbf{Z}_{t-1}), t \in \mathbb{N}\}$. These restrictions, although testable, are rarely data-acceptable. As shown in McGuirk and Spanos (2008), the common factor restrictions in (9) impose highly unappealing restrictions on the temporal structure of the vector process $\{(y_t, \mathbf{X}_t | \mathbf{Z}_{t-1}), t \in \mathbb{N}\}$ that involve several Granger non-causality presumptions! More generally, it showed that it is always more general to model the observable processes involved directly instead of indirectly via the error term.

(c) It highlighted an important difference in attitude toward departures from model assumptions between the two traditions. For the textbook econometrics tradition such departures are viewed as a problem and a nuisance to be 'corrected'. In contrast, for the LSE tradition such departures are not a nuisance but a blessing, since the modeler can use the additional statistical information to improve both the reliability and precision of inference; see Hendry and Mizon (1978).

(d) It brought out the importance of keeping track of the relevant error probabilities in sequential testing by introducing the *general-to-specific* procedure first introduced by Anderson (1962). This was in contrast to the textbook econometrics tradition that favored *simple-to-general* modeling procedures.

(e) It revealed the questionable nature of the textbook strategy of adopting the alternative model when the D-W test rejects the null (step 4), and offered more general ways to account for the presence of the temporal dependence, e.g. respecifying the original Linear Regression into the Dynamic Linear Regression model. It is interesting to note that in the case of Linear Regression with an AR(1) error term (see (2)), Durbin (1960) argued in favor of ignoring the common factor restrictions and estimating the parameters of the Dynamic Linear Regression model using OLS.

Element (4), relating to M-S testing was initiated in Sargan's early writings and enhanced by Durbin's contributions in this area, represents another crucial departure from textbook econometrics. As argued by Hendry (1980, 406):

The three golden rules of econometrics are test, test and test: that all three rules are broken regularly in empirical applications is fortunately easily remedied. Rigorously tested models, which adequately describe the available data, encompass previous findings and were derived from well-based theories would greatly enhance any claim to be scientific.

A strategy for M-S testing was initially formalized by Mizon (1977) and applied more broadly by other members of the LSE tradition, especially Hendry and his coauthors; see Davidson et al (1978), Hendry (1980). This also encouraged practitioners to use graphical techniques that bring out the chance regularities in the data with a view to render statistical model specification more effective; see Spanos (1999). The LSE perspective favored a thorough probing of the model assumptions to account for all statistical information in the data and respecify if the model is misspecified.

Elements (5)-(6) pertain to model validation and model selection procedures that played an important role in the modeling practices of the LSE tradition; see Sargan (1973). Choosing between a linear and log-linear specification arose naturally in the context of modeling with time series data, and since the two specifications were non-nested parametrically one needed alternative ways to the Neyman-Pearson testing to choose between them. Ultimately, however, the issue of choosing between the two specifications is one of statistical adequacy (the model assumptions are valid for the data), and the linear vs. log-linear specifications differ in more ways than just the functional form of the regression function; see Spanos, Hendry and Reade (2008). Hence, Sargan proposed to specify one's statistical model in a way that ensures that error term is approximately white-noise. This is in contrast to the textbook perspective which encourages the practitioner to retain the original theory-model and change the probabilistic assumptions of the error term.

The LSE tradition's answer to the problem of choosing among (parametrically) *non-nested* models was the *encompassing principle* and the associated procedures; see Mizon (1984), Mizon and Richard (1986), Hendry and Richard (1989).

In light of the fact that the LSE tradition encouraged the specification of statistical models with lags and trends, even when the structural model was

static, the need for a systematic way to test downwards from a general to more specific models arose naturally. Anderson (1962, 1971) provided the answer in the sense that it showed how one can begin with a general specification and test sequentially downwards using Neyman-Pearson testing and keeping track of the error probabilities. Mizon (1977) extended these results to non-ordered hypotheses and non-linear restrictions. This led to the *General-to-Specific* procedure that grew into a more distinct methodology associated with David Hendry and his coauthors because of its key role in guiding model validation and selection; see Hendry (2000), ch. 19, Campos et al. (2005).

Element (7) constitutes an example of several crucial inferential methods and procedures put forward by the LSE tradition that were often motivated by their experience in empirical modeling with time series data.

Sargan (1958, 1959) greatly generalized the Instrumental Variables (IV) method in the context of the SEM that included dynamic specifications and non-linearities. Of particular interest are several papers on estimation, identification and testing in the context of the SEM with special emphasis on the finite sample properties of structural parameter estimators (IV, 2SLS, 3SLS, FIML) and tests, as well as dynamic specifications, using Edgeworth and Gram-Charlier approximations with a view to improve the asymptotic sampling distributions. Monte Carlo simulations were also used extensively to study the sampling distributions of such estimators and tests; see Maasoumi (1988b), Phillips (1985). This was clearly motivated by the practical problem of undue reliance on asymptotic theory even in cases of small sample sizes; Sargan (1964) relied on $n=16$. As Sargan explains (Phillips, 1985, 126): “I had been worried for some time that all our theory except for linear models was asymptotic theory, and I realized that the Edgeworth expansion was a way forward.

