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The First Fifty Years of Modern Econometrics

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Abstract

We characterize modern econometrics in terms of the emergence a widely accepted analytical framework. A major theme which dominated much of the debate through the century was whether and how econometric models can reflect theory-generated economic structures. In the period prior to the 2nd world war, economists adopted a wide variety of analytical methods, some ad hoc but others reflecting advances in statistical methodology. Business cycle analysis and demand analysis were the two major areas in which statistical theory was employed. Methods became increasingly formalized but problems of data adequacy, estimation and identification were not always well distinguished. During and immediately after the war, Cowles Commission research sought to base econometrics on autonomous probabilistic models specified in terms of underlying structural parameters. Least squares would not normally be consistent in such models and maximum likelihood estimation was to be preferred. Subsequently, however, the pendulum swung back towards least squares-based methods and this was reflected ion the textbook expositions of what was accepted as standard econometrics in the late sixties and early seventies. Subsequently, the paradigm was undermined by the challenges imposed by rational expectations modelling, which challenged standard identification assumptions, and by the poor forecasting performance of many macroeconomic models by comparison with black box time series competitors. The result was a revival of non-structural modelling, particularly in the analysis of macroeconomic data.

Key words: Econometrics, History, Estimation, Identification
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1 Introduction

In what follows, we characterize modern econometrics in terms of the emergence a widely accepted standard framework in which economic data can be analyzed and on the basis of which subsequent methodological developments would take place. We argue that it was only in the mid nineteen sixties that this framework had come into existence. On that basis, the first fifty years of modern econometrics, interpreted is the period in which the framework was being generated, starts around the time of the First World War. This seems right – it is possible to find precursors of modern methods prior to 1914, but they were isolated, had relatively little influence and did not constitute a movement.

From the mid nineteen sixties onwards, econometrics was established and accepted as a discipline in its own right. Advances in computer technology and software, together with the availability of larger and higher quality datasets, eliminated computational constraints and permitted real advances. These produced solid dividends in microeconometrics, but less so in the analysis of aggregate macroeconomic time series. The greater diversity apparent over the final three decades of the century demonstrates a new maturity in the discipline.

2 Rise of modern Econometrics: The pre-1940 period

At the start of the twentieth century, there were no generally accepted and established paradigms for synthesizing data evidence and theory. Various mathematical and statistical methods were employed, economic postulates were rearticulated in mathematical equations, different approaches were tried and debated and new concepts and new schemes were proposed. However, all of these activities took place in a relatively unstructured environment, both conceptually and geographically. For these reasons, it is difficult to discuss the literature from this period in a systematic and comprehensive manner.[1] In what follows, we consider a selected group of influential works from this period under five interrelated topics: data-instigated attempts, theory-based investigations, theoretical inventions, pursuit for new statistical methods, and methodological debates.

2.1 Data-instigated analyses

A major area where data-instigated attempts thrived was business cycle analyses. However, these discussions adopted different perspectives and served different purposes.

[1] See (Morgan, 1990) and (Hendry and Morgan, 1995) for a comprehensive discussion.

This contrast is evident in comparison of the contemporaneous analyses of H.L. Moore (1914) and W.M. Persons (1916, 1919) which come right at the start of our fifty-year period.

Moore (1914) was interested in the question whether there was a regular length in business cycles. He related the observed cyclical patterns in grain yield and price data to cyclical climate conditions, such as those observed in rainfall data. The methods that he used were periodograms and Fourier frequency analysis. These helped Moore to arrive at a general causal explanation of business cycles and an estimate of 8-year per cycle average length. But such a partial explanation was obviously too naive to capture the distinct irregularities in business fluctuations. Persons (1916, 1919) was both more ambitious and more practical. His attempts to forecast the short-run business cycle movements entailed more intensive data analysis. Persons classified the forces he saw as the causes of economic data fluctuations into four types: secular (long-term) forces, seasonal forces, cyclical forces and accidental (irregular) forces. He first attempted to filter out the trend (secular) element from time-series data by the methods of moving averages and curve fitting. He then used seasonal median values to remove any obvious seasonal patterns. The remaining cyclical and irregular patterns in the filtered data were the focus of Persons' attention. He felt it very difficult to distinguish these two types of forces. Instead, he attempted to construct, out of numerous detrended and seasonal-adjusted time series, a few composite indices which would 'epitomise', and lead the way of, the general business situation. These indices formed the basis of what became known as Harvard ABC barometers (e.g. Samuelson, 1987). However, the barometers do not correspond to any economic causes of the cycles in spite of their practical appeals. Clearly, Persons was much more interested in how economic time series behave than testing or measuring theories.

Data-instigated investigations also emerged in other areas of economic studies. For example, A.L. Bowley (1933) examined the statistical distributions of a number of very different economic time series including changes in wholesale and retail prices and in household income. He attempted to relate departures from normality to specific economic factors. Around the same time, H. Working (1935) examined wholesale wheat prices with the objective of learning about how commodity futures prices respond to the impact of multiple factors under 'laws of dynamic equilibrium'. He related spot-futures differentials (premia or backwardations) in the Chicago wheat market to inventories and storage availability. He also showed how Chicago spot prices reacted to shocks from the US wheat market, to shocks originating in other US markets and to shocks originating from abroad (specifically from the Liverpool market). Working's research anticipates methods which have since become standard in financial econometrics.

The mid 1930s saw the impact of newly formed statistical inference procedures, specifically deriving from the work of R.A. Fisher, and also of J. Neyman and E.S. Pearson. The result was a subtle translation from search for economic 'laws' or regularities to a framework based on hypothesis testing.

R.H. Coase and R.F. Fowler's (1937) joint study on pig-cycle serves as an interesting illustration. They started their investigation to test whether the widely used cobweb model adequately described pig cycles by positing the model as null hypothesis. Their data analyses generated results rejected the cobweb model. In an effort to provide an alternative hypothesis, they discovered that expectations of future pork prices 'must be considered as independent variables determining the demand in the same way as actual prices'. As well as constituting a pioneering economic application of the new Neyman-Pearson methodology, this was a clear precursor of more recent expectations based modelling.

Perhaps the most ambitious attempt to use data evidence to test theories, treated as hypotheses rather than laws, was carried out by J. Tinbergen for the League of Nations in the late 1930s. G. von Haberler (1937) had already undertaken an extensive survey on various business-cycle theories. At Haberler's request, Tinbergen's task was to carry out statistical tests of these theories. This resulted in Tinbergen's (1939) monumental business cycle volume – see also (Morgan, 1990; Chapter 4). He tried to use the state of art of econometrics of the time, which shows a clear move of econometric practice from data-led approach to theory-led approach (see the following subsection 2.5).

In summary, the interwar period saw an exploration of new methods and a gradual move towards theory-driven explanations in what remained an applied (forecasting and policy) orientated environment.

2.2 Theory-based investigations

Turning to theory-based studies, most of these are clustered in consumption and demand analyses where theory was well-developed and widely accepted. Applied work was therefore able to focus on parameter measurement. The main obstacle encountered in these studies was the *ceteris paribus* gap between theory and data.

An early pioneer of statistical demand studies using price (p) and quantity (q) time-series data is H.L. Moore. For each commodity, Moore (1914, 1925) used ordinary least squares to estimate two regressions: $p_t = \alpha_0 + \alpha_1 q_t$ which he interpreted as the demand relation $q_t = \beta_0 + \beta_1 p_{t-1}$ which he interpreted as the supply relation.[2] Implicitly, the *ceteris paribus* condition is supposed satisfied through prior detrending of price and quantity data. Moore was led to believe that he had discovered a new type of demand curves when he obtained positive parameter estimates from his demand regression for some producer goods. His claim prompted discussions on how to empirically identify a demand curve from a supply curve using tine-series data sets – see the subsection 2.5 below.

R.A. Lehfeldt (1914) focussed explicitly on the *ceteris paribus* requirements in his estimate of the price elasticity of demand for wheat. He adjusted his data to make them as consistent as possible to the theoretical setting of the definition of the elasticity. The quantity series was standardized by population to remove the population growth effect; the price series was adjusted to remove the impact of possible supply-side dynamic shocks. The elasticity estimate was finally obtained not by regression but by taking the ratio of standard deviations of the logs of the pre-adjusted quantity to price series.

These two cases define the extremes. The bulk of demand studies combined adjusted data with interpretations of theory adapted according to the purposes of the studies. This is best seen from Tinbergen's (1930) ingenious study on supply curves. He tackled the problem of differentiating between supply and demand curves by adding different explanatory variables into the demand and supply equations respectively, e.g. a variable of domestic stock of the commodity into the demand equation and the production quantity supplied from abroad into the supply equation. He also used a time dummy to detrend the price series rather than detrending prior to regression. Finally, he derived and estimated the reduced form before inferring the demand and the supply coefficients indirectly, a technique which subsequently came to be known as Indirect Least Squares (ILS).

Flexible adaptation of theories, data and regression functional forms is quite commonly seen with early practitioners. For example, H. Staehle (1937, 1938) experimented with a consumption function estimated on German quarterly time-series data which explained retail sales by real labour income both with and without an additional variable measuring income inequality; whereas F.C. Dirks (1938) proposed an alternative, nonlinear, regression explaining the change of retail sales by the lagged change of income. C. Clark (1938) used both the current and a lagged income variables to estimate the UK consumption function, again on quarterly data. J.R.N. Stone and G. Stone (1938) experimented with a variety of functional forms including log-linear, log quadratic and

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[2] He also investigated polynomial regressions by adding quadratic and cubic regressors, e.g.

additional time trend when regressing aggregate consumption on income using both time-series and cross-section data sets.[3]

To summarize, the *ceteris paribus* gap between the received theory and the data was often too wide for empirical studies to be anchored directly on theory. This posed the question of whether theory was to be regarded as immutable, indicative or inadequate? Further, what criteria one should use to judge the empirical results, economic theory or statistical theory? Although the main purpose of demand and consumption studies was the measurement of elasticities and multipliers, these studies threw up new methods of data and inference, in particular in relation to the issues of identification and multicollinearity.

