Econometric Methodology at the Cowles Commission: Rise and Maturity

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Considering the contribution of the Cowles research institute to the development of econometrics, one has little choice but to focus on one major achievement, the building of the simultaneous-equation methodology. It is not necessary to demonstrate the indisputable fact that this methodology was conceived and elaborated at the Cowles Commission in the forties. Neither does one need to insist on the long-standing significance of this achievement nor on its central place in any education or reflection concerning statistical inference about economic phenomena. More interesting is the question of how research at Cowles during the first 15 years of its existence led to this result and how further econometric research here during the last 30 years relates to the simultaneous-equation achievement. Also relevant are the following questions: How does the message that was sent out by the Cowles people to the world in 1950 stand today? Should it be replaced by another different one? Or should it simply be somewhat amended and supplemented?

To do full justice to the research work and achievements of the many econometricians who were associated with Cowles through the years would require a much longer paper than the present one. But one must at least also try to summarize here those main concerns that stood outside the simultaneous-equation methodology.

A Historical Perspective

A good historical account of the present subject is not so easy to elaborate because, at least during the first two decades, neither the priority given to econometrics in the Cowles research program, nor the specificity of the Cowles Commission with respect to its immediate scientific environment were very clear. Before anything else these two features require consideration.

Theory of Measurement

For 20 years, the motto of the Cowles Commission, printed on its monographs and reports, was Lord Kelvin's sentence, "Science is measurement." This might suggest that the main emphasis was then given to econometrics and that, from the beginning, research was devoted to the subject that matured in the forties.

The facts are not so simple. In the first place, during the first years a good deal of attention was devoted to direct measurement, as distinct from inference based on available statistical measures. Cowles Commission Monograph No. 3, written by A. Cowles and associates, first published in 1938, was the outcome of the first research project selected in 1932 and was entirely devoted to working out monthly indexes of stock prices and annual indexes of stock yields. During the six Cowles Commission summer conferences held from 1935 to 1940, which then played a great role in the Commission's activities, the progress of official statistics was occasionally discussed, in particular in papers that dealt with such subjects as population censuses.

During those first years, the research program at Cowles may be characterized as a combination of direct measurement, econometrics, and nonformalized study of economic evolution. Particularly typical of the last component was Cowles Commission Monograph No. 4, *Silver Money*, written by D. H. Leavens and published in 1939.

What about pure economic theory, which later took the predominant place in the Cowles Foundation activities? It was certainly never considered outside the domain of interest. Indeed, the original articles of the Commission, formally chartered in 1932, contain these words: "The particular purpose and business for which said corporation is formed is to educate and benefit its members and mankind, and to advance the scientific study and development ... of economic theory in its relation to mathematics and statistics." Moreover, economic theory was often discussed in the summer conferences. But it was not the direct subject of substantial research before September 1939 when O. Lange and I. Mosak simultaneously joined the staff Their Cowles Commission Monographs, respectively Nos. 8 and 7, both published in 1944, *Price Flexibility and Employment*, and *General-Equilibrium Theory in International Trade*, were the first ones to deal with formalized economic theory.

The next step occurred in 1942–1943 when L. Hurwicz, I. Marschak, and T. Haavelmo successively joined the Commission and when the series of the Cowles Commission Papers was started, to be later called the Cowles Foundation Paper Series. Glancing through the titles of the papers contained in this series, one may note that a sharp turn took place in 1950; indeed, economic theory was the subject of about one-third of the papers up to 1950, but of roughly two-thirds in the following years. By 1952 the role of theory had become so important that the motto "Science is measurement" no longer seemed to be appropriate. When C. Hildreth proposed to change it to "Theory and measurement," the suggestion was immediately accepted.[1] I do not know exactly when and why the motto was dropped; but I note that it still appears on the report for the two years 1952–54 but no longer on the 1954–56 Report of Research Activity.

