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CONSOLIDATION OF THE HAAVELMO-COWLES COMMISSION RESEARCH PROGRAM

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Much of modern econometrics stems directly from the post-1940 works of Haavelmo and the Cowles Commission (CC) Monograph 10. This paper examines the consolidation process of the Haavelmo-CC research program mainly during the 1950–70 period from three aspects: (i) developments of econometrics textbooks, (ii) emerging themes and trends in econometric research, and (iii) the contribution of the program to empirical modeling of real-world issues. The examination reveals that the program has gained dominance primarily through its adherence to the scientific banner and style rather than its empirical relevance. The adoption of the hard science methodology is decisive in winning over the academic community; the taxonomy of econometrics into steps involving primarily specification, identification, and estimation has played a pivotal role in generating compartmentalized research topics with manageable technical challenge and also in facilitating the educational need for compiling self-contained subjects and definitely soluble questions.

Modern econometrics is commonly regarded as being laid out by Trygve Haavelmo's *The Probability Approach in Econometrics* (1944) and formalized by researchers at the Cowles Commission (henceforth, CC) during the 1940s (see Koopmans, 1950; Hood and Koopmans, 1953). The present paper examines the historical process through which the Haavelmo-CC research program became consolidated into orthodox econometrics. The examination starts from a summary of the Haavelmo-CC program (Section 1). The consolidation process is subsequently examined respectively through three areas: the development of standard econometrics textbooks (Section 2), the evolving themes and trends of research in econometric methods (Section 3) and the contribution of the Haavelmo-CC program to applied modeling (Section 4). The process went quite smoothly during the two decades 1950–1970. These two decades can therefore be viewed as a 'normal science' period by Kuhn's terminology (1962).

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1. HAAVELMO AND COWLES COMMISSION METHODOLOGY

The history of Haavelmo-CC methodology has been relatively well recorded and studied, e.g., see Christ (1952a, 1994), Epstein (1987), Hildreth (1986), Qin (1993), Gilbert and Qin (2006), and Spanos (2006). This section is merely a highly condensed summary of it.

A major impetus to the formalization of the methodology was the famous Tinbergen debate, triggered by Keynes' skeptical evaluation of Tinbergen's pioneering work '*Statistical Testing of Business-Cycle Theories*' (1939).[1] The debate highlighted the need for an academic base for the type of macroeconometric models explored by Tinbergen. In response to the need, Haavelmo and the Cowles Commission essentially formalized a new discipline, which still defines the core of present-day orthodox econometrics.

Methodologically, the Haavelmo-CC enterprise is closely associated with the 'structural' approach. The approach is genealogically attributable to R. Frisch, e.g., see Phelps Brown (1937),[2] Frisch (1938), and Bjerkholt and Qin (2010). As an assistant to Frisch, Haavelmo played a pivotal role in adapting Frisch's proposal to probability-based statistical theories around the turn of 1940. The adaption stemmed from Haavelmo's captivation by the Neyman–Pearson theory of hypothesis testing, as illustrated by his vehement argument for taking economic theories as testable statistical hypotheses (1939). However, Haavelmo's attention was soon turned to the identification problem in connection with a simultaneous-equation model (SEM) and the associated estimation problem, which led to his discovery of the 'simultaneity bias' of the ordinary least squares (OLS) estimator (1943) and his full embracement of the probability approach as the foundation of econometrics (1944).[3]

Much of the CC research program under Marschak's directorship during 1943–8 fed immediately on Haavelmo's work. In retrospect, the CC program may be summarized from three perspectives. At a broad methodological level, it attempts to bridge theory and empirical research in a logically rigorous manner. Specifically, the CC research principle is to make all assumptions explicit in order to facilitate discovery of problems and revision of the assumptions in the light of problems that might subsequently emerge. Moreover, the assumptions should be as consistent as possible with knowledge of human behavior and are classified into two types: the first are those assumptions which are statistically testable and the second are provisional working hypotheses (see Marschak, 1946). At the level of the economics discipline, demarcation is made between economists and econometricians. The job of postulating theoretic models is delegated to economists while econometricians are to specify and estimate structural models relayed to them by economists. Accepting on faith that the structural relations of those models are correct and autonomous,[4] econometricians' job is further confined to devising statistically the best estimators for the parameters of the structural models. The importance of working with *a priori* constructed structural models is derived from the belief that only such models would satisfy the need for quantitative policy analyses. This structural approach also differentiates econometrics from statistical economics (see Gilbert and Qin, 2007). At the technical level, the CC researchers formalize the econometric procedure into three steps: model specification, identification, and estimation. They choose a simultaneous-equations model (SEM) as the most general form of structural models and the starting point of the three steps. The choice is essentially based on the conceptual adequacy of SEMs in representing the Walrasian general equilibrium system.