Elements (8)-(9) represent another component of the LSE tradition that helped to make available to practitioners a lot of the innovative procedures proposed by its members. In the early 1960s Sargan wrote the code for RALS for his 1964 paper and Hendry continued that tradition with GIVE (Generalized Instrumental Variables Estimation) and PcGive; the latter has been continuously updated and widely used to this day. Hence, from the mid 1960s onwards the writing of computer programs to implement estimation and testing procedures was an important feature of applied research in econometrics at the LSE and continued unabated to this day, particularly in Oxford. This has helped to broaden the appeal of the LSE tradition because practitioners could use the software to implement its innovative procedures.

David Hendry played a crucial role in enhancing and developing further the themes and methodological issues initiated by Sargan (1964). He popularized and enhanced the modeling procedures and strategies in the form of the *General to Specific* modeling that can be implemented using PC-GIVE; see Gilbert (1986, 1989), the papers in Hendry (2000) and ch. 19-20, Hendry (1995, 1987, 2009), Mizon (1995) and the papers in Campos, Eric-

son and Hendry (2005). More recently, a related software program, known as 'Autometrics', is designed to implement the LSE modeling methodology, including model selection and data mining issues, in an automated and more systematic way; see Doornik (2009), Hendry and Mizon (2011), Castle *et al.* (2012).

Element 10 on 'matching the econometric theory to the substantive empirical modeling problem' represents the key feature of the LSE tradition. It pertains to how their empirical work, beginning with Sargan's 1964 wage-price model, strived to bridge the gap between economic theory and data by accounting for the regularities in the data without ignoring pertinent theory information.

4.3 The LSE Tradition was Never Taught at the LSE

From a teaching perspective, the LSE tradition at its place of birth remained largely an 'oral tradition'. This is primarily due to the fact that its first 'official' textbook, Hendry (1995), had an extended gestation period. This was long after Hendry left the LSE for Oxford University in 1982.

The LSE tradition was primarily reflected in the research of the LSE econometrics group and their seminar series, such as the weekly SSRC funded workshop on "Specification and Estimation Problems with Dynamic Econometric Models" (1974-6), and the monthly meetings of the SSRC Econometrics Workshop. The first attempt to demarcate this tradition by contrasting it to the textbook approach was made in Hendry and Wallis (1984), a volume dedicated to Sargan, with contributions by Steve Nickell, Andrew Harvey, Jean-François Richard, Adrian Pagan, Grayham Mizon, Pravin Trivedi, David Hendry and Meghnad Desai, along side a reprint of Sargan (1964).

Sargan (1988), based on his recorded lectures on the Advanced Econometric Theory course during the academic year 1983-4, represents a rigorous presentation of traditional textbook methods with special emphasis on asymptotic theory, the SEM, OLS, GLS and Instrumental Variables, maximum likelihood methods and alternative testing procedures. In this book the LSE tradition is reflected in occasional comments and its oblique focus on modeling the dynamics, but little else. Indeed, Sargan always saw himself as working within the post-Cowles tradition. His alternative perspective only concerned the practical aspects of relating theory to data, in general, and the empirical aspects of modeling with time series data, in particular. Peter Robinson, who succeeded Sargan in the Tooke Chair in 1984, had no interest in the methodological issues raised by the LSE tradition, but continued Sargan's predilection for rigorous mathematical arguments in the presentation of textbook econometrics.

5 Retrospective and Perspective

The process of blending the above methodological insights from Haavelmo (section 2.1) into the LSE tradition in econometrics to address certain key methodological problems began by focusing on modeling the observable process $\{\mathbf{Z}_t, t \in \mathbb{N}\}$ underlying data \mathbf{Z}_0 , instead of making probabilistic assumptions about error terms; see Spanos (1986). This section elaborates on how the key methodological problems raised by the LSE tradition can be addressed in the context of this framework, as well as reply to several charges leveled against this tradition by its critics, including Hansen (1996, 1999), Faust and Whiteman (1997), and Wooldridge (1998).

5.1 Haavelmo and the LSE Tradition

The key to elucidating and addressing the methodological problems [A]-[B] (section 1.1) was the untangling of the statistical from the substantive premises. The answer was inspired by Haavelmo's SEM and his emphasis on the joint distribution of the observables $D(\mathbf{Z}_1, \mathbf{Z}_2, \dots, \mathbf{Z}_n; \phi)$. Behind a *structural model*:

$$\mathcal{M}_\varphi(\mathbf{z}): \quad \mathbf{\Gamma}^\top(\varphi)\mathbf{y}_t + \mathbf{\Delta}^\top(\varphi)\mathbf{x}_t = \varepsilon_t, \quad \varepsilon_t \sim N(\mathbf{0}, \mathbf{\Omega}(\varphi)), \quad t \in \mathbb{N}, \quad (10)$$

there is a *reduced form* which is in essence the (implicit) *statistical model*:

$$\mathcal{M}_\theta(\mathbf{z}): \quad \mathbf{y}_t = \mathbf{B}^\top(\theta)\mathbf{x}_t + \mathbf{u}_t, \quad \mathbf{u}_t \sim N(\mathbf{0}, \mathbf{\Sigma}(\theta)), \quad t \in \mathbb{N}, \quad (11)$$

with (10) and (11) related via the *identifying* restrictions:

$$\left\{ \mathbf{B} = (\mathbf{\Gamma}^\top)^{-1} \mathbf{\Delta}^\top, \mathbf{u}_t = (\mathbf{\Gamma}^\top)^{-1} \varepsilon_t \right\} \rightarrow \mathbf{G}(\varphi, \theta) = \mathbf{0}, \quad \varphi \in \Phi, \theta \in \Theta. \quad (12)$$

The substantive $\mathcal{M}_\varphi(\mathbf{z})$ and statistical $\mathcal{M}_\theta(\mathbf{z})$ premises can be disentangled by viewing the former as based on the theory and the latter as a parameterization of the observable process $\{(\mathbf{y}_t | \mathbf{X}_t), t \in \mathbb{N}\}$, as given in table 1 in terms of the testable probabilistic assumptions [1]-[5]; not as derived from $\mathcal{M}_\varphi(\mathbf{z})$.