Surprisingly this change, whose significance should of course not be overemphasized, occurred when a reorientation was felt to be advisable. The same 1954–1956 report opens with an introduction signed by lames Tobin who was appointed Director in July 1955 when the Commission moved to Yale and became the Cowles Foundation. It contains the following words: "The development of tools is a natural emphasis for a small community of university teachers and scholars.... In the last decade, the [Cowles Commission's] greatest contributions were in the development and refinement of tools of analysis, both theoretical concepts and models, and statistical methods. ...Tools for an empirical science cannot, however, be built in a vacuum. Methods of theoretical analysis and techniques of statistical inference must be relevant to the substantive problems of the science.... For these reasons, the Cowles Foundation seeks to maintain a balanced research program in which tools of wide applicability are developed in response to the needs of substantive research in economics and are tested on accessible empirical observations." Indeed, the brief account of past activities that is printed in almost exactly the same words at the

[1] This piece of information, as well as many others used in this text, is taken from *Economic Theory and Measurement: A Twenty Year Research Report, 1932–1952*. Cowles Commission for Research in Economics, University of Chicago, 1952.

beginning of each report since the one for 1967–70 states: "The move to Yale in 1955 coincided with a renewed emphasis on empirical applications in a variety of fields."

Looking at the Cowles Foundation Papers published since then, I estimate that roughly one-fourth of them were devoted to such applications, the proportion being definitely higher during the late fifties. From the point of view of this paper, the important observation is, however, that research in econometric methodology did not regain, for three decades, the predominance it had had in the forties: only some 35 Cowles Foundation Papers out of more than 460 published between 1956 and 1982 clearly concerned this subject.[2]

The Early Econometricians

When discussing the work at the Cowles Commission during its first twenty years, one should not isolate it from its immediate scientific environment, which the rising econometric movement provided.

The association between the Econometric Society and the Cowles Commission was indeed very strong. The first serious move of Alfred Cowles, when he decided to sponsor research, had been to write to the President of the newly born Society, Irving Fisher, and to offer him a research institute working in the discipline that the young Society was trying to promote. Whether to accept this offer had been discussed by the Council of the Society and accepted only after serious consideration, the Commission being then pretty much considered as a de facto research organization of the Society. Alfred Cowles provided the finance for the launching of *Econometrica*, whose first issue appeared in January 1933. Upon its incorporation, the Cowles Commission became host of the archives of the Society. Charles Roos, the first Secretary of the Society, was appointed director of research of the Commission in September 1934. When he left in 1937, Alfred Cowles became Secretary of the Society. When in September 1939, the Commission moved from Colorado Springs to Chicago, it was only natural that the Society's offices moved along. This strong association explains why it was the habit for the yearly research reports of the late thirties and early forties to contain a section on the activities of the Econometric Society.

What did this association mean for the scientific work of the Commission? The early econometricians made a very lively community within which many new ideas and new research methods were tried and discussed. It was not a "school" but something like a "movement," "finding its bond of unity in the common skills and methods of its adherents rather than in any uniformity of theoretical position," as Kenneth Boulding wrote in his review of *A Twenty Year Research Report*. One may infer that association with the Econometric Society gave to the Commission a

[2] A simple Count of the number of Cowles Commission or Foundation Papers is admittedly not a very reliable measure of the number of publications that directly resulted from research done at Cowles, not to speak of a measure of the significance of this research. Publications sometimes occurred several years after the author had left Cowles and the decision to include reprints of the article in the series of papers implied submission, or at least approval, by the author and judgment on the part of the Foundation staff. But if, for this reason, the measure is biased against papers on econometric methodology, that fact is also revealing. Since 1982 the situation has definitely changed: during the five following years, the proportion of Cowles Foundation papers dealing primarily with econometric methodology exceeded 30%.

strong stimulus, both by exposing it to a wide range of ideas and by confronting it with the high standards of rigor that were then maturing. But it did not constrain the research program that the Commission could select; indeed, one can see for instance that the table of contents of *Econometrica* covered a very wide range during these early years.