Given a linear SEM where sets of endogenous and exogenous variables are denoted by y_t and z_t, respectively:

$$\sum_{i=0}^{p} B_i y_{t-i} = \sum_{i=0}^{p} \Gamma_i z_{t-i} + \varepsilon_t, \tag{1}$$

model specification amounts to adopting a jointly normal distribution for the error term, ε_t, following Haavelmo's arguments (1944) that all the variables, i.e., $x_t = \left(y_t, z_t\right)$, should be specified as a set of jointly distributed stochastic variables. Identification is designated to the examination of the conditions under which the structural parameters of interest, especially those in the nondiagonal matrix B_0, are uniquely estimable.[5] The issue is demonstrated via a transformation of the structural model (1) into what is now commonly called the 'reduced-form':

$$y_t = -\left(\sum_{i=1}^{p} B_0^{-1} B_i y_{t-i}\right) + \sum_{i=1}^{p} B_0^{-1} \Gamma_i z_{t-i} + B_0^{-1} \varepsilon_t = \sum_{i=1}^{p} A_i y_{t-i} + \sum_{i=1}^{p} \Pi_i z_{t-i} + u_t. \tag{2}$$

Identification requires that structural parameters, (B_i, Γ_i), should be uniquely deductible from the reduced-form parameter estimates, (A_i, Π_i), and that the data sample in use should contain adequate variability to enable the estimation of (A_i, Π_i). The role of structural estimation is to deal with the nonlinear nature of the transformation of $(B_i, \Gamma_i) \to (A_i, \Pi_i)$. The principle method adopted is maximum likelihood (ML), because the OLS is an inconsistent estimator with the specification of an SEM, as shown by Haavelmo (1943). Ideally, the full-information maximum likelihood (FIML) estimator is to be used but, given the primitive computing facility of the time, a computationally more convenient method, known as limited-information maximum likelihood (LIML) estimator, is devised (e.g., see Epstein, 1987, 1989).

The CC's technical advance initiated a new standard for research. The standard embraced not just the rigorous workmanship but also the task of measuring assumed known structural models. The CC group believed it crucial that the measurement was focused on structural models, because these models were the only type valid for simulating policy alternatives, e.g., see Marschak (1946). Reduced-form models were only useful for forecasting, a task somehow implicated as being inferior to that of policy analyses. It should be noted that the anchor of econometric measurement on structural models effectively granted the models the status of the maintained hypothesis, making them immune from hypothesis testing. Although the principle of hypothesis testing was highly commended, it became peripheral to the Haavelmo-CC enterprise. Indeed, neither Haavelmo nor the CC group endeavored much to transform the principle into operational devices for model diagnostic purposes. In the CC parlance, the issue was referred to as making 'model choice' among multiple hypotheses and was intentionally left out of their research agenda, as acknowledged by Marschak (1950, Sect. 2.6). But the CC group was not unaware that the issue would become unavoidable as soon as their formalized procedure was to put into practice (see, e.g., Koopmans, 1957). The SEM of (1) was indeed general enough to represent any economy in abstract but it was also unidentifiable by definition without further restrictions. The CC group thus saw as the next stumbling block the lack of more specifically formulated theoretical models than the general SEM and reoriented its research direction from measurement issues to theoretical model formulation around 1950 (see Christ, 1952a).

2. PROGRAM CONSOLIDATION: TEXTBOOK STANDARDIZATION

Although the CC Monograph 14 (Hood and Koopmans, 1953) was produced for the purpose of expounding their earlier technical developments made in Monograph 10 (Koopmans, 1950), their works catered better for the research community than for education. Meanwhile, the post-war first generation of econometrics textbooks were essentially distilled out of the modeling experience of practitioners and styled as workman's manuals covering a wide range of measurement issues, as can be seen from Tinbergen (1951) and Klein (1953). It took over a decade before econometrics textbooks gradually converged toward a regression-based statistics manual centred on estimation methods, i.e., a setting which has been widely recognized as standard econometrics (see also Gilbert and Qin, 2006, Sect. 4.4.3). It was actually a decade during which the CC methodology had met with doubts and criticisms from several directions (see the next section and also Qin, 1993, Ch. 6). However, none of these were discussed in the textbooks subsequently published.