2.3 Exploration of statistical methods

At the start of the twentieth century, simple correlation, basic descriptive statistics and regression were the known and widely used by the small number of quantitatively-orientated economists. The need for new statistical methods gradually arose from a number of empirical experiments to fill gaps between data and theory. Early innovations were proposed as *ad hoc* methods to handle particular problems. For example, P.G. Wright (1928) came up with the idea of the instrumental variable (IV) method for the purpose of circumventing the simultaneity problem between demand and supply. Wright saw the need of using extra information and demonstrated how to get both demand and supply curves by using an additional factor uncorrelated with supply but correlated with demand, and vice versa. On handling the same simultaneity problem, Tinbergen (1930) derived the ILS method for estimating his simultaneous equations supply and demand model – see section 2.2.

Concerns over measure errors in variables were a dominant theme in the interwar period and these often motivated questions about the appropriate direction for regression. Both C. Gini (1921) and H. Schultz (1925) proposed orthogonal regression in estimating what are now referred to as errors-in-variables models. Similarly, R. Frisch (1929) developed diagonal mean regression as a solution for choosing the bivariate regression direction. The 1930s saw more concerted attempts to devise universally applicable statistical tools for economic data analysis. Frisch (1934b) invented the method of bunch map analysis in an attempt to resolve the problem of non-unique relationships holding simultaneously among a set of variables all subject to measurement errors – see (Hendry and Morgan, 1989). C.F.

$p_t = \alpha_0 + \alpha_1 q_t + \alpha_2 q_t^2 + \alpha_3 q_t^3$.

[3] For a detailed study on the history of consumption function, see (Thomas, 1989).

Roos (1937) and T.C. Koopmans (1937)[4] advocated explicit use of criteria of statistical optimality in choosing estimation methods and devised general forms of weighted regression as the best estimators for error-in-variable regression models. Koopmans based his derivation on the maximum-likelihood (ML) principle - see (Qin, 1993; Chapter 3). His analysis paved the way for systematic adoption of formal statistical criteria in what would become known as econometric inference.

Active interchanges between pioneering econometricians and statisticians as well as applied researchers in other disciplines resulted in recently invented statistical methods becoming known and used in economic analyses. For example, P.G. Wright (1928) introduced path analysis, which had been invented by his son, S. Wright for studies in genetics, for causal chain determination of prices and quantities of agricultural produces.[5] The early econometricians also quickly became aware of the new methods of principle components and canonical correlation invented by H. Hotelling (1933, 1936) in the context of factor analysis models applied to psychological data – see (Koopmans, 1937) – though applications were not reported until the 1940s – see (Waugh, 1942) and (Stone, 1947). Interestingly, these methods subsequently faded from econometric practice although they have now re-emerged in the context of cointegration analysis and leading indicator models.

In summary, we can see a steady process of formalization gathering place over the interwar period such that econometrics methods were becoming more clearly based on clear statistical foundations, while at the same time, there was an openness to comparable developments taking place in other disciplines.

2.4 Theoretical innovations

The need for a general and appropriate mathematical apparatus to help conceptualise and describe economic dynamics together with the difficulty in constructing such an apparatus became increasingly clear in the initial decades of the century. The initial approach was to describe business cycles in terms of Fourier series based on periodic analyses of time-series data, as in Moore (1914). The apparatus quickly came to be regarded as unhelpful due to its lack of foundation in economic theory and the difficulties

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[4] This was his 1936 doctoral thesis.

[5] Goldberger (1972) gives has a detailed description of Wright's path analysis; Farebrother (1986) describes the very early history of IV, which goes back to the 1750s.

in adapting it to account for the erratic features of economic data.[6] The method was further discredited by G.U. Yule's (1927) investigation on the impact of random shocks in time series which showed that cycles might be the result of sizable shocks (or errors) rather than regular cyclical factors which, on this view, would no longer be identifiable from the data.

Subsequent attempts mainly use difference equations or mixed difference and differential equations. This could be traced to the influential works of Yule (1927) and E. Slutsky (English translation 1937).[7] Yule argued that it was more straightforward to represent random time series by autoregressive equations in the time domain than in the frequency domain. Slutsky postulated that the observed cyclical time-series data were generated by a summation of random shocks and hence could be represented by a moving average model of random errors. These ideas were combined in Frisch's (1933a) macro-dynamic model. Frisch deplored the absence of any element of structural explanation in Yule's and Slutsky's time-series models. He rejected the decomposition of economic time series into trend, cyclical, seasonal and erratic shock components as 'mechanical'. In his view, macro-dynamics of economic time series could be characterised by the combination of a *propagation* mechanism and *impulse* shocks. To establish the propagation mechanism, one should start from a dynamically formulated economic model, i.e. a model which contains economically interpretable lagged (or lead) variables in the form of a mixed difference and differential equations,[8] and then augment this by random shocks. Frisch's programme evolved into a fuller agenda for econometric research which later became known as the structural approach – see (Frisch, 1937) and (Bjerkholt, 2005). His approach was experimented and popularised by Tinbergen's practical modelling work, firstly on the Dutch model – see (Tinbergen, 1936, English translation 1959) and (Morgan, 1990; Chapter 4) and then the US model for the League of Nations (Tinbergen, 1939).

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[6] W.L. Crum (1923) tried to use periodic analysis on cyclical patterns in commercial paper rates and found the results unsatisfactory. B. Greenstein (1935) used the method on business failures in the USA and yielded partially negative conclusions about the method.

[7] This is originally published in Russian in 1927.

[8] Frisch illustrated this by an investment demand equation. Dynamically formulated mathematical models of economic 'laws' were increasingly used both in macro and micro studies during this period, e.g. Roos's (1934) dynamic demand model and the well-known cobweb model – see (Ezekiel, 1938) for a survey.

The problem of how to characterise economic time series were discussed from a somewhat different angle by A. Wald (1936).[9] Wald refers to as 'internal' the definition of components of time series in terms of 'unequivocal functions of the original data', e.g. trend, cycle, seasonal and erratic shocks, as opposed to the 'external definition' which relates the time series to a set of *a priori* postulated economic causes. After pondering over various difficulties in starting the analysis from the theory-led 'external' definitions, Wald concluded that the only fruitful approach is to start from the data-led 'internal' definitions and to try to establish how 'the internal components correspond with the equivalent external components'. Technically, Wald's internal definition was strengthened by H.O.A. Wold's (1938) rigorous formulation of random time-series decomposition on the basis of probability theory and stochastic distribution theorems.[10] Methodologically, however, Wald's ideas were overshadowed by Frisch's (1937, 1938) persuasive argument for the structural approach,[11] until they were subsequently reflected in A.F. Burns's and W.C. Mitchell's (1946) methods for measuring business cycles (e.g. Morgenstern, 1961) and much later in the general-to-specific dynamic modelling approach (e.g. Hendry, 1995).

Towards the late 1930s, concerns over the economic foundation of applied econometric modelling had already come to dominate those relating to the dynamic characterisation of time-series decompositions – see (Qin, 1993; Chapter 2).[12] Empirical examination of time-series properties came to be reduced to an intermediate step prior to the derivation and estimation of structural time series models, known as the 'final equations' following Tinbergen (1939). Theoretical studies of economic dynamics of Frisch's (1933a) model were pursued by R.W. James and M.H. Belz (1936, 1938) and Koopmans (1940), who all used periodic analysis in examining the cyclical and dynamic features of estimated final equations. However, this approach faded in importance as a consequence of the probability revolution associated with the publication of (Haavelmo, 1944) (see the subsection below), which advocated a complete reformulation of structural models in terms of stochastic variables instead of a deterministic system of differential/difference equations augmented

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[9] This work was undertaken at the request of O. Morgenstern of the Vienna business cycle research institute - see (Hendry and Morgan, 1995; Introduction).

[10] Wold (1938) referred to the works of Yule, Slutsky, Frisch and Tinbergen. However, despite some citations, this work had little impact on the econometricians.

[11] 'External' factors were used to refer to non-structural random shocks in Haavelmo (1940).

[12] Wold (1938) referred to the works of Yule, Slutsky, Frisch and Tinbergen. However, despite some citations, this work had little impact on the econometricians.

by random errors. The frequency domain method largely disappeared in econometrics until the 1960s, see section 5.3.

In summary, the interwar period provides evidence of an increasing separation between the macroeconomic and econometric approaches to business cycle analysis. Theory formulation came to be delegated to economists, leaving econometricians with the task of specifying and estimating structural model. Data-led considerations of economic dynamics became marginalized by the theoretical worries about simultaneity and static equilibrium.

2.5 Methodological debates

Methodological debates promote and enliven development of sciences and econometrics is no exception in this regard. As illustrated in the previous sections, the early econometricians adopted different approaches and methods in exploring the best ways of theorising and modelling data evidence. Arguments and debates over the relative merits of these different choices were unavoidable.

An early and important issue for debates originates from demand analysis by means of regression. The methodological issue is whether and how one can be sure that the regression results correspond to demand curves. Lehfeldt and P.G. Wright immediately noted that the positive curve estimated by Moore (1914) was much more likely a supply curve than a demand curve – see (Morgan, 1990; Chapter 5) and (Hendry and Morgan, 1995; Part 3). This raised awareness of the substantial data-theory gap. Different opinions were put forwards as the main causes of the gap. For example, E.J. Working (1927) believed that lack of information was the main culprit to prevent modellers from identifying the theoretical curves of demand and supply, whereas M. Ezekiel (1928) disagreed and argued that the bivariate static demand equation was only part of the correct theoretical model of demand (i.e. the correct model should contain other economic factors). Correspondingly, different solutions were proposed either by pre-adjusting data to bring them closer to theory, by augmenting theory to bring it closer to data, or by choosing statistical concepts or tools thought to be appropriate to theoretical requirements.