Of more direct influence were the summer conferences that were organized by the Commission every year from 1935 to 1940. They gathered a small number of people within a simple but friendly setting for about a month. Besides contributing to a better personal acquaintance among the participants, they were devoted to informal discussions on work in progress. A report was later published, with something like a two-page summary for each paper. Hardly fewer than 200 lectures were given by 102 lecturers during the six conferences. Their subjects ranged from pure statistics to economic theory. Their speakers were sometimes such prominent figures as Ronald Fisher, Corrado Gini, and Irving Fisher, but more often members of the young econometric movement — R. Allen, R. Frisch, T. Haavelmo, J. Marschak, R. Roy, H. Schultz, A. Wald, to name only a few among those who gave two or more lectures and were not then members of the Cowles Commission.

These conferences helped considerably to establish the Commission as a lively center of good research. They were probably in large part responsible for the excellent recruitments that were made in the late thirties and early forties and that, of course, explain the exceptional quality of the research work during the second decade. The impact of the conferences on the early research program is not clear since the two closely connected Cowles Commission Monographs that were then in preparation — No. 5, *The Variate Difference Method*, by G. Tintner, and No. 6, *The Analysis of Economic Time Series*, by H. Davis, to be published in 1940 and 1941 — can as well be considered as resulting from the emphasis placed since the beginning on the statistical analysis of business cycles.

Moreover, there was apparently some hesitation around 1940 about the choice of the program for the subsequent period. The report for 1940 begins with the following sentence: “Among economic problems none is more important than unemployment of labor and other resources.” It later states: “Unless there are compelling reasons for a change of plans, the long-run program of the Commission will be directed to a study of the problems centering in the flow of investment and the incomplete use of resources.” But in early 1942, the Cowles Commission turned to the study of wartime price control and rationing, for which it received a special grant. These two successive choices did not lead to very significant scientific output, although Monograph No. 9, *Price Control and Business*, by G. Katona, published in 1945 and based on field studies among producers and distributors of consumer goods, initiated a method of applied economic research that is definitely useful. In January 1943, when he became director of research, Jacob Marschak immediately thought it necessary to reorient the research program and to give it the content to which I shall now turn for a deeper examination.

Inference for Dynamic Simultaneous-Equation Systems

The Cowles Commission contributed to the rise of econometric methodology in two determinate ways. On the one hand, it imposed “the probability approach”: each application should begin with the definition of a precise stochastic model representing the phenomenon under study and the

generation of the data; the method to be used for inference should then be rigorously determined within the framework of this model. On the other hand, it showed why most models to be built by economists should appear as systems of equations disturbed by additive random terms and often containing the values taken by the same variables in a few successive observations; it then fully determined methods to be recommended for estimation or testing within such models.

We must reflect today on the nature of this double essential contribution, evaluate its role, and critically review its suitability for econometrics. I consider that this is much more easily done for the simultaneous-equation methodology than for the probability approach, and I shall take up first the easy part of my assignment, again following historical lines.

Antecedents

Present econometricians might think that the work of Jan Tinbergen for the League of Nations in 1939 was the main source of inspiration at the Cowles Commission when simultaneous-equation systems were identified and studied as being appropriate for econometric applications. Looking back at the available evidence, one must conclude that it was only one source of inspiration among several and in no way a preponderant one.

It is true that the first sentence written in 1950 by Lawrence Klein in his Monograph No. II dealing with the macroeconometric modeling of the U.S. economy reads as follows: “Tinbergen did a great service to the study of economics when he prepared his volumes on the statistical testing and measurement of business-cycle theories.” But the book contains just one other reference to Tinbergen, and on what may appear to be a minor point, namely, the argument in support of the use of *linear* systems. One is surprised to note that the fundamental Monograph No. 10 also contains just one reference to Tinbergen’s work, although at the beginning of the main paper by T. C. Koopmans, H. Rubin, and R. B. Leipnik, but not in the general presentation written by Jacob Marschak. Still more revealing is the fact that no mention of Tinbergen is made in the l1-page, heavily documented presentation of the Cowles Commission research on simultaneous-equation econometrics in *A Twenty Year Research Report*.