In order to better present the textbook convergence process, key features of the major textbooks during the two decades of 1950–1970 are summarized in Table 1. In particular, the thematic layout of the textbooks is represented in four categories: 'applied analysis', 'statistical methods and estimators', 'SEM techniques', and 'pre-1940 techniques'. The first, i.e., 'applied analysis', covers chapters or sections which address squarely and relatively fully economic topics instead of mere and simple illustrations of econometric methods; the middle two categories are self-explanatory and the last, i.e., 'pre-1940 techniques', refers to those statistical methods which fell out of fashion during the consolidation era, such as 'bunch map analysis', principal components, and factor analysis. The categories are shown in terms of percentage of page coverage. As easily seen from the table, there was, during the 1960s, a substantial increase in the shares of the

TABLE 1. Thematic layout of major textbooks during 1950–1970

Author (year)	Number of chapters	Pages	Percentage of pages on themes of: Applied analysis	Statistical methods & estimators	SEM techniques	Pre-1940 techniques*
Tinbergen (1951)	8	258	38%	21%	1%	1%
Tintner (1952)	11	370	27%	50%	8%	41%
Klein (1953)	7	355	14%	69%	15%	0%
Valavanis (1959)	12	223	0%	85%	32%	8%
Johnston (1963)	10	300	0%	83%	21%	0%
Goldberger (1964)	7	399	6%	79%	23%	0%
Malinvaud (1966) (French ed.: 1964)	20	631	5%	76%	18%	3%
Christ (1966)	11	705	11%	48%	19%	0%
Leser (1966)	7	119	41%	46%	19%	0%
Fox (1968)	14	568	20%	60%	17%	6%
Dhrymes (1970)	12	592	18%	73%	40%	7%
Wonnacott and Wonnacott (1970)	18	480	0%	83%	22%	0%

Note: * 'Pre-1940 techniques' mean those techniques experimented on by early econometricians but became largely disused during the consolidation era, such as 'bunch map analysis', principal components, and factor analysis. The selection of a page range is based on the content of a section or a chapter; simple numerical illustrations of a technique within a section are not counted as 'Applied analysis'. There is certain overlap among the last three thematic categories.

statistical contents at the expense of applied analyses; and the increase was accompanied by a rising shares of the SEM part. Let us now take a closer look into the contents of some of these books.

The main part of the pioneering textbook by Tinbergen (1951), i.e., Part II, is a working description of how to practise econometrics by starting from available theories, and then applying suitable statistical methods to measure and test the theories so as to enable model-based policy analyses. The remaining two parts contain ample discussions on applied cases and policy analyses, such as demand for agricultural products and technical development. The relevant statistical methods are packed into the appendix, such as correlation analysis, and SEMs versus the reduced form.

Contrary to Tinbergen's practitioner style, Tinter's *Econometrics* (1952) is essentially a manual of the statistical techniques which have been tried on economic data, with emphasis on multivariate analysis and time-series topics. It is nevertheless discernible from the book that discussions of special time-series features of economic data dominate those on techniques, a characteristic which has almost totally disappeared in the present-day textbooks. Technical issues pertaining to SEMs are sketchily addressed in Chapter 7 under the title 'Stochastic Models with Errors in the Equations'. It is with the publication of Klein's *A Textbook* *of Econometrics* (1953) that the SEM techniques began to move to the central scene.[6] The move is further helped by Valavanis' textbook (1959). Focusing on ML methods, Valavanis implicitly promotes the SEM techniques to the cream of econometrics. One noticeable aspect of the SEM instruction in these textbooks is a notational simplification of the original CC models. The simplification amounts to reducing equations (1) and (2) into a pair of static models:

$$By_t = \Gamma z_t + \varepsilon_t$$
$$y_t = B^{-1}\Gamma z_t + B^{-1}\varepsilon_t = \Pi z_t + u_t. \quad \textbf{(3)}$$

Since the dynamic aspect of structural model (1) is not directly relevant to the technical gist of the identification conditions and the LIML method, the simplification helped to endorse the static SEM of (3) in neglect of the time-series features of economic data, e.g., those discussed at length in Tintner (1952). It was not until the rational expectations movement in macroeconomics and the rise of the VAR approach and the LSE dynamic specification approach in econometrics from the mid 1970s onward that the importance of dynamic modeling became gradually reinstated.

Clear signs of textbook standardization come with Johnston (1963) and Goldberger (1964). The two textbooks are arranged in quite similar structures, probably because the two authors shared their drafts and teaching experiences at the University of Wisconsin (see Gilbert and Qin, 2006). Statistical techniques and regression-based estimation methods occupy the greatest part of the two books. The SEM techniques are placed at the centre whereas those disfavored pre-1940 techniques are dropped out completely. Meanwhile, empirical cases merely function as illustrations of statistical techniques, and features particular to economic data are portrayed as 'miscellaneous' data 'problems' or 'complications' from the viewpoint of standard regression models. This toolbox style was significantly strengthened with the forthcoming English version of Malinvaud's textbook (1966). While retaining the layout of placing regression techniques in the elementary part and the SEM techniques in the final part, Malinvaud's textbook was more comprehensive in terms of coverage and more rigorous in terms of mathematical treatment. It thus helped reinforce the Haavelmo-CC SEM approach as the central edifice of econometrics.