This provides a purely probabilistic construal of $\mathcal{M}_\theta(\mathbf{z})$, with the Statistical Generating Mechanism (GM) being viewed as an orthogonal decomposition of the form:

$$\mathbf{y}_t = E(\mathbf{y}_t | \mathcal{D}_t) + \mathbf{u}_t, \quad t \in \mathbb{N},$$

where $\mu_t = E(\mathbf{y}_t | \mathcal{D}_t)$ denotes the systematic component, with $\mathcal{D}_t = (\mathbf{X}_t = \mathbf{x}_t)$ the relevant conditioning information set chosen with a view to render the 'educated' non-systematic component $\mathbf{u}_t = \mathbf{y}_t - E(\mathbf{y}_t | \mathcal{D}_t)$ a martingale difference process, i.e. $E(\mathbf{u}_t | \mathcal{D}_t) = \mathbf{0}$. In this sense, the statistical error term \mathbf{u}_t is [i] *derived* and represents non-systematic statistical information in \mathbf{Z}_0 relative to μ_t , and [ii] *local* in the sense that it pertains to the statistical model

$\mathcal{M}_\theta(\mathbf{z})$ vis-a-vis the data \mathbf{Z}_0 . In contrast, the structural error term ε_t is [i]* *autonomous* and could represent errors of measurement, errors of approximation, omitted effects, shocks etc., as well as [ii]* *global* in the sense that it pertains to the structural model $\mathcal{M}_\varphi(\mathbf{z})$ vis-a-vis the phenomenon of interest. $\mathcal{M}_\theta(\mathbf{z})$ is specified in terms of $D(\mathbf{y}_t|\mathbf{x}_t; \theta)$ via the probabilistic reduction:

$$\begin{aligned} D(\mathbf{Z}_1, \dots, \mathbf{Z}_n; \phi) &\stackrel{!}{=} \prod_{t=1}^n D_t(\mathbf{Z}_t; \varphi(t)) \stackrel{\text{iid}}{=} \prod_{t=1}^n D(\mathbf{Z}_t; \varphi) \\ &= \prod_{t=1}^n D(\mathbf{y}_t|\mathbf{x}_t; \varphi_1)D(\mathbf{x}_t; \varphi_2), \end{aligned} \quad (13)$$

rendering $\theta = H(\varphi_1)$ a particular parameterization of the process $\{\mathbf{Z}_t, t \in \mathbb{N}\}$.

Table 1 - The Multivariate Linear Regression Model

Statistical GM:		$\mathbf{y}_t = \beta_0 + \mathbf{B}_1^\top \mathbf{x}_t + \mathbf{u}_t, t \in \mathbb{N}.$
[1]	Normality:	$D(\mathbf{y}_t \mathbf{x}_t; \theta)$ is Normal
[2]	Linearity:	$E(\mathbf{y}_t \mathbf{X}_t=\mathbf{x}_t) = \beta_0 + \mathbf{B}_1^\top \mathbf{x}_t$, linear in \mathbf{x}_t ,
[3]	Homoskedasticity:	$Cov(\mathbf{y}_t \mathbf{X}_t=\mathbf{x}_t) = \Sigma$, free of \mathbf{x}_t ,
[4]	Independence:	$\{\mathbf{y}_t \mathbf{X}_t=\mathbf{x}_t\}, t \in \mathbb{N}\}$ - independent process
[5]	t-invariance:	$\theta := (\beta_0, \mathbf{B}_1, \mathbf{V})$ are constant for all $t \in \mathbb{N}$.
		$\beta_0 = \mu_1 - \mathbf{B}_1^\top \mu_2, \mathbf{B}_1 = \Sigma_{22}^{-1} \Sigma_{21}, \mathbf{V} = \Sigma_{11} - \Sigma_{21}^\top \Sigma_{22}^{-1} \Sigma_{21},$
		$\mu_1 = E(\mathbf{y}_t), \mu_2 = E(\mathbf{X}_t), \Sigma_{11} = Cov(\mathbf{y}_t), \Sigma_{21} = Cov(\mathbf{X}_t, \mathbf{y}_t), \Sigma_{22} = Cov(\mathbf{X}_t)$