It appears in this presentation and other texts that the inspiration often came from problems raised by the estimation of demand and supply elasticities, or even from the fitting of production functions. General models of the whole economy were probably not so often discussed. Looking at the reports of the Cowles Commission summer conferences, in which Tinbergen never participated, one finds that only two lecturers approached the issue of econometric models for the whole economy. F. Dresch was involved in building a general-equilibrium model initially intended for discussions of the impact of various taxation devices. This model was first presented in the 1937 conference by G. E. Evans in a series of three lectures, but with no clear expression of intention for econometric fitting. F. Dresch was present at the 1939 and 1940 conferences and gave lectures on the progress of his work; a good deal of consideration was then given to the fit of the model to an actual economy; in 1940, F. Dresch reported in particular on the difficulties he was facing in obtaining a complete set of time series for the variables of his 15-equation model. One important piece for understanding the intellectual environment at the time is a never-published memorandum prepared by R. Frisch for a Business Cycle Conference held in July 1938 at

Cambridge, England, to discuss J. Tinbergen's work.[3] The memorandum takes a quite critical position, strongly questioning the meaning of Tinbergen's equations. Without really proving his point, Frisch claims that these equations are different from the truly autonomous ones, the knowledge of which matters for testing economic theories; the main point is that Tinbergen's method may lead him to find any of the many equations that can be derived from the underlying system of autonomous structural equations. The memorandum is also interesting because, within the theory of systems of difference equations, it discusses the concepts of autonomy and identifiability of an equation (called "irreducibility").

The same questions were discussed in the same fundamental framework at the 1940 Cowles Commission summer conference by T. Haavelmo in a paper entitled "The Problem of Testing Economic Theories by Means of Passive Observations." There he discussed the problem of identifying parameters, as well as the problem of solving a set of structural equations, in a non-stochastic general simultaneous-equation setting.

If the Cowles Commission people referred so little to Tinbergen's study, or for that matter to Dresch's model and to Frisch's or Haavelmo's conference papers, it is probably because they were not really concerned with advocating the use of simultaneous-equation models; they took it for granted that such models often appear in economics. Their concern was to solve the statistical problems raised by inference in these models.

This concern was natural considering the statistical orientation of the research work at the Commission in the late thirties and considering the discussions that were going on at the time, notably during the summer conferences, about the difficulties of demand and supply econometric analysis. One may be sure that the same concern for finding good inference methods was also often present at the "small econometrics seminar that met regularly in New York on weekends during 1940–1942," as it was described in *A Twenty Year Research Report*, with T. Haavelmo, T. C. Koopmans, J. Marschak, and A. Wald, the first three of whom would join the Commission in 1943 or 1944.

Also revealing is the fact that, when presenting in 1950 the development of their research, Cowles Commission people often referred to two articles as having provided its initial impetus. In Monograph No. 10, J. Marschak wrote: "A new milestone was reached in 1943 when two articles were published in *Econometrica* by Haavelmo and by Mann and Wald"; T. C. Koopmans, H. Rubin, and R. B. Leipnik similarly comment on these two articles at the beginning of their long contribution. In his 1943 article, T. Haavelmo discusses the statistical estimation of a three-equation model of national income determination, shows how to calculate the maximum likelihood estimate of the consumption equation, exhibits the difference between this estimate and the least-squares regression, and finally explains the origin of this difference. On their part, H. B. Mann and A. Wald mainly study the large-sample properties of the least-squares regressions applied to a linear system of difference equations in reduced form, with no exogenous variable and no

[3] The text of the paper is available in mimeographed form together with the reply of J. Tinbergen and papers by T. Haavelmo and T.C. Koopmans; these are also available in print, within a Memorandum of the Oslo Institute of Economics, dated November 6, 1948, and entitled "Autonomy of Economic Relations." I received the text of the paper, thanks to the assistance of T. C. Koopmans and P. Meinich, University of Oslo.

overidentification restriction. The simultaneous-equation aspect is really discussed only at the end of the paper. The Cowles Commission people were very impressed by this work because it provided a rigorous theory for the treatment of autoregressive phenomena, which were recognized as playing an important role in economic time series.