A few of the late 1960s textbook writers endeavored to maintain the applied style led by Tinbergen (1951), e.g., see Leser (1966) and Fox (1968). For example, Leser (1966) devotes one chapter on production functions and another on demand analysis. Unfortunately, the endeavor somehow failed to compete with the growing dominance of a statistics-menu based econometrics in university curricula. The growing dominance was accompanied by lengthening of the menu. For instance, an increasing number of post-CC techniques appeared in later textbooks such as Dhrymes (1970) and Wonnacott and Wonnacott (1970), e.g., spectral analysis and various cross-section data processing methods. The style was secured and settled in the 1970s, as could be seen from Theil (1971), Koutsoyiannis (1973), and Maddala (1977). It deserves to note here that textbooks of the 1970s became branched into elementary/introductory and advanced levels to facilitate multisemester teaching. Applied topics on their own merits were marginalized in the core teaching, which became largely a statistics subject in economics departments. In some universities, applied topics were organized into optional courses. Separate textbooks on applied econometrics appeared around the turn of 1970, e.g., by Cramer (1969), Wallis (1973), and Wynn and Holden (1974).

Consequently, a simplified version of the Haavelmo-CC approach had been popularized through textbook standardization. Econometrics was taught as simply a set of universal statistical tools and useful mainly for estimating parameters of *a priori* formulated theories. The scientific rigor of such a structural approach promoted the research styles of selecting and adapting applied issues and data to fit theories, e.g., see the case of Lucas-Rapping inverse Phillips curve in Qin (2011), and of hiding data exploratory modeling experiments as ‘sinful’ activities, e.g., see Leamer (1978, Preface).

3. PROGRAM CONSOLIDATION: EMULATIVE RESEARCH

As mentioned earlier, the Haavelmo-CC enterprise was met by serious skepticism and opposition soon after the publication of the CC monographs 10 and 14. The opposition arose mainly over three issues.[7] The first and possibly the most heated one was on identification, or arbitrary imposition of identification restrictions on SEMs needed in practice. This is shown from the reservation by Orcutt (1952) in association with the classification of ‘endogenous’ versus ‘exogenous’ variables, the contest by Wold in objection to SEM as a valid structural model for lack of clearly specified causality (see Bentzel and Wold, 1946; Wold and Juréen, 1953; Wold, 1954, 1956), and the critique by Liu (1960) demonstrating the absence of correspondence of those restrictions to reality. The second was concerned with the CC choice of research focus, which was criticized for being too narrow to allow for ‘discovery’ or ‘hypothesis seeking’, as shown from the ‘measurement without theory’ dispute (see Koopmans, 1947, 1949; Vining, 1949) between the two rival groups—the CC and the National Bureau of Economic Research (NBER). The last one was related to the restoration of the least squares as a versatile and respectable estimation method in practice, demonstrated by (i) Fox’s comparison of the OLS and ML estimates of the Klein–Goldberger model (1955), (ii) Monte Carlo experiments carried out by Wagner (1958) and Christ (1960) respectively to compare the two estimation methods, and (iii) Waugh’s summary verdict in favor of the OLS method (1961).

Somehow, those disputes and disagreements were short lived. They hardly stifled the inspiration that the CC’s rigorous technical exposition excited, especially among young academics. To many of them, the CC’s monographs opened a vast territory for a new style of scientific research. As researches following the CC’s technical path accrued, the critical stage was reached in the 1960s to have econometrics established around the core task of devising estimators for given structural parameters from *a priori* postulated theoretical models. Inventions such as the two-stage least squares (2SLS) procedure made independently by Theil (1953) and Basmann (1957) and the instrumental variable (IV) estimation procedure by Sargan (1958, 1959) for SEMs were among the leading works. An arguably more innovative avenue was explored by Drèze (1962) to reformulate the CC's SEM methods by the Bayesian statistical approach. Drèze's initiative inspired a number of researchers and their endeavors during the 1960s were best represented by the first Bayesian textbook by Zellner (1971), the layout of which was essentially patterned on Johnston (1963) and Goldberger (1964).[8]