From this perspective, the choice of $\mathcal{M}_\theta(\mathbf{z})$ begins with data \mathbf{Z}_0 , irrespective of the theory or theories that led to its choice. Once selected, data \mathbf{Z}_0 take on ‘a life of its own’ as a particular realization of a generic process $\{\mathbf{Z}_t, t \in \mathbb{N}\}$. The link between data \mathbf{Z}_0 and the process $\{\mathbf{Z}_t, t \in \mathbb{N}\}$ is provided by a pertinent answer to the key question: ‘what probabilistic structure, when imposed on the process $\{\mathbf{Z}_t, t \in \mathbb{N}\}$, would render data \mathbf{Z}_0 a truly *typical realization* thereof?’ (Spanos, 2006a). The answer offers the relevant probabilistic structure for $\{\mathbf{Z}_t, t \in \mathbb{N}\}$, which gives rise to the model in table 1. An answer that can be assessed using thorough M-S testing to assess the validity of model assumptions, such as [1]-[5]; see Mayo and Spanos (2004). The structural model $\mathcal{M}_\varphi(\mathbf{z})$ enters the picture when choosing a particular parameterization $\theta \in \Theta$ for $\{\mathbf{Z}_t, t \in \mathbb{N}\}$ so that $\mathcal{M}_\varphi(\mathbf{z})$ is nested parametrically in $\mathcal{M}_\theta(\mathbf{z})$ via $\mathbf{G}(\theta, \varphi) = \mathbf{0}$.

Generalizing the above distinction, one can argue that behind every structural model, generically specified by:

$$\mathcal{M}_\varphi(\mathbf{z}) = \{f(\mathbf{z}; \varphi), \varphi \in \Phi \subset \mathbb{R}^p\}, \mathbf{z} \in \mathbb{R}_Z^n, p < n, \quad (14)$$

where $f(\mathbf{z}; \varphi)$ is the joint distribution of the sample $\mathbf{Z} := (\mathbf{Z}_1, \dots, \mathbf{Z}_n)$, there exists (often implicit) a statistical model, taking the generic form:

$$\mathcal{M}_\theta(\mathbf{z}) = \{f(\mathbf{z}; \theta), \theta \in \Theta \subset \mathbb{R}^m\}, \mathbf{z} \in \mathbb{R}_Z^n, m \geq p, \quad (15)$$

that can be viewed as a parameterization of the observable stochastic process $\{\mathbf{Z}_t, t \in \mathbb{N}\}$ underlying data \mathbf{Z}_0 , and the statistical adequacy of $\mathcal{M}_\theta(\mathbf{z})$ underwrites the reliability of all inferences based on $\mathcal{M}_\varphi(\mathbf{z})$ via $\mathbf{G}(\theta, \varphi) = \mathbf{0}$.

This perspective enables one to assess the statistical validity of $\mathcal{M}_\theta(\mathbf{z})$, by testing its assumptions, e.g. [1]-[5], independently of $\mathcal{M}_\varphi(\mathbf{z})$, since [1]-[5] concern only the data \mathbf{Z}_0 . This purely probabilistic construal of $\mathcal{M}_\theta(\mathbf{z})$ enables one to delineate the two very distinct questions which are often conflated:

[a] *statistical adequacy*: does $\mathcal{M}_\theta(\mathbf{z})$ account for the chance regularities in \mathbf{Z}_0 ?

[b] *substantive adequacy*: does the model $\mathcal{M}_\varphi(\mathbf{z})$ adequately capture (describes, explains, predicts) the phenomenon of interest?

Statistical adequacy is established by probing thoroughly the assumptions of $\mathcal{M}_\theta(\mathbf{z})$ using trenchant M-S tests and ascertaining that no departures are detected. This addresses the concerns of the LSE tradition about statistical misspecification by ensuring that any inferences based on $\mathcal{M}_\theta(\mathbf{z})$ are reliable in the sense that the *actual error probabilities* for a test, a confidence/prediction interval, approximate closely the *nominal* (assumed) ones. Applying a .05 significance level test, when the actual type I error is closer to .9 will lead an inference astray; such discrepancies can arise with, what in a textbook econometric terms might be described as, ‘minor’ departures. What matters is not the ‘size’ of the departure but the magnitude of the discrepancy between actual and nominal probabilities it induces; see Spanos and McGuirk (2001).

$\mathcal{M}_\theta(\mathbf{z})$ is built exclusively on the statistical information contained in data \mathbf{Z}_0 , and acts as a *mediator* between $\mathcal{M}_\varphi(\mathbf{z})$ and \mathbf{Z}_0 . The ontological commitments in specifying $\mathcal{M}_\theta(\mathbf{z})$ concern the existence of:

(a) a rich enough probabilistic structure to ‘model’ the chance regularities in \mathbf{Z}_0 ,

(b) a $\theta^* \in \Theta$ such that $\mathcal{M}_{\theta^*}(\mathbf{z}) = \{f(\mathbf{z}; \theta^*)\}$, $\mathbf{z} \in \mathbb{R}_Z^n$, could have generated \mathbf{Z}_0 .

On the other hand, $\mathcal{M}_\varphi(\mathbf{z})$ is viewed as aiming to approximate the *actual mechanism* underlying the phenomenon of interest by using abstraction, simplification, and focusing on particular aspects (selecting the relevant observables \mathbf{Z}_t) of this phenomenon, and should be assessed as such. To establish substantive adequacy one needs to secure statistical adequacy first, and then proceed to probe for several potential errors, like omitted but relevant factors, false causal claims, etc.