The famous 'pitfalls' debate between W.W. Leontief and Frisch published in *Quarterly Journal of Economics* in the early 1930s[13] largely turns on the validity of Leontief's identification of economic theory with the statistical assumption of independence between the error terms of the demand and supply equations – see (Hendry and Morgan, 1995; Introduction and Chapter 22). For Frisch, this identification was misleading as the two error

[13] See (Frisch, 1933b, 1934a) and (Leontief, 1934a, 1934b).

terms were unlikely to correspond purely to independent demand and supply shocks. The debate was closely related to the issue of how applied modellers should conceptualise the error terms – whether they were measurement errors or errors in equations, and if the latter, whether they were not further decomposable to allow for augmented theoretical models.[14] The issue was also related to the appropriate choice of estimation methods, e.g. whether OLS was adequate rather than, say weighted least squares.

Pre-adjustment of data was a commonly used practice to try and bring data closer to a controlled experiment environment where theories were normally postulated, i.e. under the *ceteris paribus* condition. For example, Moore, and his student, Schultz, insisted that modellers should use detrended data to ensure sufficient dynamic variation to capture the underlying static demand and/or supply curves – see (Schultz, 1925, 1928). Prior detrending of the data was opposed by B.B. Smith (1925) and W.F. Ferger (1932) – see (Morgan, 1990; Chapter 5). However, Frisch and Waugh (1933) pointed out that the regression coefficients of a model of two level variables including a linear time trend and a model of the two variables pre-adjusted by removing from each a linear trend were actually identical. They argued that what was crucial to was to have an explicit *a priori* 'structural' model to enable applied modellers to interpret their statistical results in as close correspondence as possible to the structural model.

Frisch (1937) elaborated the idea of starting empirical modelling from a structural model in an 'ideal programme' that he sketched at the European Meeting of the Econometric Society in 1936 – see also (Frisch, 1938). It took a certain amount of persuasion for his programme to become the accepted econometric approach. As shown in the previous sections, the early econometricians chose various ways in bridging data with theory, some following a more theory-led approach while others were more data oriented. Tinbergen (1935), for example, favoured the data-led approach, which he referred to as the 'historical' method and which he contrasted to the theory-led 'structural' method. At that time, his view was that available theories were generally inadequate to explain the data at hand. However, he reversed his position in the late 1930s and endorsed Frisch's structural approach – see (Qin, 1993; Chapter 2).

A major factor that helped to unite Tinbergen and other econometricians under Frisch's structural approach was the scepticism of non-econometric economists in relation to the scientific validity of the nascent econometric methods. This scepticism was powerfully

[14] See (Qin and Gilbert, 2001) for further discussions on the history of the error terms.

expressed by John Maynard Keynes (1939, 1940) in his critique of Tinbergen's approach to testing business cycle theories – see also (Tinbergen, 1940). The issues of the debate was so fundamentally important for the econometricians that Frisch (1938), and J. Marschak and O. Lange (1940) joined ranks with Tinbergen in vehemently defending the new discipline.[15] The debate was also a motivation for T. Haavelmo's comprehensive exposition of the foundations of econometrics (see the next section).

2.6 Summary

By the end of the 1930s, econometrics had already growth into a vibrant enterprise. A great deal of the available raw materials had been studied and tried out. What remained absent was a coherent and unified view of the econometric programme which could be accepted by practitioners and understood by the remainder of the economics profession. The Keynes-Tinbergen debates emphasized this requirement. Moreover, there was an impetus to standardise econometric practice with a strong reliance on economics and to get econometrics recognised as a sub-discipline of economics. From a practical standpoint, and aside from the impact of Keynes' criticism, there were worries in relation to the dangers of nonsense and spurious regressions, illustrated by Yule (1926; 1927). Further, Tinbergen's models (Tinbergen, 1939, 1940) highlighted the potential role of models in policy analysis as distinct from just for out-of-sample forecasting. Policy analysis entails the construction of autonomous structural models, e.g. see (Frisch, 1938), (Marschak and Lange, 1940). However, the type of macrodynamic model explored by Frisch and Tinbergen was criticised for not being sufficiently theoretical by comparison with the approach of dynamically extended Walrasian system advocated by J. Hicks.[16] These and other considerations reinforced the perceived need to strengthen Frisch's structural approach to modelling.

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[15] Neither Frisch's memorandum (1938) nor Marschak and Lange's paper (1940) was published at the time. Marschak and Lange acknowledged that their paper benefited from discussions with Haavelmo, and the Chicago economists J.L. Mosak and T.O. Yntema.

[16] See G. Tintner's comment in the report of the Econometric Society Meeting at Philadelphia in 1939, (Leaven, 1940).

3 Formalization of econometrics: 1940s[17]

The formalization of Frisch's structural approach took place during the Second World War, and largely in the USA. The war itself was important for the development of econometrics in two distinct respects. First, and paradoxically, it served to bring many of the early econometricians together. As the Nazi onslaught closed down normal academic discourse in continental Europe, many active European econometricians sought refuge in the United States. The effect was to bring together the European scholars, coming from different traditions, both with other econometricians and also with American statisticians and econometricians. For example, Haavelmo, who came to Harvard from occupied Norway where he had studied under Frisch, was apparently converted to probabilistic reasoning by contact with Neyman, who was of Romanian origin and who had arrived at Columbia via Vienna (Morgan, 1990; p.242).

The Cowles Commission (henceforth, CC) played an important role in this process – see (Epstein, 1987). Founded in Colorado Springs in 1932, it moved to Chicago in 1939. US entry into the war in 1942 resulted in many key staff leaving. Marschak, who came via Oxford from the Marxist school of German economics, was appointed Director the same year. L. Hurwiz (Polish origin), Lange (Polish) and Koopmans (Dutch, Director of the CC from 1948) were among other Europeans recruited to the CC by Marschak. Haavelmo was invited as a consultant. Prominent Americans included T.W. Anderson, K.J. Arrow, C.F. Christ, J. Mosak and L. Klein, H.A. Simon, see (Christ, 1952).

The second respect in which the war facilitated econometrics is that it established an economic policy environment which favoured and even required active government. This was true at the macroeconomic level where there was now general acceptance of the need for fiscal activism to maintain full employment but also in the wartime and postwar regimes of regulated prices. As Epstein (1987) observes, price controls required knowledge of industry cost curves and even demand elasticities. Marschak saw econometrics as an instrument for 'social engineering', a phrase which was later softened to the analysis of 'rational economic policy', where rational was taken to imply statistically-based – see (Epstein 1987; pp.61-2). Those European econometric refugees to the United States who

[17] There is a relatively rich literature on this period of the history, see e.g. (Christ, 1952, 1994), (Hildreth, 1986), (Epstein, 1987), (Morgan, 1990) and (Qin, 1993). This section is a summary of the literature and builds heavily on (Qin, 1993).

came from socialist or social democratic backgrounds made an easy transition to technocratic implementation of the New Deal policies.

The formalization was mainly carried out at the CC under the leadership of first Marschak and subsequently Koopmans with much of the work undertaken by Haavelmo, Koopmans. Under Frisch's dominating influence (see e.g. Bjerkholt, 2005), Haavelmo laid the groundwork of shaping econometrics into a more systematic discipline within economics, see especially (Haavelmo, 1944). His work was further elaborated and standardised by the CC, culminating in the famous monographs 10 (Koopmans, 1950) and 14 (Hood and Koopmans, 1953). The formalisation involved systematising econometric procedures in model specification, estimation, identification and testing within the paradigm of a stochastic framework. The baseline economic model was the dynamic Walrasian general equilibrium system, i.e. a dynamic, simultaneous-equations model (SEM):

(3.1) $$A_0 x_t + A_1 x_{t-1} + \cdots + A_m x_{t-m} = u_t$$

where x_t is a vector of all the relevant variables of an economy, u_t a vector of random disturbances, and the As are structural parameter matrices to be estimated with A_0 non-diagonal matrix allowing the possibility of simultaneity.

3.1 Estimation

The early econometricians were already aware that the least squares estimation method had to be adapted in accordance with the assumed type of error terms in a regression model (see section 2.3 above). For example, when the two variables in a bivariate regression context were both subject to errors, weighted least squares should be used instead of OLS. Statistical optimality criteria were also brought in during the late 1930s, e.g. by Koopmans (1937), as explicit yardsticks for choosing estimators. Research in estimation methods during the 1940s emphasized the importance of combining these two concerns in choosing estimators, i.e. these estimators should be contingent on model formulation, especially on the error term specification, as well as on statistical optimality. As nonlinear estimators became the principal choice, the research opened up a new dimension – how to adapt the estimation procedure to ease the computational burden of nonlinear estimators.

Haavelmo (1943) was an influential work in the development of estimation methods for the SEM.[18] Haavelmo demonstrated that the OLS parameter estimates of an SEM, when

[18] The main result of this paper was already available in his 1941 mimeograph and known to Marschak, Wald, Koopmans and others well before 1943.

extended with error terms in equation, were inconsistent with the SEM specification. Such systematic inconsistency becomes known as 'Haavelmo bias'. Haavelmo showed that, if this inconsistency is to be corrected, it is necessary to consider the joint distributions of the variables in the stochastic SEM, a direction which would lead to the maximum likelihood (henceforth, ML) principle.