However, the area where the consolidation has experienced the most enduring success is probably microeconometrics. Pioneering instances include the Tobit estimation for models of limited dependent variables (see Tobin, 1955, 1958), and the seemingly unrelated regression (SUR) procedure for models containing a set of individual regressions with correlated cross-regression error terms (see Zellner, 1962). Both methods were devised for data features particular to microeconomic sample surveys. Since wide availability of such data and the computing capacity to handle them came gradually, technical diffusion of those methods was slow albeit steady. Most of the major advances were made well after the turn of 1970, when the Haavelmo-CC approach encountered a new wave of methodological criticisms in macroeconometrics. Remarkably, these criticisms caused little deterrent for the consolidation of the Haavelmo-CC paradigm in microeconometrics.[9] The very nature of cross-section survey data put the emphasis of empirical studies on one-off explanations, i.e., explanations of particular surveys at the individual case level without explicit predictive targets, an emphasis which stimulated directly the construction of more detailed structural models. Newly constructed structural models in turn helped consolidate the measurement role of applied researches. The combination resulted in further strengthening of the CC paradigm. One interesting example of this was a joint study by Chamberlain and Griliches on modeling the economic effect of education (1975). Technically, their econometrically 'novel' contribution was the device of an ML estimation procedure for an SEM extended with the error-in-variable component, an extension in order to represent the unobservable-variable phenomena. However, they concluded, after all the efforts spent on deriving the ML estimation procedure, that this new procedure 'did not produce results which differed greatly from those based on simpler methods' and that this 'elaborate procedure, designed to detect possible sources of bias, yielded little evidence of such bias' (p. 436). This finding was virtually a rediscovery of the OLS versus ML verdict over fifteen years earlier, as mentioned in the first paragraph of this section, and also of what was implied in Haavelmo's empirical studies nearly two decades before, as to be described in the next section.

4. PROGRAM CONSOLIDATION: THE ROLE OF APPLIED MODELING

Econometrics was largely part of applied economic research prior to its formalization through the Haavelmo-CC enterprise during the 1940s. Among the CC community of the time, only two members, Haavelmo and Klein, have ever engaged themselves in empirical studies. Noticeably, their engagement shows substantial difference in motivation; and the consolidation is reflected by Haavelmo's motivation gaining gradually an upper hand over Klein's within the academic community.[10]

Haavelmo's empirical studies were essentially motivated by the need to demonstrate the inferiority of the OLS estimation method in SEMs. Aggregated consumption was chosen for the purpose in two successive studies (Girshick and Haavelmo, 1947; Haavelmo, 1947). In the first study, the structural parameter of interest was the marginal propensity to consume with respect to income, which was estimated by two methods, OLS versus ILS (indirect least squares), the latter being conditional upon the assumption that consumption and income were mutually endogenous in a theoretical model where investment was the driving independent variable. The US national account data of the period 1922–41 were used and two sets of estimates were obtained, one from the full sample and the other the subsample of 1929–41. The full-sample OLS estimate was reported as 0.732, which fell just outside the upper limit of the reported 95% confidence interval (0.57, 0.73) of the ILS estimate 0.672, and the subsample point estimates by the two methods were shown to be much closer. What is peculiar, however, is the absence of the corresponding confidence intervals of the OLS estimates, when they are compared to the ILS estimates. To find explanations, a reestimation of Haavelmo's model is carried out and the results are given in Table 2, along with Haavelmo's original results. As seen from the table, it would have been so obvious that there lacked statistical significance of the OLS inconsistency under small and finite samples, had the missing intervals or the standard deviations of the OLS been reported.

Somehow, Haavelmo's (1947) empirical study strengthened his conviction of the inferiority of the OLS. In his joint work with Girshick (1947), simple OLS estimates were totally absent and the OLS method was dismissed from the outset for being logically inconsistent. A five-equation SEM was set up to study the demand for food; but the real purpose was to illustrate, using annual time-series data of the

TABLE 2. Estimates of the marginal propensity to consume (Haavelmo, 1947)

	Sample periods	1922–1941	1929–1941
ILS estimates via the income-investment equation (95% confidence interval)	Haavelmo's estimates	**0.672** **(0.57, 0.73)**	**0.712** (N/A)
OLS estimates of the consumption equation	Haavelmo's estimates	**0.732** (N/A)	**0.723** (N/A)
	Reestimates	0.732	0.724
(95% confidence interval)		(0.670, 0.795)	(0.656, 0.791)

Note: Haavelmo's subsample estimate seems to be using the full-sample fitted income from the first-stage LS in the second-stage LS estimation. A reestimation following that approach yields 0.714, which is close to 0.712.