It is important to note that the notion of statistical adequacy is related to the LSE tradition’s notion of *congruence* with some important differences, including the fact that congruency assumes ‘homoscedastic, innovation errors’ and ‘theory consistent, identifiable structures’ (Hendry, 1987). Statistical adequacy assumes (indirectly) *martingale difference* errors (that could be heteroskedastic), and it purposely excludes any form of theory consistency to allow one to separate, *ab initio*, the statistical from the substantive assumptions. As argued next, the distinction between the statistical $\mathcal{M}_\theta(\mathbf{z})$ and the substantive $\mathcal{M}_\varphi(\mathbf{z})$ premises is instrumental in elucidating and addressing several crucial methodological issues and problems in econometrics, as well as countering the critics of the LSE tradition.

5.2 Substantive vs. Statistical Premises of Inference

1. *'Realisticness' vs. statistical misspecification.* The confusion between substantive vs. statistical inadequacy is pervasive in the pre-eminence of theory literature as exemplified by claims like Prescott's (1986, 84): "The models constructed within this theoretical framework are necessarily highly abstract. Consequently, they are necessarily false, and statistical hypothesis testing will reject them."

It is one thing to say that a structural model $\mathcal{M}_\varphi(\mathbf{z})$ is a crude approximation of the reality it aims to capture, and entirely another to claim that the implicitly assumed statistical model $\mathcal{M}_\theta(\mathbf{z})$ could *not* have generated data \mathbf{Z}_0 , which is what statistical inadequacy amounts to. Hence, a structural model may always come up short in securing a substantively adequate $\mathcal{M}_\varphi(\mathbf{z})$ for the phenomenon of interest, but $\mathcal{M}_\theta(\mathbf{z})$ may be perfectly adequate for answering substantive questions of interest. Hence, there is nothing wrong with constructing simple, abstract and idealized theory-models. It becomes problematic when the data \mathbf{Z}_0 are given the *subordinate* role of 'quantifying' $\mathcal{M}_\varphi(\mathbf{z})$ in ways that (i) largely ignore the probabilistic structure of the data, (ii) employ unsound links between $\mathcal{M}_\varphi(\mathbf{z})$ and the data \mathbf{Z}_0 , like calibration and moment matching, and (iii) the probing of the substantive adequacy of $\mathcal{M}_\varphi(\mathbf{z})$ is ignored; see Spanos (2014).

2. *Statistical model validation vs. inference.* The above perspective brings out the distinct nature stemming from the different questions they pose to the data by the *statistical model validation* and the *inferential components* of modeling. M-S testing assesses whether the family $\mathcal{M}_\theta(\mathbf{z})$, $\theta \in \Theta$ could have generated \mathbf{Z}_0 , regardless of the 'true' value θ^* of θ . Statistical inference takes that for granted and aims to narrow Θ down to θ^* - whatever θ^* happens to be! The former precedes the latter and constitutes a separate stage of empirical modeling that secures the **reliability of inference**. Moreover, blending the two components into an overall decision theoretic problem can lead to fallacious framing like the pre-test bias claim; Spanos (2010a).

Hence, a structural model $\mathcal{M}_\varphi(\mathbf{z})$ in the context of the SEM is said to be *empirically valid* when (Spanos, 1990):

- (a) the implicit statistical model $\mathcal{M}_\theta(\mathbf{z})$ is statistically adequate and
- (b) the *overidentifying restrictions*: $\mathbf{G}(\varphi, \theta) = \mathbf{0}$ are *data-acceptable*.

The testing in (b) is a signature issue for the LSE tradition aiming to distinguish between pertinent and non-pertinent substantive information, and constitutes the first step towards establishing the substantive adequacy of $\mathcal{M}_\varphi(\mathbf{z})$ vis-a-vis the phenomenon of interest. Under (a)-(b) the estimated *empirical model* $\mathcal{M}_{\hat{\varphi}}(\mathbf{z})$: $\mathbf{\Gamma}(\hat{\varphi})^\top \mathbf{y}_t + \mathbf{\Delta}(\hat{\varphi})^\top \mathbf{x}_t = \hat{\varepsilon}_t$, enjoys both statistical and theoretical meaningfulness. Hence, it can be used as the basis of inferences, including prediction and policy simulations. This perspective passes the onus of bridging the gap between theory and data onto the theorist, by calling for structural models that are empirically valid in the sense of (a)-(b). In this sense, the LSE's link between the theory and the data in the form of

long-run solutions and error-correction specifications, although expedient, are too weak if the primary objective is to secure substantively adequate structural models.

3. *Time-series vs. cross-section data.* Viewing a statistical model $\mathcal{M}_\theta(\mathbf{z})$ as a parameterization of $\{\mathbf{Z}_t, t \in \mathbb{N}\}$ renders the distinction between time-series and cross-section data models (see Wooldridge, 2012, 344) misleading, since viewing data as realizations of stochastic processes is equally applicable to both types of data. The only tenuous difference between the two types of data is that for time series data there is one natural ordering, time, which is an interval scale variable, but for cross-section data there might be several natural orderings of interest, like spatial location, size, gender, age, etc., whose scale of measurement might be ordinal, nominal or interval. Hence, the claim that for cross-section data one does not need to worry about dependence or/and heterogeneity is misguided. The LSE tradition's concerns about statistical misspecification and the ensuing unreliability of inference for time series data are even more relevant for cross-section data.