The Construction Period

The major work for the building of simultaneous-equation econometrics at Cowles took place during the five and one-half years that roughly coincided with Jacob Marschak's term as director of research (January 1943 to June 1948). In the report for 1943, it appears that a good half of the research done during the year belonged to the simultaneous-equation econometrics field, although still with a definite orientation toward applied questions (demand functions, production functions, multiplier models). The presentation of this work already stresses the general methodological issues that were becoming the object of major concern. Not much later, the Cowles Commission called what turned out to be the most influential conference on statistical inference in economics ever held. It took place in Chicago from January 27 to February 1, 1945, and was attended by R. L. Anderson, T. Haavelmo, H. Hotelling, L. Hurwicz, L. R. Klein, T. C. Koopmans, R. Leipnik, H. B. Mann, J. Marschak, H. Rubin, G. Tintner, and A. Wald. The Commission research staff had prepared a number of papers dealing with various subjects, concerning in particular time-series analysis. The most novel contribution was certainly the one presented by Tjalling Koopmans and his research assistant Herman Rubin; it discussed both identification and maximum likelihood estimation in simultaneous-equation systems, proving the main theoretical results on both questions.

The report of the conference was prepared, with contributions made by other participants and with additions of subsequent work done at Cowles. These additions concerned mainly some theoretical developments; computational questions, which at this precomputer time might have been a real obstacle; and the "limited information" method of estimation, which was conceived in early 1946 by T. W. Anderson and H. Rubin and subsequently fully worked out by them. This report was to become Cowles Commission Monograph No. 10, *Statistical Inference in Dynamic Economic Models*, edited by T. C. Koopmans. We know that the manuscript was completed in early 1947, but publication was delayed until 1950 by typographical and other printing difficulties.

During those years and up to the early fifties, a number of important journal articles were also published by Cowles research people on the fundamental theory, on its relationship to neighboring fields in mathematical statistics, on methods of computation, and on economic applications. Since this material was either dispersed among a number of journals or very difficult reading, the Cowles Commission thought it was its duty to prepare another monograph that would expose the methods and their main properties in less technical terms. This was the object of Monograph No. 14, *Studies in Econometric Method*, edited by W.C. Hood and T.C. Koopmans and published by J. Wiley in 1953. The book also contained some new material "representing the fruits of continuing research." It is, however, fair to write that by 1950 econometric methodology had become a subject of almost secondary concern at the Cowles Commission. Exciting theoretical developments in mathematical economics were then going on and attracting much more attention.

Any Follow-Up?

Indeed, it might seem strange that, from that time on, the Cowles Commission took such a detached stand about the future evolution and progress of what had been the first major scientific achievement of the organization. Neither questioning by H. Wold with his "causal chain approach," nor appearance of the two-stage least-squares estimator, nor Monte Carlo generation of the first small-sample results gave rise to any reaction from the Cowles Commission. Among those who had been directly associated with the simultaneous-equation work of the 1940s, only L. Klein and T.W. Anderson showed publicly some interest in the subsequent development of the subject, long after they had left Cowles — L. Klein in connection with the problems raised by statistical treatment of macroeconometric models, T.W. Anderson by his theoretical work on multivariate analysis and on small-sample distributions. But after the lengthy presentation of research on econometric methodology in *A Twenty Year Report*, published in 1952, the research report for 1952-54 presents Monographs 10 and 14, but contains less than a page on current econometric research.

Surprising as it is, this detachment was wise. We know many examples of human groups that lived too long on their previous achievements. On the contrary, Cowles people had much to do with deep research into other subjects and did not slow down their march forward by holding the ground they had discovered.

Much later the subject was taken up again in various ways at the Cowles Foundation, as we shall see in the next part of this paper. But it may still be guessed at this stage that, in the last years of the Cowles Commission at Chicago, some were probably not quite happy with the detachment from simultaneous-equation methodology. Indeed, one of the first new projects to be launched at Yale in 1955 was a large-scale numerical experimentation into the small-sample properties of simultaneous-equation estimators. This was assigned to Robert Summers and described as follows in the research report for 1954–56: "Summers is investigating the distributions of small-sample estimates given by a number of alternative procedures: single-equation least squares; full information maximum likelihood; full information diagonal matrix maximum likelihood; limited information maximum likelihood; reduced least squares — Theil's method. His investigation concerns (1) the bias and efficiency of the various estimates, (2) their differences in computation costs, (3) their sensitivity to failure of the assumed specifications of the statistical model." As is well recognized, the result of this major work still is today one of our sources of knowledge about the small-sample properties of the various estimates.