TABLE 3. Structural parameter estimates of the demand for food equation in the SEM of Girshick and Haavelmo (1947)

Parameters of interest: coefficient for	Relative prices	Income
Girshick and Haavelmo's ILS estimates	**−0.246**	**0.247**
(standard deviation)	(N/A)	(N/A)
OLS reestimates	−0.346	0.286
(standard deviation)	(0.068)	(0.038)
2SLS reestimates	−0.423	0.287
(standard deviation)	(0.107)	(0.043)
LIML reestimates	−0.607	0.324
(standard deviation)	(0.238)	(0.072)

Note: These results are based on equation (4.1) in the original paper. The 2SLS reestimates are found noticeably different from the original estimates, because there are several numeric differences in the variance-covariance matrices between the calculation of the original paper and the present reestimation.

period 1922–41, how to estimate the structural coefficients consistently with the *a priori* SEM specification. The calculation job was much heavier and more complicated than that in Haavelmo (1947). Lessening of the computational burden formed an important drive for the invention of the LIML estimator (see Epstein, 1989). Sadly, the empirical significance of going through such a burden was not evaluated. Again, reestimation of two key parameters in the food demand equation of their model shows clearly that the OLS inconsistency is statistically insignificant (see Table 3).

The lack of significant improvement by those elaborate SEM-consistent estimators over the OLS in applied studies was soon exposed explicitly by Christ (1952b), as described below, and subsequently verified by others which led to the practical rehabilitation of the OLS around 1960, as mentioned earlier. Unfortunately, a general awareness of the lack was somehow absent in the academic community and the lack was to be repeatedly 'rediscovered' from applied modeling experiments for decades to come. Aside from the case of the Chamberlain-Griliches 1975 study described in the previous section, and two other cases are given in Qin (2011)—one of modeling the wage–price relation by Sargan (1964) and the other of modeling the unemployment–inflation trade-off by Lucas (1972, 1973). It was as if a conscientious choice of the academic community to ignore the lack. Certainly, the lack was hardly mentioned in those textbooks listed in Table 1.

From hindsight, it is Haavelmo's method-illustrative style which has apparently touched the right chord of the academic community. To many, applied studies have become evaluated primarily by their illustrative capacity of methods and techniques rather than empirical significance. What really counts is the scientific rigor of Haavelmo's arguments. For example, Haavelmo's (1947) paper is regarded as 'the first direct contribution to the development of exact finite sample distribution theory' (Basmann, 1974, p. 210).

Around the end of the 1960s, Haavelmo's method-illustrative style became widely adopted in econometric journal publications. The goal of dealing with real economic issues with appropriately selected statistical techniques, the original goal which had promoted the birth of econometrics in the first place, was relegated largely to applied economists. According to a survey paper by Wallis (1969), applied econometrics was defined as including primarily those 'methods and techniques which have seen successful application in a number of areas of economic investigation' (p. 771). The methods and techniques covered in Wallis's survey dealt mainly either with estimation issues associated with different model formulations, such as partial adjustment models, expectations models, or with forecasting and simulation issues of SEMs.

Noticeably, Klein's applied modeling career shows a reverse pattern. It gradually diverged from method-instigated topics toward reality-instigated topics. Klein's first applied endeavor was a 16-equation macroeconometric model of the US covering the sample period of 1921–1941 (known as Klein's Model III, see Klein, 1947, 1950). The model was inspired by both Tinbergen's macroeconometric model building works and the subsequent CC theoretical contributions. Although considerably smaller than Tinbergen's model of the US (1939),[11] Klein's Model III was the first full-fledged macroeconometric model based on the CC structural approach. It followed the SEM structural → reduced-form procedure and estimated by both the OLS and the LIML. The practical accomplishment of his endeavor was closely scrutinized and severely challenged by Christ (1952b).[12] Among other things, Christ found that the OLS estimates did not differ significantly from the complicated LIML estimates on the whole, that the model predictions using OLS estimates produced smaller errors than those using LIML estimates and that the predictions of the structural model were no better than those by naïve models of data extrapolation. Essentially, Christ's investigation revealed that the most vulnerable place in applied modeling lay in the equation and model design rather than the choice of estimation methods. This finding also implied that model design was actually where the greatest potential lay in terms of raising the signal-to-noise ratio and thus the value added of the modeling research.

Christ's investigation cast discernible impact on the subsequent Klein–Goldberger model of the US, which contained 20 equations covering the period of 1929–1952. That is most clearly seen from the discussions that Klein and Goldberger (1955) devoted to issues relating to equation and model design, for example, whether additional lag terms were desirable and what level of aggregation was appropriate. Moreover, the choices were often made from a combined consideration of both available theories and data. Such heavy use of Tinbergen's 'kitchen work' style demonstrated the severe practical limit of the CC's structural modeling approach. Nevertheless, the Klein–Goldberger model adhered to the CC methodology when it came to estimation. It was estimated by LIML only. The alternative OLS estimates were subsequently supplied by Fox (1956). The difference of those estimates from the LIML estimates was smaller than expected, illustrating once more that there was relatively little to be gained from more sophisticated estimation methods than the OLS.