Haavelmo's advocacy of ML estimators in the SEM context raised questions about the statistical properties of these estimators. A significant statistical extension of Haavelmo's work was carried out by H.B. Mann and Wald (1943), who provided proofs of the consistency and asymptotic normality of the ML estimators of a linear, stochastic difference equation system with normally distributed errors. Mann and Wald also showed that certain *a priori* restrictions would be required of the closed SEM system (3.1) to achieve unique estimates, a condition linked to the issue of identification. They demonstrated this point by transforming the SEM into what they called the 'reduced form' of the model (a term which is now commonly accepted):

(3.2) $$\begin{aligned} x_t &= \left(-A_0^{-1}A_1\right)x_{t-1} + \cdots + \left(-A_0^{-1}A_m\right)x_{t-m} + \left(-A_0^{-1}\right)u_t \\ &= \Pi_1 x_{t-1} + \cdots \Pi_m x_{t-m} + \varepsilon_t \end{aligned}$$

Their derivation provides a formalisation of Tinbergen's (1930) ILS procedure.

The Mann and Wald work was further pursued by Koopmans and the CC group in three directions, see (Koopmans, 1950).[19] The first was to extend the closed SEM into an open SEM allowing the presence of exogenous variables. Business cycle research during the 1930s had encouraged extension of the above closed system into an open one in this way, with the vector of variables x_t being classified into a subset of endogenous variables y_t and sa second subset of exogenous variables z_t:

(3.3) $$B_0 y_t + B_1 y_{t-1} + \cdots + B_m y_{t-m} = \Gamma_0 z_t + \Gamma_1 z_{t-1} + \cdots + \Gamma_m z_{t-m} + u_t$$

with its reduced form:

(3.4) $$y_t = P_1 y_{t-1} + \cdots + P_m y_{t-m} + Q_0 z_t + \cdots + Q_m z_{t-m} + \varepsilon_t$$

The second extension was to separate the issue of whether a set of structural parameters is uniquely estimable from that of the choice of estimator – see the following section.

The third extension was to search for a simpler calculation procedure as the ML estimator is nonlinear and entailed high computational burden for any reasonably sizable SEM at a time when only hand calculators were available. This led to the development of limited information ML (LIML) estimators which concentrated the likelihood function with

[19] The work was mostly already complete by the mid 1940s, see (Epstein, 1987).

respect to 'nuisance' structural parameters to allow estimation of only those parameters in the equation of interest. The original system ML estimators were now termed full information ML (FIML).

The discovery of the "black magic" LIML method was apparently initiated by M.A. Girshick, who sent a letter to the CC in 1945 proposing to estimate, for simplicity, an SEM from its reduced form first and then solve back for the structural estimates, see Epstein (1987, 1989). This idea was quickly extended and formalised by T.W. Anderson and H. Rubin (1949) in the form of the LIML estimator.

To summarize, work under the leadership of the CC established a new paradigm in terms of estimation methods and statistical rigour explicitly linked on the one hand to model specification and identification conditions on the other with practical concerns about computability.

3.2 Identification

The formalisation of identification conditions, as taught in present-day econometrics textbooks, plays an essential role in the formalisation of econometrics as a sub-discipline in economics. Although the early econometricians developed *ad hoc* methods for tackling the identification problem in the 1920s and 1930s, it was not until the beginning of the 1940s that the problem was separated from estimation issues and clearly formulated in mathematical and statistical terms.

In Frisch's initial attempt to formalize the structural approach, e.g. see (Frisch, 1938), the correspondence issue stood out, i.e. how a modeller could get the appropriate statistical econometric model which truly corresponds to an *a priori* formulated structural model. Frisch saw the correspondence problem as arising out of limited data information. In practical terms, Frisch proposed checking the rank condition of the data matrix relating to the structural variables. However, from a modern viewpoint, Frisch's discussion of the correspondence problem muddles issues of identification with those of multicollinearity and estimability.

Identification became more clearly isolated from these other issues through discussion of the general conditions under which the structural parameters of an SEM can be uniquely determined. For example, Marschak (1942; written in 1940) distinguished two aspects of identification: one of 'variate parameters', i.e. the issue of deciding which equation in an SEM possesses the relatively stable parameters, and the other of 'lost dimensions', i.e. the condition under which those stable parameters are estimable. He also represented the condition in terms of a rank check, via a non-vanishing Jacobian determinant.[20] Haavelmo (1940, 1944) provided a more focused representation. He described identification as one of the two key issues in structural parameter estimation, namely the issue of how to establish sufficient conditions for unique estimability of structural parameters using available data (the other being the issue of how to choose the 'best' estimators). Technically, he also followed the idea of rank checking using Jacobian determinant.

The clear demonstration of the relationship between the SEM and its reduced form in (Mann and Wald, 1943) demonstrated that the classic identification problem was generic to the SEM type of models. This led to subsequent formalization work by the CC. The group separated out identification as a logically independent step lying between of model specification and estimation. They introduced two additional clarifications: the requirement for additional restrictions to allow the determination of structural parameters in the SEM and the need for data variability to guarantee that these parameters can in fact be determined from a given sample. These two aspects were represented by the now familiar rank (necessity) and order (sufficiency) conditions in terms of the Bs and Γs of (3.3) – see the paper by Koopmans, Rubin and R. Leipnik in (Koopmans, 1950).

The rank condition also helped the group to classify three types of states – unique identification, over-identification and under-identification, see the paper by Hurwicz in (Koopmans, 1950), and to the design of over-identification tests, see (Anderson and Rubin, 1950). Subsequently, Koopmans and Olav Reierøl (1950) extended the identification conditions to the errors-in-variables models. The CC research on identification provided the paradigm for later researchers on the parameter identifiability for models of other classes.

Technical clarification of the identification conditions as a separate modelling step also stimulated further conceptual explication of this step in relation to structural model formulation, specifically to the issues of exogeneity and causality. Koopmans explored the link between identifying conditions and the need for exogenous variables under the question 'when is an equation system complete for statistical purposes' (Koopmans, 1950; Chapter 17).[21] His discussion invoked the issues of how to test the validity of assumed exogenous conditioning and the limit of statistical testability of certain causal implications

[20] Both Frisch (1938) and Marschak (1942) are included in (Hendry and Morgan, 1995), cf. also (Hendry and Morgan, 1995; Introduction).

[21] The two aspects of model formulation discussed by Koopmans (1950) – causal formulation in terms of endogenous and exogenous variables and completeness – were originally distinguished by Tinbergen (1939).

of a structural SEM. It also touched on the weakness of statistical means in helping model choice.

The link between identifiability and causality was exhaustively examined by Simon (1953, 1955), who attempted to formalize the idea of causal ordering to justify asymmetric static relationships, in particular exogeneity in the context of an SEM, as distinct from causal asymmetry in time dimension (such as lagged input causing current output). This conceptual aspect of identification has aroused recurrent controversy. Wold challenged the clarity of causal formulation of a general, unidentifiable SEM, preferring to rationalize exogeneity in terms of a causal chain – see (Wold and Juréen, 1953). G.H. Orcutt (1952), and later T.-C. Liu (1960), criticised the arbitrariness in exogeneity specification, which they argued were made with the sole objective of obtaining unique estimates of *a priori* assumed structural parameters, but which might have little or no correspondence with reality.

3.3 Model specification and the probability approach

The concept of 'specification' was introduced into econometrics by Koopmans (1937). In an attempt to rigorously generalise Frisch's statistical analysis of the errors-in-variables model work, Koopmans advocated the full adoption of Ronald Aylmer Fisher's model specification procedures (see Qin, 1993; section 2.3). This entailed an explicit distinction between sample and population. Concurrently, Wold (1938) made a more ambitious argument for the adoption of sampling theory, and probability and stochastic process concept into time-series analyses, in particular in conjunction with the business cycle modelling work by Frisch and Tinbergen.

The stimulus of Keynes-Tinbergen debates led Koopmans (1941) to further clarify the logic of model specification procedures. He adapted these procedures to Frisch's structural approach by making the formulation of structural models the starting point of econometric research. He maintained that the structural model should be represented as a 'general working hypothesis' for the causal relationship in relation to economic theory. Specification should then cover all the additional information required to allow for conditional statistical inferences. He listed five specification principles for the model formulation and specification steps, principally to make the model as explicit, precise, plausible and simple as possible.

A thorough foundational work of the structural model approach was carried out by Haavelmo in his well-known manifesto for the probability approach (1944). Further than Koopmans and his mentor Frisch, Haavelmo offered a forceful justification for probability theory and stochastic specification of structural models from both the conceptual and the operational aspects. His argument for the probability approach has three connected components, which start out from the data and move to the theory. First, economic time-series data should be viewed as realisations of stochastically generated series and thus obey certain probability distributions; second, sampling theory is applicable to the analysis of such data and conducting statistical inference; third, economic theories should be formulated explicitly as stochastic, testable hypothetical models. Operationally, he demonstrated, using the case of an SEM, how important it was to have explicit stochastic specification of an *a priori* structural model to avoid making inconsistent choice of estimation methods, see also (Haavelmo, 1943). His probability approach bridged economic and econometric models by extending the Walrasian deterministic system of general equilibrium into a stochastic and dynamic system.

Haavelmo's path-breaking programme was pursued and extended by the CC. The CC's research was focused on the statistical formalisation of the modelling procedures starting from a general structural model of the SEM type. The problem of how to make selections among 'multiple hypotheses' in structural model construction was tentatively left aside, see (Marschak, 1946) and (Koopmans, 1950; pp44-5). The CC's achievement mainly amplified the operational aspect of Haavelmo's probability arguments. Indeed, his probability legacy became mostly known as Haavelmo's bias and Haavelmo's distribution. His conceptual argument for the necessity of forming structural models as stochastic and testable hypotheses was little materialised during this period.