4. *Revisiting the Gauss-Markov theorem.* Despite its historical importance in the development of statistical modeling and inference, when viewed from the above perspective the Gauss-Markov theorem is at best of very limited value and at worst highly misleading. First, 'linearity' ($\hat{\beta} = \mathbf{L}\mathbf{y}$) is a phony property, unbiasedness ($E(\hat{\beta}) = \beta$) without consistency is useless, and *relative* efficiency within an artificially restricted class of estimators is of very limited value, the theorem does not provide a sufficient enough basis for inference. For instance, it cannot be used to test $H_0: \beta = \mathbf{0}$, since knowing that $\hat{\beta} \sim \overset{?}{\mathbf{D}}(\beta, \sigma^2(\mathbf{X}^\top \mathbf{X})^{-1})$ with $\mathbf{D}(\cdot)$ unknown provides insufficient information for reliable inferences; see Bahadur and Savage (1956). Second, broad statistical premises yield imprecise inferences that often invoke $n \rightarrow \infty$ without any assurance of enhanced reliability. Indeed, when such broad premises include non-testable assumptions, as in the case of nonparametric models, the reliability of inference is at best unknown. Learning from data takes place when one applies reliable [actual error probabilities \simeq nominal ones] and incisive inferences [optimal methods] stemming from the statistical adequacy of $\mathcal{M}_\theta(\mathbf{z})$; Spanos (2012). Statistical adequacy is the price one has to pay to secure learning from data. Hence, the emphasis on complete and internally consistent set of probabilistic assumptions pertaining to observable process $\{\mathbf{Z}_t, t \in \mathbb{N}\}$ underlying \mathbf{Z}_0 , in contrast to an incomplete set of error term assumptions, mixed in with substantive assumptions like 'no omitted variables', etc.; Spanos (2010c).

5. *Revisiting Instrumental Variables (IV).* The above distinction between $\mathcal{M}_\varphi(\mathbf{z})$ and $\mathcal{M}_\theta(\mathbf{z})$ sheds very different light on IV estimators and the choice of 'optimal' instruments. Behind every IV estimator there is an implicit reduced form whose statistical adequacy is taken for granted. However, if the latter is statistically misspecified, the sampling distribution of the IV estimator will differ from the assumed, and that would give rise to unreliable inferences. Hence, the choice of instruments should be based on a statisti-

cally adequate reduced form which would often require respecification to include lags and trends in the case of time series data. That is, the choice of instruments is not based solely on theoretical information; statistical information plays a crucial role in determining the optimal instruments needed to secure the statistical adequacy of the implicit reduced form; see Spanos (1986).

Similarly, despite confident declarations to the contrary: "One must decide which variables are endogenous and which are conditioning variables using outside criteria." (Wooldridge, 1998, 297).

Statistical information plays a crucial role in determining which variables can be treated as conditioning variables. As shown in Spanos (1994), when the distribution $D(\mathbf{Z}_t; \varphi)$ in (13) is Student's t, weak exogeneity (Engle et al, 1983) does not hold for *statistical* reasons, and thus one needs to retain $D(\mathbf{x}_t; \varphi_2)$ for inference purposes.

6. *Model validation vs. model selection.* The same distinction clarifies the difference between *model validation* at the statistical level, which refers to establishing the statistical adequacy of $\mathcal{M}_\theta(\mathbf{z})$, and *model selection* at the substantive level which concerns $\mathcal{M}_\varphi(\mathbf{z})$. The problems of omitted variables or selecting the relevant regressors (Sargan, 1981) belongs to the latter category. What is crucial when posing substantive questions of interest, however, is the reliability of the test which is secured when $\mathcal{M}_\theta(\mathbf{z})$ is statistically adequate. No evidence for or against a structural model $\mathcal{M}_\varphi(\mathbf{z})$ can be established on the basis of a misspecified $\mathcal{M}_\theta(\mathbf{z})$.

This relates to the LSE strategy of *general-to-specific* in conjunction with *encompassing* that aim to address model validation and selection simultaneously. This strategy is most effective in the special case where (i) all the potentially relevant variables are included in data \mathbf{Z}_0 at the outset, and (ii) the general family of models selected includes a statistically adequate one. However, irrespective of whether one uses a general-to-specific or a specific-to-general testing procedure, the key issues are: (i) keep track of the relevant error probabilities, and (ii) ensure that inferences rely on a statistically adequate model to secure their reliability; see Spanos (2006b).

Akaike-type model selection: Sargan's intuition that Akaike-type criteria, like the AIC, are inadequate for model selection (see Phillips, 1985, 133) is fully justified when viewed in the context of the above modeling perspective. It can be shown that the AIC *ranking* of the different models is inferentially equivalent to *pairwise comparisons* among the different models in $G_\varphi(\mathbf{z}) = \{\mathcal{M}_{\theta_i}(\mathbf{z}), i=1, 2, \dots, m\}$, using N-P testing, but with a serious flaw: it ignores the relevant error probabilities. Moreover, these model selection procedures are in direct conflict with model validation using thorough M-S testing to secure statistical adequacy. This is because M-S testing would give rise to a choice of a particular model within $G_\varphi(\mathbf{z})$, assuming it includes such an adequate model, but this choice will rarely coincide with the AIC highest ranked model. Worse, the AIC will yield a highest ranked model even when $G_\varphi(\mathbf{z})$ does not include a statistically adequate one; see Spanos (2010b).