The OLS received its formal rehabilitation toward the end of the 1950s, as mentioned in the previous section. Ambivalent about the rehabilitation and especially its impact on single-equation versus simultaneous-equation modeling approaches, Klein nevertheless acknowledged, 'users of econometric models are often not really interested in particular structural parameters by themselves. They are interested in the solution to the system, under alternative sets of conditions. ... It is the difference between partial and general analysis that is involved. It is conceivable that partial analysis is an end, in itself, for some problems – possibly those of a purely pedagogical nature – but most problems call for a more complete analysis of the system' (1960, p. 217).

Interestingly, the Klein–Goldberger model did not derive its empirical appeal from adherence to the CC techniques. Rather, the appeal was mainly from the dynamic properties of the model, i.e., properties which would enable quantitative studies of the time effects of different economic factors (see Goldberger, 1959). Scrutinized independently by Adelman and Adelman (1959), the capacity of the Klein–Goldberger model to generate dynamic properties interesting and desirable for business cycle studies was confirmed. Modeling dynamics became the central theme in two immediate progenies of the Klein–Goldberger model, one built by Liu (1955, 1963) and the other by Suits (1962), see also Bodkin et al. (1991, Ch. 3). Liu's modeling experience made him acutely critical of the CC's approach; Suits abandoned the conventional structural model formulation and went for a growth-rate model. Both modelers simply used OLS and made forecasting accuracy their primary criterion in model evaluation.

As his empirical experiences accrued, Klein's modeling approach also became more eclectic, synthetic, and increasingly detached from the CC's approach. Realizing that it was of primary importance to have as much data information as available, he soon ventured into models using quarterly time series. In his first quarterly model (see Barger and Klein, 1954), he experimented with Wold's recursive model, a rival model to the CC's SEM, and tried to hybridize the two in order to deal with the time-series problems newly emerged in association with quarterly data. Extension of this research resulted in a quarterly model of the UK (see Klein et al., 1961), and subsequently the massive Brookings model (see Duesenberry et al., 1965, 1969).

It was evident from both the documentation and comments on these models (see, for example, the comments by Griliches, 1968; Gordon, 1970) that a great deal of attention and debate was devoted to the choice of variables, equation forms, and specifications, as well as cross-equation linkage within a model. In comparison, execution of the CC's SEM techniques yielded marginally, since the actual modeling process was far more complicated and entangled than the CC's formalized stepwise procedure. Nevertheless, these repeatedly negative findings did not shake Klein's faith in the CC paradigm, nor that of his fellow modelers. It seemed that the strong scientific image of the paradigm mattered much more than its empirical usefulness. As observed by Bodkin et al. (1991, Ch. 4), a teamwork approach and institutionalized modeling maintenance were among the major factors driving the rapid growth of macroeconometric modeling activities of the 1960s. Both factors entailed funding, which would be impossible to secure without the backing of a strong scientific image that the Haavelmo-CC paradigm readily projected.[13] Reversely, the macro modeling activities led by Klein attracted dwindling interest from the academic research community, as shown from the citation analysis by Qin (2013, Ch. 10).

Nevertheless, applied macroeconometric modeling had approached its hey day (Klein, 1971), and econometrics had been established as a fully respectable subdiscipline within economics (see, e.g., Schachter, 1973), by the end of the 1960s. Worries of the pioneers over the status of econometrics, especially those aroused by the Tinbergen debates, were over and almost forgotten.

5. CONCLUDING REMARKS

The previous sections describe how the Haavelmo-CC paradigm was consolidated and standardized through research and higher education during a 'normal science' period of around two decades. The process was accompanied by further division of research topics, interests, objectives and especially splits of research fields between theoretical and applied econometrics within econometrics, as well as between econometrics and applied economics within economics. To a large extent, the division was necessary for maintaining the scientific rigor of research that Haavelmo and the CC group had vehemently promoted. In fact, the history reveals that it was the scientific banner and style rather than its empirical relevance that the Haavelmo-CC program has won its dominance.[14]

In retrospect, internal consistency of arguments and mathematic elegance appear to rank top of the ingredients, serving as the key positive heuristic during the consolidation process. The possibility of further division of the econometric discipline into compartmentalized research and self-contained teaching topics has also played an important role. The most vital is probably the division of research tasks between economics and econometrics. The division helps to convert the esthetics of econometric research to producing mathematically elegant measurement tools for available theoretical models. In comparison, applied relevance is secondary if not lower rated, partly because robust, precise, and conclusive empirical evidence is very hard to come by in economics. These findings provide a historical explanation of how 'the scientific illusion', as referred to by Summers (1991), has come to dominance in economics.