Looking Back

When we look back and try to give a broad evaluation of the achievement of the simultaneous-equation work of the 1940s, we of course know that the theory was not complete by the end of this period. Alternative estimators had to be discovered, small-sample properties to be investigated, nonlinear simultaneous-equation models to be considered, efficient computational softwares to be built, even pedagogical presentations of the theory and of its algebra to be found. Nevertheless, after thirty more years of theoretical research in this field, the Cowles Commission construction essentially stands untouched; new wings and pinions have been added, good maps have been

drawn, but the central building needs no repair. This was a perfectly sound and impressive piece of methodological work. No doubt or questioning can be expressed in this respect.

The importance of the achievement for econometrics is a bit more delicate to discuss. Skeptics may point to the fact that, notwithstanding a tremendous increase in our computing facilities, simultaneous-equation estimators are not so often used in applied work and that, when they are compared to more traditional estimators, they most often turn out to give similar results. Some may indeed think that Cowles Commission people overemphasized the role of simultaneous-equation models for inference on economic phenomena. When such a view is expressed, one does not know whether and how one should dispute it, because neither the two fundamental Monographs 10 and 14, nor the expressions of skepticism about their importance provide, on this issue of relevance, any quantitative or falsifiable statement that could be proved to be wrong.

It can be said, however, that no one who wants to be initiated into the art of inference on economic phenomena today can neglect to visit the simultaneous-equation castle. One must go on top of the keep and look from there at the surrounding landscape. Those who do not will be definitely handicapped and clumsy when making assessments about exogeneity, about identifiability, or about estimation bias resulting from interdependence between phenomena.

Indeed, the simultaneous-equation tower is so visible in the field of econometric methodology and its architect is so well known that other works done by Cowles people in the same field suffer from the comparison. Many contributions were brought in particular to the statistical analysis of economic time series; but no part of this other, still more bulky and elaborate construction is commonly said to be the work of the Cowles Commission or Foundation.

Thirty Years of Research since Then

From the late forties to the end of the seventies, econometric methodology was no longer the main field of research at Cowles. The feeling may even have sometimes prevailed among those who were not closely connected with the Cowles Foundation that it no longer brought interest to this subject. This was, of course, wrong as testified by the series of *Reports of Research Activities*, published every two or three years.

Indeed, the research staff always contained a few members who were working mainly in the econometric area. The Cowles Foundation provided them with a favorable environment, and each one in his own way contributed to the methodological developments that were taking place in the wider community of econometricians. But readers of journals seldom pay great attention to the exact place where a research has matured and the role of the Cowles Foundation often passed unnoticed.

It would not be interesting to survey here completely this econometric research, which was of course multifarious, complementary with research done elsewhere, and often dependent on the particular interest of those who had been attracted to Cowles. I shall therefore be selective and deal with three topics only, which for one reason or another seem to be worthy of special attention here: initiation around 1955 of the econometrics of models with qualitative or limited endogenous

variables, contributions to time-series econometrics from 1965 to 1975, and a recent revival of interest in simultaneous-equation econometrics.

Tobit Estimators

"As the attention of empirical research in economics turns increasingly to cross-section data, there will be need for development of new statistical tools appropriate for economic survey statistics. The next ten years may witness a methodological development in this area comparable to the developments of the past decade in the analysis of economic time series. The techniques of multiple regression and the analysis of variance, which have been so popular and so useful in much econometric work both on survey data and on time series, are powerful tools and allow for the effects of many variables. However, surveys frequently confront us with variables to which regression and analysis of variance models obviously fail to apply."