7. *Spurious results*. Spurred by the general impression that all statistical methods which rely on ‘regularities’ in the data are highly susceptible to the statistical spuriousness problem, textbook econometricians criticize the LSE perspective as indulging in a more sophisticated form of ‘data mining’; see Faust and Whiteman (1997). Contrary to such view, statistical adequacy provides the key to explaining *spurious results*, including the classic paper by Yule (1926) relating to the LR model as the result of departures from the model assumptions, such as [1]-[5] (table 1). Similarly, the Granger and Newbold (1974) spurious results stem from the fact that the simulated data have temporal dependence which is ignored by the estimated LR model. Indeed, their simulation results constitute a classic example of the actual error probabilities being different from the nominal ones due to statistical misspecification. Phillips (1986) derived the sampling distributions of the estimated parameters under the misspecification to shed light on simulation results. Despite its unquestionable importance, such derivations do not address the problem of spurious results. For that one needs to respecify the LR model to account for the temporal dependence in the data ignored by the original specification using the Dynamic Linear Regression:

$$y_t = \alpha_0 + \alpha_1 x_t + \alpha_2 x_{t-1} + \alpha_3 y_{t-1} + \varepsilon_t, \varepsilon_t \sim \text{NIID}(0, \sigma_0^2), t \in \mathbb{N}.$$

8. *Mis-Specification (M-S) testing*: thoroughly probing the validity of the probabilistic assumptions of $\mathcal{M}_\theta(\mathbf{z})$, e.g. [1]-[5], vis-a-vis data \mathbf{Z}_0 . This only concerns the question [a] above, and constitutes ‘testing outside’ the boundaries of $\mathcal{M}_\theta(\mathbf{z})$ aiming to exhaustively probe the set $\mathcal{P}(\mathbf{z})$ of all possible models that could have given rise to \mathbf{Z}_0 . The generic hypotheses for M-S testing take the form:

$$H: f^*(\mathbf{z}) \in \mathcal{M}_\theta(\mathbf{z}) \quad \text{vs.} \quad \bar{H}: f^*(\mathbf{z}) \in [\mathcal{P}(\mathbf{z}) - \mathcal{M}_\theta(\mathbf{z})],$$

where $f^*(\mathbf{z})$ is the ‘true’ distribution of the sample. This framing should be contrasted with N-P testing which constitutes ‘testing within’ $\mathcal{M}_\theta(\mathbf{z})$:

$$H_0: f^*(\mathbf{z}) \in \mathcal{M}_0(\mathbf{z}) = \{f(\mathbf{z}; \boldsymbol{\theta}), \boldsymbol{\theta} \in \Theta_0\} \quad \text{vs.} \quad H_1: f^*(\mathbf{z}) \in \mathcal{M}_1(\mathbf{z}) = \{f(\mathbf{z}; \boldsymbol{\theta}), \boldsymbol{\theta} \in \Theta_1\}.$$

That is, M-S testing is proper statistical testing, but different from N-P testing! The differences between them raise a number of conceptual and technical issues, including ‘how to particularize $[\mathcal{P}(\mathbf{z}) - \mathcal{M}_\theta(\mathbf{z})]$ to construct M-S tests’, ‘how to interpret a rejection of H ’, and ‘how to secure the effectiveness/reliability of the diagnosis’. The latter can be rendered effective by following specific strategies, including:

(a) *astute ordering* of M-S tests so as to exploit the interrelationship among the model assumptions with a view to ‘correct’ each other’s diagnosis using,

(b) *joint M-S tests* (testing several assumptions simultaneously) designed to minimize the maintained assumptions, and

(c) combining *parametric* (high power but narrow scope) and *nonparametric* (low power but broad scope) tests; see Spanos (2000, 2010b).

These features of M-S testing can be used to explain away several confusions and misplaced charges level against it, including infinite regress, circularity, double counting, multiple testing and data mining; see Spanos (2010a-b).

9. *Respecification* of $\mathcal{M}_\theta(\mathbf{z})$ to account for systematic information in the data calls for returning to the stochastic process $\{\mathbf{Z}_t, t \in \mathbb{N}\}$ underlying data \mathbf{Z}_0 with a view to choose a more pertinent probabilistic structure; see Spanos (1994). In the case of time series data NIID that underlies the model in table 1 is likely to be impertinent and replacing it with Normal, Markov, mean-heterogeneous and covariance stationary might be more appropriate. Hence, the LSE strategy of using statistical models with trends and lags in the case of time series data is often the only sound move if ‘learning from data’ is to be attained. This calls for replacing $\mathcal{D}_t = (\mathbf{X}_t = \mathbf{x}_t)$ with $\mathcal{D}_t^* = (\mathbf{X}_t = \mathbf{x}_t, \mathbf{Z}_{t-1})$, irrespective of the theory, since the parametric nesting via $\mathbf{G}(\varphi, \theta) = \mathbf{0}$ is easily retained when adding trends and lags. For substantive adequacy purposes, however, one needs to replace generic terms like trend polynomials—which represent ignorance—with relevant explanatory variables; see Spanos (2010c).

When viewed from the above perspective, the ‘error-fixing’ strategies of the textbook approach, like error-autocorrelation and heteroskedasticity ‘corrections’ can be blamed for contributing significantly to the untrustworthiness of the empirical evidence in econometrics journals. This is primarily due to two interrelated sources: (a) the neglect of establishing statistical adequacy, and (b) setting up the fallacy of rejection as normal practice. Hence, textbook econometric arguments such as: “my recommendation to applied researchers would be to omit the tests of normality and conditional heteroskedasticity, and replace all conventional standard errors and covariance matrices with heteroskedasticity-robust versions.” (Hansen, 1999, 195) are misplaced because the form of non-Normality could matter, and the ‘corrections’ do nothing to address the unreliability of inference problem that concerns the discrepancy between actual and nominal error probabilities; Spanos and McGuirk (2001), Spanos and Reade (2014).