As the formalised structural modelling approach by the CC lacked an operational procedure in posing theories as testable hypotheses and allowing for data-based selection of various tentative postulates, the approach met immediate criticism from the NBER (National Bureau of Economic Research) researchers in business cycles for disregarding the aspect of what is now knows as specification search in probability theory, see (Vining, 1949). While Koopmans (1947) made a successful counter argument of criticising the NBER data-led approach as 'measurement without theory', the problem of how applied econometric modellers should handle the lack of generally enough specified structural models remains unsolved. They were obliged to rely on *ad hoc* rules in applied modelling for years to come.[22]

[22] How to conduct specification search remains a recurring topic of methodological debates. The arbitrary practice of applied econometric model specification was referred to as 'sinning in the

3.4 Hypothesis testing

The desire to test economic theories using data evidence was a major impetus in the rise of econometrics. However, this desire remained incoherent and vaguely defined among the early practitioners, for example in Tinbergen's (1939, 1940) ambitious modelling work. The statistical concept and apparatus of hypothesis testing came into econometrics in the late 1930s with the Neyman-Pearson method of rejection, see e.g. (Brown, 1937). Although the Neyman-Pearson approach was not rejected, econometricians at the time were too preoccupied with concerns over solving various problems in corresponding economic data analysis results to available economic theories than to adopt rigorous statistical test apparatus.

The Neyman-Pearson approach inspired Haavelmo in his reformulation of Frisch's structural modelling programme within the framework of probability theory.[23] In his 1944 monograph, Haavelmo emphasized two key aspects of the hypothesis-testing scheme: one was the formulation of *a priori* theories into a set of 'admissible hypotheses', and the other was the choice of the optimal critical regions underlying statistical tests. The former aspect appears to have particularly appealed to Haavelmo. He used it as an essential argument for the justification of the probability approach, for the paramount importance of stochastic specification of economic models prior to econometric analyses, and for the importance of spelling out the identification conditions in order to make structural models estimable.

Haavelmo (1944) devoted one chapter on the Neyman-Pearson hypothesis-testing approach. However, his discussion was largely adoption rather than adaptation. Unlike with the joint distribution specification and the least-squares simultaneity bias, Haavelmo fell short of offering an operational route which would combine the Neyman-Pearson hypothesis-testing apparatus and the structural modelling approach so as to utilise the test tools to discriminate statistically between feasible and unfeasible hypotheses to assist in the model choice problem. Rather, he considered the choice of the set of admissible hypotheses 'a matter of general knowledge and intuition'.

The CC also bypassed the issue of model choice, as mentioned above. Development of testing tools enjoyed very low priority in their research agenda. Classical tests such as *t*- and *F*-tests were adopted but not adapted. Since the structural SEM was assumed correct, there was little perceived need to develop diagnostic tests. The only in-house test invention

basement' by Leamer (1978) in his advocacy for the Bayesian approach, see section 4.5.1. The issue was recently discussed again by Kennedy (2002), Hendry (2002) and Magnus (2002).

[23] Haavelmo held very high respect to Neyman, see (Bjerkholt, 2005).

that the group made were the over-identification test, see (Anderson and Rubin, 1949, 1950). It was nearly a decade later that a set of diagnostic tests was conscientiously assembled together for the purpose of misspecification analysis, see (Theil, 1957a). The importance of model testing did not become widely acknowledged until the 1970s, see section 5.3.

3.5 Summary

The Cowles Commission motto was ‘science is measurement’ – see (Christ, 1952, 1994). Their approach to research may be characterized as that of making all assumptions explicit in order to enable discovery of problems and revision of the assumptions in the light of these problems. The assumptions should be as consistent as possible with knowledge of human behaviour and may be classified into two types: the first are those assumptions which are statistically testable and the second are provisional working hypotheses. The objective was produce results ultimately useful for economic policy analyses.

Haavelmo’s pioneering work on the probability approach has become recognized as a crucial landmark in the building of econometrics. It was the first systematic attempt to bridge empirical research and theory in a logically rigorous manner. This process would facilitate cross-fertilisation between theory and data. The legacy in terms of the methodology for identifying and estimating structural models remains strong, especially in microeconometrics – see e.g. (Heckman, 2000a).

4 Emergence of standard regression-based econometrics

The two decades up to and including the second world war were a period in which, exaggerating only slightly, a thousand econometric flowers bloomed. Less metaphorically, by 1945, many of the foundations for modern econometrics had been laid. The process of methodological innovation continued through the postwar period, but, at the same time, a process of consolidation took place. The consolidation was mainly around the linear regression model which became the econometric dominant tool. The consolidation occurred though applied practice, as well as through the spread of macroeconometric model building sponsored by governments, and the growth of econometrics teaching in universities.

4.1 The Fall and Rise of Least Squares

Least squares regression had been widely used by applied researchers throughout the interwar period. This was particularly true of agricultural economists – see (Fox, 1986, 1989). It was only in the postwar period, however, that it was to become accepted as the principal tool in the economist's toolbox. Before that, however the identification and bias issues discussed by the CC lead to a decline in its acceptance.

The implication the identification problems discussed above in section 3.2 and the subsequent discovery of the Haavelmo bias was that least squares estimates would generally result in biased estimates of parameters of structural models. The discovery of the LIML estimator promised a viable alternative approach and resulted in what Epstein (1989) has called the 'fall of OLS'. The consequence was that, under the influence of the CC, least squares regression appeared a flawed technique. Reviewing (Haavlemo, 1944), Stone (1946) summarized this position in stating 'except in very special cases, unbiased estimates … cannot be obtained by considering each equation in isolation'. New estimation methods were required, and LIML was to become the most prominent of these.

It is difficult to know to what extent LIML was actually employed as distinct from being discussed but the Klein-Goldberger model (1955), an annual Keynesian model of the U.S. economy over the period 1929-52 (omitting 1942-45) was one prominent implementation. One reason for the failure of LIML to supplant OLS is that some of the early implementations failed to show much practical difference between the two estimators. Fox (1956) re-estimated the Kelin-Goldberger model by OLS and found the Haavelmo bias to be smaller than expected, while Christ (1960) used Monte Carlo experiments to compare ML and least squares in finite samples. He also found that the results differed little. These results contributed to the widespread view that the Klein-Goldberger LIML estimates of the parameters of the 14 behavioural equations in their model offered few improvements over OLS estimates – see (Epstein, 1987; pp. 118-21) for discussion.

LIML is computationally much more burdensome than OLS, and this was important at a time when computations were all undertaken on hand calculators.[24] Two stage least squares (2SLS), due independently to (Theil, 1953) and (Basmann, 1957), simplified these computations, and with electronic calculators now more widely available, facilitated comparison with OLS. This technical innovation played an important role in the return to favour of least squares. For example, 2SLS was the main estimation method in the large scale macroeconometric models, such as the Wharton model.[25] Further developments of

[24] Electronic calculators started to become available around 1945 (Goldberger, 2002).

[25] The Wharton model used quarterly time series whereas the Klein-Goldberger model had used annual data. This allowed Wharton to give greater focus to short-run forecasts. To better capture

least squares and related methods also helped, in particular SURE, 3SLS and IV. The final verdict in favour of least squares was pronounced by Waugh (1961).

Another important debate between least squares and ML is led by Wold. (Wold, 1943-44) had defended least squares from a very different angle – see also (Bentzel and Wold, 1946). He saw the SEM as a poorly specified causal model and advocated for clearly specified causal chain models. Obviously, once there is no simultaneity in the model, LS becomes legitimate, see (Qin, 1993; Chapter 6.3). Wold's argument was fully adopted later by Liu (1969) in his experimental monthly model of the US.[26]

A second reason relates to the foundations for inference in relation to estimated least squares equations. Fisher's (1925) formulation of the 'classical' errors in equations regression model and the Neyman-Pearson hypothesis testing apparatus became known to economists in the late 1930s – see above section 3.4, where we also noted Haavelmo's (1944) arguments for formal statistical testing of hypotheses. It is exactly the probabilistic formulation of the regression models that allowed the regression model to rise above the 'statistics without probability' (Morgan, 1990; pp.230-38) paradigm that had kept it at the level of one of a number of available statistical tools. Morgan attributes the formulation of this approach to (Koopmans, 1937). However, Koopmans was not able to define a clear sampling framework for economic time series. Instead, it was left to (Wold, 1938) and Haavelmo (1944) to argue that classical methods could be applied to economic time series by regarding the data set (y, X) as a single realization (sample) from the infinite number of possible realizations (the population) that 'Nature' might have chosen. This approach to regression formed the basis of what was to become the standard treatment in the 1950s and 1960s.

The probabilistic basis for the classical errors-in-equations regression model was important in resulting in this, rather than the errors-in-variables model, becoming the standard for applied research. The errors-in-variables model underlay Frisch's (1934b) confluence analysis approach – see (Hendry and Morgan, 1989). However, Frisch was not able to provide probabilistic foundations for the bunch map techniques used to implement

dynamics, it employed Almon distributed lags (Almon, 1965) in investment equations and first difference (growth rates) in some other equations, see (Bodkin *et al*, 1991).

[26] Liu had been disillusioned by the SEM approach when he tried to build a quarterly model (1960). In essence, he attempted to achieve identification through the use of higher frequency data.

confluence analysis.[27] Instead, bunch maps were visual devices for discriminating between alternative model specifications (direction of regression, variables to be included and excluded). Because specification analysis was the major gap in the CC programme, it might be thought that confluence analysis would complement regression modelling, but instead, lacking probabilistic foundations, it was unclear how it could be related to the errors in equations model, or explained to the newer generation of economists brought up to model in probabilistic terms.

A good example of this process in the evolution of the (pre-demand system) research on demand analysis carried out at the Cambridge (UK) Department of Applied Economics (DAE) over the immediate postwar decade by Stone and his co-workers - see (Gilbert, 1991). There were four important contributions: (Stone, 1945), (Prest, 1949) and (Stone, 1951, 1954). Stone (1945) was based entirely on bunch maps (in conjunction with a general theoretical structure) and reports only point estimates of coefficients 'fiducial limits' being regarded as possibly misleading). In effect, Stone used bunch maps as a form of equation diagnostic: with short samples, the number of variables suggested by theory exceeded the number of accurately estimable coefficients. An 'exploding' bunch map indicated that simplification was required.