The concerted pursuit of the Haavelmo-CC research style encountered, however, a wave of criticisms around the turn of 1970. One of the perhaps most extraordinary criticisms came from the prominent founding father of econometrics, Frisch, who derided the econometric research of the time as 'playometrics' for its little economic relevance or interest in 'social and scientific responsibility' (1970). Frisch's criticism almost coincided with that by W. Leontief, who delivered a blunt and scathing attack on the over use of mathematics and excessive ranking of the mathematical contents in economics in his presidential address at the 83rd meeting of the American Economic Association in 1970 (see Leontief, 1971). Their criticisms were subsequently reinforced by a series of condemnation of the formalist movement in economics, e.g., see Phelps Brown (1972), Worswick (1972). More severe attacks arose in the wake of the 1973 oil-crisis triggered recession. For example, Lucas and Sargent (1979) called those macroeconometric model mis-forecasts during the post oil-crisis turbulent period an 'econometric failure on a grand scale'.

Subsequently, econometrics underwent a reformative period during the two decades from the mid 1970s onward, which was led arguably by three methodological approaches (see Pagan, 1987, 1995). The first is the Bayesian specification approach pioneered by E. Leamer, the second is the VAR (Vector AutoRegression) approach led by C. Sims, and the third is the LSE (London School of Economics) dynamic specification approach led mainly by D.F. Hendry. Although different in opinions and methods, these approaches share at least one aim in common—to shift the essential task of econometric research away from estimation of *a priori* postulated structural models to model specification analysis and diagnostic testing. Moreover, none of them actually abandons the Haavelmo-CC paradigm. The standard and the criterion of scientific rigor are kept beyond doubt and the importance of serving the need for measurement of structural models is maintained, albeit at various degrees.

Meanwhile, the central status of *a priori* postulated structural models has been strengthened by the rise of real business cycle (RBC) and computable general equilibrium (CGE) modeling approach in macroeconomics, even though the approach has drastically played down the role of parameter estimation. In microeconomics, the increasing supply of survey data has helped promote the growth of microeconometric researches. The growth is built intimately upon the Haavelmo-CC tradition, e.g., see Heckman (2000). Hence, mainstream econometrics has remained to evolve around providing statistical service for 'directly substantive' models in the sense of Cox's classification (1990), see also Phillips (1977). In comparison, applied researches of dominantly data exploratory by nature and/or mixed with nonstatistical based information analyses remain to be regarded academically second-rate or heterodox. The endurance of the Haavelmo-CC paradigm may be the best memento for Haavelmo's centenary.[15]

NOTES

1. This debate is well documented in Hendry and Morgan (1995, part VI and also the Introduction).
2. Phelps Brown (1937, pp. 356-6) reports a summary of 'an ideal program for macrodynamic studies' presented by Frisch at the 1936 Oxford Meeting of the Econometric Society.
3. The initial version of this work was released in 1941; see Bjerkholt (2007) for a detailed historical account.
4. For the concept of autonomy, see Frisch (1938), Haavelmo (1944, Sect. 8), and also Aldrich (1989), Qin (2014).

5. Note that 'identification' carried far wider connotation prior to this formalization, e.g., Hendry and Morgan (1989) and Qin (1989).

6. Notice that the Cowles Monograph 14, which aimed at popularizing the SEM techniques, was only published in 1953. However, Klein had first-hand knowledge of the CC's works since he joined the group in 1944. On the other hand, Tintner was not involved.

7. For more description of these debates, see Qin (1993, Ch. 6) and Gilbert and Qin (2006).

8. See Qin (1996) for a more detailed historical account of the rise of Bayesian econometrics.

9. The enduring effect of the consolidation in microeconometrics is discernible from Heckman's retrospection of the CC tradition (2000). More evidence is presented through citation analysis in Qin (2013, Ch. 10).

10. For more evidence from Citation analysis on this point, see Qin (2013, Ch. 10).

11. Tinbergen's model contained 48 equations with 52 time series of the period 1919–1932.

12. Christ's test of Klein's model was descendant from Marshall's test (1950) and jointly discussed by M. Friedman, Klein, G.H. Moore, and Tinbergen, as appended to Christ (1952b).

13. Evidence provided by Armstrong (1978, 1979) suggests that econometricians carry out research more on belief than on fact, that the majority believe in the superiority of method complexity, and that advocacy and group conformity tend to dominate objectivity in their research strategy.

14. This can also be seen as a part of what McCloskey (1983) describes as the pervasive movement of 'modernism'.

15. A much more detailed discussion and analysis of the reformative period of econometrics and the impact of the Haavelmo-CC paradigm is given in Qin (2013).

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