Similarly, textbook claims like ‘departures from the no-correlation assumption only affect the efficiency and not the unbiasedness and consistency of the OLS estimator $\hat{\beta} = (\mathbf{X}^\top \mathbf{X})^{-1} \mathbf{X}^\top \mathbf{y}$ ’ are highly questionable. This is because this claim relies on two dubious (but LSE-tradition testable) pre-suppositions:

(i) the Dynamic Linear Regression model in (8) is statistically adequate for the particular data \mathbf{Z}_0 – evading the fallacy of rejection–, and

(ii) the common factor restrictions (9) are valid for \mathbf{Z}_0 .

In practice, the stipulations (i)-(ii) are unlikely to hold, rendering both the OLS $\hat{\beta}$ and the GLS $\tilde{\beta} = (\mathbf{X}^\top \mathbf{V}_n^{-1} \mathbf{X})^{-1} \mathbf{X}^\top \mathbf{V}_n^{-1} \mathbf{y}$ estimators *inconsistent*, giving rise to untrustworthy evidence; see McGuirk and Spanos (2008).

10. *Addressing Fallacies*. The distinction between $\mathcal{M}_\varphi(\mathbf{z})$ and $\mathcal{M}_\theta(\mathbf{z})$ in conjunction with the notion of *severity* can be used to address certain foundational problems associated with frequentist testing, including safeguard-

ing inferences against:

- (a) *the fallacy of acceptance*: interpreting accept H_0 [no evidence against H_0] as evidence for H_0 ; e.g. the test had low power to detect existing discrepancy, and
- (b) *the fallacy of rejection*: interpreting reject H_0 [evidence against H_0] as evidence for a particular H_1 ; e.g. statistical vs. substantive significance (Mayo & Spanos, 2006).

A retrospective view of the textbook respecification strategy of adopting the particular alternative H_1 when the M-S test rejects H_0 , is an example of the fallacy of rejection. For instance, a rejection of H_0 in (5) by the D-W test (6) provides evidence *against* H_0 and *for* the presence of temporal dependence of the generic form $E(\varepsilon_t \varepsilon_s | \mathbf{X}_t = \mathbf{x}_t) \neq 0$ in (3), but does *not* provide evidence *for* the particular form (4) assumed by H_1 . For that, one needs to validate the assumptions of the alternative model in (2), which involves confirming the presuppositions (i)-(ii) in 9.

Despite several papers by the LSE tradition questioning the ‘error-auto-correlation correction’ (Mizon, 1995b), Wooldridge (1998) criticizes Hendry (1995, 297):

Another notable feature of Hendry’s approach is his insistence that all conditional means have fully specified dynamics. He and some others have been on this crusade for years, and, at least in the United States, have had little impact on mainstream empirical econometrics. Static models and finite distributed lag (FDL) models are estimated routinely, often with corrections for serial correlation.

This comment epitomizes the attitude of the textbook tradition. It has institutionalized fallacious reasoning in the form of ‘error-fixing’ strategies, and chides anybody departing from it. Any attempt to elucidate the fallacies of acceptance/rejection by replacing confusing notions with more pertinent concepts is dismissed as nothing more than “mounds of jargon” or “ill-conceived idiosyncratic treatment”.

6 Conclusions

Since the mid 1960s the LSE tradition has contributed many innovative techniques and modeling strategies to applied econometrics. Its perspective differs from other post-Cowles traditions, in so far as it strives to strike a balance between the theory-oriented textbook econometrics and the data-oriented traditions by giving the data ‘a voice of its own’, without ignoring pertinent substantive information.

Denis Sargan is undoubtedly the ‘father’ of the LSE tradition, but the protagonist who brought out the revolutionary nature of the LSE perspective and unflaggingly endeavored to change empirical modeling in economics was David Hendry. Their different personalities complemented each other in a way that contributed significantly to the success of that tradition. Sargan was a reluctant revolutionary because he saw himself as pursuing the agenda set out by the Cowles Commission in the early 1950s. He

was a lot more comfortable discussing Instrumental Variables, Edgeworth expansions and Gram-Charlier approximations than methodological issues pertaining to empirical modeling. In contrast, Hendry relished the opportunity to compare different approaches to modeling, and break new ground by introducing alternative inference procedures and modeling strategies that improve learning from data.

The above retrospective appraisal of the LSE tradition revealed that its key contributions revolved around Haavelmo's call "to build models that explain what has been observed", and its perspective could be justified on sound philosophical grounds; see Spanos (2010a; 2012). Its full impact on applied econometrics will take time to unfold, but the pervasiveness of its main message stems from the fact that, in fields like economics a statistically adequate model could play a crucial role in guiding the search for better theories (substantively adequate) by demarcating 'what there is to explain'. This calls for paying sufficient attention to accounting for the statistical regularities in the observed data. Kepler's 'law' for the elliptical motion of the planets was originally just an empirical regularity that eventually guided Newton toward his theory of universal gravitation; see Spanos (2007).

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