Prest (1949) discusses bunch maps in relation to methodology but not in relation to the estimated equations, while (Stone, 1951) is entirely based on the errors in equations regression model. (Stone, 1954) contains bunch maps but does not discuss them.[28] The most important criterion for equation specification in all three of these later DAE papers is the R^2 statistic. While confluence analysis is also discussed at length, Stone (1954) provides a thorough and modern discussion of estimation and inference in the errors in equations regressions model.[29] Once 'fiducial limits' (confidence bands) had become accepted as the appropriate way to judge how well individual parameters were determined, confluence analysis became redundant. Both it and its associated bunch maps simply faded away over the first postwar decade.

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[27] Resierøl (1945) and Geary (1949) subsequently provided this framework through the development of instrumental variables, but this approach was not widely understood until the same methods were applied to the simultaneity problem – see also (Epstein, 1987; p. 165, fn. 10).

[28] The Stone (1951) estimates are the food sector subset of those subsequently reported in (Stone, 1954).

[29] A summary of this had appeared in (Prest, 1949).

Although the problems of OLS when applied to an SEM was recognised by Stone (1946) almost immediately after Haavelmo and the CC's contribution, Stone (1954) soon considered that the CC had over-emphasized the importance of simultaneity in the estimation of consumer demand equations and believed that OLS estimates were likely to be more accurate than their LIML counterparts – see (Gilbert, 1991). Residual serial correlation and the need to model lagged responses, emphasised in (Prest, 1949), were more serious practical issues.

Haavelmo's methodology was correct in principle but problems other than simultaneity were likely to be more important in practice. So whereas in 1950, the CC researchers were able to look forward to a future in which structural estimators such as LIML would provide the basis for future economic research, by 1960, when it was becoming feasible to estimate sizable models, it was less clear that LIML or other 'sophisticated' estimators offered any advantage over least squares estimators such as 2SLS – see (Epstein, 1987; pp. 118-21). Simultaneous equations bias and possible lack of identification came to be seen as just one of a number of problems that might arise in applications, and where this was a problem, 2SLS appeared to cope adequately.

4.2 Residual Serial Correlation and Equation Dynamics

We have noted that insufficient variation in short time series, manifesting itself as collinearity, was one of the major problems faced by practicing econometricians in the postwar period. The second set of problems which came to dominate research were those related to dynamic specification and residual serial correlation.

Much of this work on serial correlation took place at the DAE in Cambridge, possibly as the consequence of Yule who had argued that (in modern terminology) integrated time series data of series with either random or conjunct (correlated) differences can show spurious correlations (Yule, 1926).[30] There were two strands to the research on residual serial correlation – diagnosis and treatment. D. Champernowne (1945) had noted that the demand equations reported in (Stone, 1945) might suffer from residual serial correlation. At that time, the standard test for serial correlation was the von Neumann ratio (von Neumann, 1941, 1942), but this was appropriate for a data time series, and not a series of regression residuals. The statistic which has become known as the Durbin-Watson statistic was one of a number of diagnostics suggested by T.W. Anderson (1948) who lectured on this problem in Cambridge in 1948 (Gilbert, 1991). J. Durbin and G.S. Watson (1950) developed the theory for bounding the critical values of the statistic. Turning to treatment, D. Cochrane and Orcutt (1949) showed that, in which the errors on an equation follow a first order autoregressive process, Alec C. Aitken's (1934) generalized least squares estimator can be implemented by quasi-differencing the equation prior to estimation. This suggested the 'fairly obvious' (Corchrane and Orcutt, 1949; p.58) two stage procedure which bears their name. However, based on Monte Carlo evidence which followed (Yule, 1926) in using integrated series, they recommended differencing in preference to quasi-differencing. This was the procedure followed in the time series estimates in (Stone, 1954).[31] The emphasis on integrated series subsequently faded, possibly as the consequence of textbook treatments of what had become known as the Cochrane-Orcutt (CORC) estimator, as in (Johnston, 1963).[32]

While the British approach to dynamics focused on patching up the estimation to take into account residual correlation, US researchers tended to look for specifications for dynamic models implied by the economic theory. Koyck, who was part of the Dutch school, had utilised the distributed lag model initially introduced by Irving Fisher in the 1920s,[33] to describe the investment process (Koyck, 1954). (Klein, 1958) and (Solow, 1960) further developed distributed lag methods. The adaptive expectations formulation of the Permanent Income Hypothesis for time series data was due to M. Friedman (1956). The partial adjustment hypothesis was initially developed by M. Nerlove in relation to agricultural supply response (Nerlove, 1958). We take up these themes in section 5.2.

The (UK) London School of Economics (LSE), which had not hitherto featured in the history of econometrics, assumed an important role from the mid-nineteen fifties initially under the influence of B. Phillips. Phillips, a New Zealander with a background in sociology and engineering. His reputation was made by his hydraulic models of the circular flow of income, often known as 'Phillips machines' – see (Gilbert, 1989). He is now best known for the Phillips Curve (Phillips, 1958), a nonlinear proportional feedback

[30] Stone (1951) refers to Yule's (1926) 'picturesque examples' and Orcutt (1948) regarded non-stationarity as the norm in economics.

[31] Stone (1954) imposed income elasticities from cross-section estimates.

[32] Although Johnston does not provide an attribution.

[33] According to Alt (1942), the concept of distributed lags was first introduced in (Fisher, 1925) and used soon by G.C. Evans and C.F. Roos repeatedly. Distributed-lag equations were also used extensively in Tinbergen's macroeconometric models, e.g. (Tinbergen, 1939).

relationship from the level of unemployment to nominal wages. Phillips devised an idiosyncratic but effective estimation procedure for the nonlinear Phillips Curve which is reminiscent of *ad hoc* pre-CC statistical methods – see (Gilbert, 1976) and (Wulwick, 1987, 1989). The feedback concept was incorporated by J.D. Sargan (1964) in his model of real wage determination which follows (Phillips, 1957) in including proportional and also derivative controls, but with the proportional control relating to the real wage level.[34] This anticipated the error correction mechanism which was made explicit in (Davidson *et al.*, 1978).

Although the LSE tradition is often seen as being data-based rather than theory-driven, a number of the contributions from that group had the implication that dynamic specification should be more closely related to theory, in line with the dominant US approach. In particular, an implication of the common factor approach to equation dynamics (Sargan, 1980; Hendry and Mizon, 1978) is that the autoregressive error specification which underlies the Cochrane-Orcutt procedure entails a particular set of testable restrictions on the coefficients of a more general model. When, as will often be the case, these restrictions are rejected, the autoregressive model is misspecified and the modeller is required to reconsider the specification.

These developments in dynamic modelling all served to move econometric discussion away from the simultaneity and identification issues which had dominated the CC's discussions without explicitly detracting from the propositions that the CC had advanced. Unlike the CC concerns, they were motivated by practical concerns with modelling and policy design rather than intellectual preoccupations with how econometrics should be undertaken. Simultaneity remained a subject for discussion and research but assumed a lower priority than previously.

4.3 Consolidation and dissemination

For the pioneering generation of econometricians, econometrics was simply what they chose to do. There was no need to think of econometrics as a separate discipline so much as a way of doing applied economics. Similarly, there was no sharp demarcation between economic statistics, econometrics and mathematical economics. For their students, the structure of the discipline and its demarcation from other aspects of applied economics and statistics was defined by the courses they followed and the textbooks they used.

[34] Despite the fact that both Phillips and Sargan were members of the LSE Economics Department, there is no evidence for any direct link between the two specifications – see (Gilbert, 1989).

Demarcation between the content of different courses was reflected in the textbooks which derived from these courses. Faculty were required to cover these more specifically defined courses and these persons could allow themselves to become more specialized than the earlier generation who had developed the tools that they now taught. Whereas the development of econometrics was driven by research and policy imperatives, teaching imperatives may have been responsible for the process by which it became standardises into a coherent body of knowledge and techniques with accepted boundaries separating it from related areas of economics.

In the immediate postwar decades, econometrics was generally identified with empirical economics, with emphasis on the measurability in economic relationships. Hence Klein (1974; p.1) commences the second edition of his textbook (Klein, 1952) by 'Measurement in economics is the subject matter of this volume', and in Klein (1962; p.1) he states 'The main objective of econometrics is to give empirical content to a priori reasoning in econometrics'. This view of econometrics encompassed specification issues and issues of measurement and national income accounting as well as statistical estimation. In many universities, econometrics teaching concentrated on these broader issues well into the nineteen seventies.

This wide conception was narrowed down to the subset of 'statistical methods of econometrics', the title of (Malinvaud, 1964). Malinvaud is quite explicit about this focus. He states (1964; p. vii) 'Econometrics may be broadly interpreted to include very application of mathematics or of statistical methods to the study of economic phenomena. … we shall adopt a narrower interpretation and define the aim of econometrics to be the empirical determination of economic laws'. Johnston (1960; p. 3) provides an even clearer demarcation: 'Economic theory consists of the study of … relations which are supposed to describe the functioning of … an economic system. The task of econometric work is to estimate these relationships *statistically*, …' (italics in original).

The new generation of textbooks also had a more or less standard menu of contents and, increasingly, adopted the standard notation which endures today. After a discussion of the linear regression model and an introduction to matrix algebra, Johnston (1960) considers errors in variables, autocorrelation and simultaneous equations methods (in that order). Goldberger (1964) follows a similar order but discusses autocorrelation more briefly ion a chapter on extensions of linear regression, and errors in variables equally briefly in a chapter on regression with stochastic regressors. (Malinvaud, 1964) is the most comprehensive and longest of this generation of texts and was based on a course given from 1954 (Malinvaud, 1987). Autocorrelation is discussed in the context of time series models and the final five chapters of the book are devoted to simultaneity and identification issues.

(Johnston, 1960) and (Goldberger, 1964) share the standard $y = X\beta + \varepsilon$, or close variants of this. The similarities between the two texts arise out of Johnston's tenure of a visiting position at the University of Wisconsin, Madison, in 1958-59 at the invitation of Orcutt. Johnston delivered the graduate lectures in econometrics to a class which included Orcutt himself (Johnston, 1979). These lectures formed the basis for (Johnston, 1963). In the preface, Johnston (1963; p.ix) thanks Goldberger, also a member of the Wisconsin faculty, for the loan of the mimeographed lecture notes which would form the basis for (Goldberger, 1964) and Goldberger (1964; p.viii) himself acknowledges a partial draft of (Johnston, 1963).

The standardisation of econometric notation therefore appears to have taken place in Madison, Wisconsin. How it arrived there is more conjectural. Gilbert (1991) notes the similarities between the notation in chapter XIX of (Stone, 1954) and (Johnston, 1963) and it is possible that Orcutt was the vehicle for this transmission. Stone himself thanked Durbin and Watson for assistance with that chapter, and Durbin himself noted that 'we were probably the first people to use the $y = X\beta + \varepsilon$ notation that later became standard in econometrics' (Gilbert, 1991).

Malinvaud (1987; p.292) has remarked that the period following the 1940s was one of consolidation for econometrics, and that his textbook (1964) was 'a kind of witness to this consolidation process'. This is not to say that important innovations were absent, but only that part of the activities of what was now becoming seen as a distinct econometric profession were devoted to synthesis of the work that had gone before. Econometrics was defined more narrowly than previously on problems of estimation, inference and policy. This synthesis was built upon the foundations of probabilistic modelling advocated by Haavelmo (1944) but at the same time tended to relegate simultaneity problems to the role of advanced techniques. Linear regression had reassumed centre stage, but often with allowance for serially correlated errors.

A further important element in the dissemination of econometric practice was the development of fast computers. In the interwar period, regression was often performed by hand or by a mixture of calculating machines (used for moment calculations) and manual solution. In the early postwar years, econometricians would sometimes thank the 'computors' (the people, almost invariably young ladies, who operated the computing machines) for their assistance. Rapid progress in computer technology during the 1960s greatly facilitated macro modellers work, as appreciated by Klein (1971), 'A single development stands out as a breakthrough, namely, the development of the computer. .... We no longer shun nonlinearity, high dimensionality, covariance calculations, etc. This is all due to the development on computer technology'. (p. 134). This is despite of the fact that it was only in the later 1970s that the software became available which gave access to econometric methods to the wider economics profession.

5 Paradigm lost

While the 1960s saw consolidation of standard econometrics through textbooks, there were growing signs, in research, of deviations from, as well as dissatisfaction with, the standard approach. These occurred as researchers were trying to strengthen the bridge between theory and data following the paradigm laid by Haavelmo and the CC. The SEM framework built by the CC was intended as an econometric interface with economic theories of the most general kind (both simultaneous and dynamic). But its execution in practice was apparently hampered by paucity of both data and testable theories.

The bridge-mending job was tackled from several directions: developing more informative theories by those who held a strong faith in the theory-led approach; devising better means to process and enhance data evidence by those who gave primacy to data-based judgments; and through the better adaptation of statistical theory to economic applications by those who valued it most important to strengthen the internal consistency of the link between theory and data. During the 1970s – 1980s, these diverse research strategies came to pose an increasing challenge to SEM-based standard econometrics. This final section briefly sketches how a number of these strands originated.

5.1 Rise of Bayesian econometrics[35]

The key attraction of the Bayesian approach lies in its statistical coherence in combining *a priori* beliefs with *a posteriori* data evidence. This attraction became particularly appealing to econometricians of the 1960s for several reasons. Most important, the issue of specification choice was perceived as a serious challenge facing applied modelling. The Bayesian method appeared to offer a coherent framework in which this problem could be tackled. In particular, the Bayesian principle was felt to relate easily to

[35] This subsection draws heavily from (Qin, 1996), which contains a far more detailed description.

the problem of decision-making by applied modellers, who faced uncertainty in both theory and sample data at hand and felt obliged to compromise in combining the two in their statistical inference.[36] A second consideration related to the use of structural models for policy analysis. Under the influence of work led by the Netherlands Central Planning Bureau (CPB)[37] model-based policy analysis became a central research topic. The policy analysis framework is, in essence, a decision issue in which government chooses the policy mix to maximize an explicit welfare function. In this context, the Bayesian approach appeared more natural than classical methods since the welfare outcomes are more or less satisfactory rather than correct or incorrect. Further, the Bayesian approach to public policy appeared to closely mirror the manner in which individual agents make choices under uncertainty.

The potential usage of the Bayesian method was discussed in the econometric circle as early as the mid 1950s, see for example (Marschak, 1954). Serious adaptation of the method for econometric usage occurred in the 1960s, pioneered mainly by J. Drèze, T.J. Rothenberg and A. Zellner. Drèze (1962) attempted to reformulate the identification conditions of an SEM by the Bayesian approach to better incorporate the uncertain nature of the prior information. Zellner worked mainly on applying the Bayesian estimation methods to economic data in collaboration with statistician, G.C. Tiao, see (Tiao and Zellner 1964) and (Zellner and Tiao 1964). Rothenberg resorted to the Bayesian route to combine a classical SEM with a loss function representing policy decision so as to evaluate the effect of different priors on the posterior parameter estimates (Rothenberg, 1963).

These early Bayesian works aimed to strengthen rather than displace the CC's SEM paradigm. They resulted in a systematic reformulation of the CC structural modelling paradigm by Bayesian techniques, as embodied in the first Bayesian econometrics textbook by Zellner (1971a). The key point of contention was largely on the statistical side, specifically with regard to the use of classical versus Bayesian statistical methods. The epistemological foundations of the competing methods were also an issue – see, for example, (Zellner, 1971b) and (Rothenberg, 1971).

The Bayesian movement set out to enhance the internal consistency of the CC paradigm. However, early results show that 'for many (perhaps most) statistical problems

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[36] This problem had already been tackled by Theil (1963) using a classical approach.

[37] Much of this research is embodied in (Tinbergen, 1952, 1956), which formalized the distinction between instruments and targets, and subsequently in (Theil, 1954, 1957b, 1964), which extended Tinbergen's analysis, see also (Hughes Hallett, 1989).

which arise in practice the difference between Bayesian methods and traditional methods is too small to worry about and that when the two methods differ it is usually a result of making strongly different assumptions about the problem (Rothenberg, 1971; p195). This may be crudely parsed as 'economic specification is more important than statistical estimation', or, within a Bayesian framework, that results are highly sensitive to the choice of priors. These disappointments fermented a gradual change in direction on the part of the Bayesian camp which culminated in E.E. Leamer's influential book 'Specification Searches' (1978). The book lead Bayesian econometrics into a new phase of becoming an independent approach in econometric methodology running in opposition to the CC paradigm in which specification is taken as known – see (Pagan, 1987) and (Qin, 1996, 1998).

5.2 Rise of theory-led dynamic models

The need for better theoretical models, especially dynamic ones, was recognised by the CC researchers soon after their foundational work on econometrics, e.g. see (Koopmans, 1957). In fact, a substantial part of the CC group, including Arrow, Debreu, Hurwicz, and Koopmans himself, went into theoretical model building and lead the way to the rise of dynamic equilibrium models and growth theories. This line of research, however, contains a shift of agenda: the key task of characterising economic dynamics was recast as that of establishing conditions of stability or equilibrium of dynamic systems, see (Weintraub 1991). Theoretical analysis of the time paths of economic variables in the form of testable structural models was, for the most part, shelved for possible later attention.

Meanwhile, characterisation of these time paths was left to the *ad hoc* treatment of applied modellers. As described in section 4.3, various modellers, particularly those working on consumption and investment behaviour, managed to incorporate a dynamic behavioural component in *a priori* structural models. These dynamic, behavioural models carry empirically testable implications and may be seen as a natural extension of the SEM framework. Indeed, many of these single-equation based dynamic 'theories' were assimilated into large-scaled macroeconometric models during the 1960s, see (Bodkin *et al*, 1991). However, systematic reformation of macroeconomics into dynamically testable structural models did not occurred until almost the post 1973 oil crisis era.

The reformation is now widely known as the Rational Expectations (RE) revolution. The introduction of the RE hypothesis emerged originally from research at Carnegie-Mellon in the early 1960s. Two strands of research apparently fostered the introduction. One is Simon's (1957) theoretical research on 'rational choice' can be defined with a game theoretical model and the implications of this model for describing human behaviour. The other is the joint empirical work by Holt, Modigliani, Muth and Simon (1960) on deriving operational models for inventory control. Their approach implies dynamic regression models which may be rationalised in terms of the expectations underlying business decision making. The ideas of rational behaviour and dynamic expectations were amalgamated into the RE hypothesis proposed by Muth (1961) in the context of generalising dynamic models for price movements. In Muth's RE model, expectations are formed as unbiased predictors and interpreted as agents' 'rational expectations' of the variables in question.

It is now widely acknowledged that Lucas's (1972) rediscovery of Muth's RE model marked the beginning of the RE movement in macroeconomics, although his work is preceded by A.A. Walter's (1971) application of Muth's RE hypothesis to macro-monetary analysis.[38] Somewhat similarly to the Bayesian reformation of econometrics, the RE movement started with the intention of strengthening the CC approach by making systematic improvement on the theory side. However, this initiative rapidly resulted in two discoveries, which would severely undermine the SEM approach. The first was the well-known Lucas Critique which argued that, under the RE hypothesis, certain structural parameters are not invariant to policy shifts (Lucas, 1976). As Lucas immediately noted, this would seem to invalidate use of SEMs in policy analysis. The second was the observational equivalence of RE models with VAR models, noticed by Sargent when he tried to work out a general test base for RE models – see (Sargent, 1976). This discovery, together with a revival of Liu's (1960) concerns over identification by Sims (1980), eventually led to Sims' advocacy of the abandonment of the SEM approach and its replacement by the data-based Vector Auto-Regression (VAR) approach.

5.3 Microeconometrics

Although the distinction between microeconometrics and macroeconometrics (or time series econometrics) is relatively new, the analysis of microeconomic data, i.e. data measured at the level of the individual agent, household or firm, extends back to the earliest decades of the discipline. Data could come in the form either of cross sections or time series, but few practical distinctions were drawn between the methods appropriate to the analysis of the two data types.

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[38] There are numerous studies on the history and the methodology of the rational expectations movement, see, e.g. (Maddock, 1984), (Pesaran, 1987), (Sent, 1998), (Sheffrin, 1983), (Miller, 1994).

A number of precursors to modern microeconometrics may be identified in the first two postwar decades, although in the main these did not attract wide attention from contemporaries. According to (Cramer, 1991, p.41), the first economic application of the probit model was due to (Farrell, 1954), who was a member of Stone's DAE group. (Aitchison and Brown, 1957), also in the DAE, compared the logit and probit models. From the CC group, the Tobit ('Tobin's probit') estimator was devised by (Tobin, 1958). This was the final contribution of the CC researchers to econometric methodology, and the only application specifically to microeconomic data. It vividly illustrated the power of the Haavelmo methodology of building economic models on secure distributional foundations – see (Epstein, 1987, p.112).

Within the CC, Orcutt had despaired of resolution of the identification problem in the analysis of aggregate time series and advocated the collection of data at the individual level (Orcutt, 1952). He was ahead of his time – neither were the individual data available nor the computing power to analyze the data, had they been available. These concerns were reinforced by research on aggregation. Klein (1946a,b) and Nataf (1948, 1953) had emphasized that exact ('perfect') aggregation from individual to aggregate relations was only possible under implausibly strong assumptions. Theil (1965) reached only marginally more optimistic conclusions looking at approximate aggregation.

This research was primarily motivated by the wish to provide solid structural foundations for econometric macro models. In the 1960s and 1970s, attention shifted to the analysis of tax reform and, in particular, to the negative income tax proposal. The aggregation literature was now taken as demonstrating the importance of heterogeneity across individuals in the manner anticipated by Orcutt twenty-five years previously. This view implied that policy discussions should relate to individuals in the full diversity of their economic and social situations, instead of to supposedly representative agents. In the nineteen seventies, interest in these microeconomic issues coincided with increased availability of survey data which allowed the questions to be tackled and the availability of the computing power to facilitate the implied data-processing exercise.

The US income maintenance experiments,[39] which were motivated by interest in tax reform issues, generated some of the most extensively datasets which gave rise to novel problems, particularly those of sample selection bias, censoring and attrition bias. Research on labour supply decisions gave rise to the issue of systematically missing data – persons

[39] This literature is discussed in (Munnell, 1987).

not in employment do not report wage levels (Gronau, 1974; Lewis, 1974; Heckman, 1974). Choice over multiple, mutually exclusive, alternatives provided a focus in transportation studies – see (Domencich and McFadden, 1975).

The specification issues, particularly those of dynamics, which dominated much to the macroeconometric debate were absent from microeconometrics. Identification issues, which were present, took a different form. Above all, microeconometric research still remains grounded in the broad CC structural tradition – see (Heckman, 2000b) for an eloquent statement of this view – while macroeconometrics was moving towards a more data-based approach.

5.4 Data-based modelling again

Several factors fostered the gradual revival of the data-led modelling approach. In addition to the factors already mentioned above (the revival of least squares based regression, dissatisfaction with what were becoming seen as the arbitrary identification conditions in the SEM approach and increased popularity of the VAR approach), two further developments had been important – the development of hypothesis testing as a tool for model selection and increased attention to forecasting methodology.

Commercial and governmental demands for economic forecasts provided a major stimulus for large macroeconometric model building. There was visible optimism that forecast accuracy would improve with the increase of model size and structural complexity – see, for example, (Klein, 1971). However, this optimism was dampened by poor forecast results, including conditional forecasts/policy simulations,[40] especially by comparison with the forecasts from simple time-series models following the strategy proposed by Box and Jenkins (1970).[41] The Box-Jenkins strategy introduced a number of concepts which were either missing or neglected by the CC approach, in particular the principle of 'parsimony' in model formation, the need of rigorous diagnostic checks on model adequacy and a data-led 'identification' procedure, which essentially connotes model specification search rather than the CC sense of the terminology. The Box-Jenkins procedure offered a formal

[40] In a number of model reviews, unsatisfactory forecasts are found to be widely present with various macroeconometric models and believed to serve as a strong sign of model internal weakness, e.g. see (Evan, 1966), (Griliches, 1968), (Gordon, 1970) and (Hickman, 1972).

[41] For example, Nelson (1972) used simple ARIMA models of the Box-Jenkins type to compare with the forecasting performance of the structural model jointly developed by the Federal Reserve Board, MIT and the University of Pennsylvania. The ARIMA time-series models performed better.

methodology for tackling the model selection issue which had been set aside by the CC, but, at the same time, its 'black box' conceptualization stood in stark contrast to the CC structural approach.

Single-equation modelling was the most favoured test bed for comparing rival macro theories, although Griliches (1968) had advised the large macromodels to subject their experience to autopsy. In the most prominent debate, Friedman and Meiselman (1963), confronted monetary and fiscal approaches to macroeconomic policy - see also (McCallum, 1986). This gave renewed exposure to a number of old econometric problems: Estimated regressions gave poor forecasts; parameter estimates lacked constancy; the single explanatory variable approach proved too primitive for theory evaluation. However, in retrospect the most evident deficiency in this debate was the ad hoc nature of the procedure for handling empirically testable hypotheses, in particular when these failed to exhibit a nested structure – see also (Dhrymes *et al*, 1972). A fallout of the debate was the St. Louis model, built primarily for purposes of policy simulation at the Federal Reserve Bank of St Louis, and which ran counter to the structural tradition in adopting a reduced-form approach, based upon a small number of equations with long lag lengths in each equation - see (Andersen and Carlson 1974).

Continued cross-fertilisation from progress in statistics also played an important part in the revival of the data-led approach to modelling, in particular with regard to hypothesis testing. H. Pesaran (1974) was possibly the first econometrician to apply the Cox (1961, 1962) methodology for non-nested tests. Similarly, J.B. Ramsey's (1969) RESET model specification test became widely implemented.

One particular example of cross-fertilization from the statistics literature was the revival of frequency analysis, discarded as useless forty years previously - see section 2.4. Thanks to J.W. Tukey's extension of frequency analysis from single to multiple variable cases, the method was soon adapted into economics by Granger and Hatanaka (1964).[42] The research led Granger to become aware of the lack of an adequate statistical characterization of causality, a concern which eventually resulted in what is now known as 'Granger causality' test (1969). This test was extensively adopted by the RE movement, see also (Phillips, 1997). Not only does the Granger causality test rely on posterior data information as the criterion of causal judgement, it also abandons the possibility of contemporaneous causality.

Post-estimation diagnostic testing, which became routine as batteries of tests were implemented in standard econometric packages, tended to undermine structural models because these invariably turned out to be over simple. Models needed to be adapted, often in apparently arbitrary ways, if they were to survive confrontation with the data. Data-instigated adaptation of model specifications led to the charge of 'data-mining', articulated most clearly in (Leamer, 1978, 1983). Some of these issues were subsequently clarified through the development of cointegration analysis (Engle and Granger, 1987; Johansen, 1988) within the VAR framework since this permits interpretation of the dynamic adjustment coefficients as nuisance parameters. Others followed Leamer in concluding that poor test results often reflected more on the inadequacies of classical inferential procedures than on the economic theories being tested.

6 Final comment

At the start of the twentieth century, the term 'econometrics' did not yet exist. By the end of that century, econometrics had a clear connotation and had become a separate discipline within economics with its own journals and with dedicated faculty teaching a range of specialized courses. The process of becoming a discipline involved the formation of a standard paradigm. While not everyone agreed with the entire paradigm, dissident positions were defined with reference to the paradigm. This was a process which took place in the nineteen fifties, sixties and seventies building on the foundations laid in the interwar period and the consolidation over the wartime period when econometricians from different backgrounds were brought together as they fled the Nazis.

Many of the advances in econometrics were in statistical theory relating to the linear regression model (including multivariate versions of the model) in relation to the specific problems faced on analyzing non-experimental data, often generated with error processes correlated over time and over variables. These advances were progressive in the sense that, once discovered, this knowledge was not forgotten. After the mid-century, these advances were matched by advances in computing which, over time, eliminated a major constraint on econometric analysis.

A major theme which dominated much of the debate through the century was how, and indeed whether, econometric models can reflect theory-generated economic structures.

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[42] Morgenstern was pursuing this research on the recommendation of von Neumann. He delegated the work to Granger, see (Morgenstern, 1961) and (Granger and Morgenstern, 1963).

These debates were less progressive in the sense that old, and even apparently vanquished, themes re-emerged over time. Haavelmo's probabilistic claims, fostered by the Cowles Commission, gained the high ground at the mid-century but the assumptions required for the specific Cowles models to retain validity were questioned in subsequent decades, and, empirically, structural macroeconometric models were found to perform less well than non-structural data-based counterparts. Over the last quarter of the century, the paradigm became looser as econometricians moved to defining their positions relative to each other rather than, as earlier, relative to their non-econometric colleagues. The result was greater diversity in both theory and practice, but with a shared language and a common history. Paradoxically, the Cowles structural tradition, which had emerged at a time when the vast majority of applications were to macroeconomics, survived most effectively in microeconometrics.

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