This paragraph appears in the *Report of Research Activities, July 1954 to June 1956*. As we shall see, it was certainly written by James Tobin, who was appointed Director of the Cowles Foundation when it was established at Yale in July 1955. It reveals, already at that date, an acute and shrewd vision of what ought to have been the next major breakthrough in econometric methodology. But if the premonition was essentially correct, it was not perfectly dated. The "methodological development" that was anticipated would only occur in the seventies and eighties, econometricians then devoting a great deal of their efforts to multidimensional probit analysis, log-linear models, models with limited dependent variables, or with latent variables, and the like.

As he was working on consumer finances and on consumer spending for durables, Tobin was aware of the fact that applied econometricians would often have to consider endogenous qualitative variables or nonnegativity constraints applying to endogenous variables. In fact, Cowles Foundation Discussion Paper No. 1, written by Tobin, explains the applicability of multivariate probit analysis to economic survey data.

His main methodological achievement was, however, not in probit analysis, but in the precise discussion and estimation of censored linear models, i.e., models in which the endogenous variable y may be viewed as determined from an unobservable latent variable y^* to which it is equal if $y^* \geq 0$, but with $y = 0$ if $y^* < 0$, with the variable y^* being generated by a standard linear regression of some observable exogenous variables. These models were later to be called "Tobit models." An iterative method for computing an appropriate estimator of this model was worked out and programmed with the help of R. N. Rosett. The research was reported in *Econometrica* in 1958.[4]

For many years it remained an isolated contribution of Cowles to the methodological development that had been anticipated by the 1954–56 report, although cross-section econometrics remained a special concern and application to cross-section data of some of the research done during the following years was mentioned in the subsequent reports. The *Report of Research Activities, July 1970 to June 1973*, stated that Jon K. Peck had several research interests in the area discussed by Tobin, as well as in many others; but no written contribution belonging to this field was quoted either in this report or in the following one. It was only in 1977 that Ray Fair published a short

[4] For the definition of the estimation and a survey of the important recent literature on the same subject, see T. Amemiya, "Tobit Models: A Survey," *Journal of Econometrics*, January–February 1984.

methodological article on the subject, which was occasioned by his research on some determinants of extramarital affairs.

Time-Series Econometrics

On the contrary, the statistical analysis of time series remained an almost permanent subject of research concern at Cowles. The two monographs written by Davis and Tintner testify for the prewar period. The work on simultaneous equations during the forties concerned time series and involved dynamic specifications with lagged dependent variables. Major research activities took place again in the late sixties and early seventies.

Already in the early sixties some of the work had been directly motivated by the needs of time-series analysis, in particular, multivariate forecasting or seasonal adjustment. (See the contributions of Hooper and Zellner and of Lovell.) But it was only around 1966, when Marc Nerlove joined the research staff that work on time-series econometrics strongly developed again.

Several methodological questions were then attracting interest in econometrics: how to estimate efficiently both the coefficients of the model and the serial correlation of the errors; how to deal with this problem in specifications involving long distributed lags whose coefficients cannot be precisely estimated without some prior information such as smoothness of the time profile of these coefficients; what should be the use of spectral methods in applied econometrics and in theoretical econometrics; how to analyze panel data, which are made up of time series of observations for a cross section of individuals, households, or firms.

It would of course take too much space to describe the various research contributions that were made at Cowles on these questions during the period 1966-1974. A good sample of them is provided by the eleven articles that were included in the series of Cowles Foundation Papers and dealt with these subjects.[5] Suffice it to say that during those years, research in econometric methodology appeared again important, was successful, and attracted (at Cowles for short periods) some of the most gifted econometricians.

[5] D. Couts, D. Grether, and M. Nerlove, "Forecasting Nonstationary Economic Time Series," *Management Science*, No. 1, 1966. Balestra and M. Nerlove, "Pooling Cross Section and Time-Series Data in the Estimation of a Dynamic Model: The Demand for Natural Gas," *Econometrica*, No. 2, 1966. "Experimental Evidence on the Estimation of Dynamic Economic Relations from a Time Series of Cross Sections," *The Economic Studies Quarterly*, No. 3, 1967. M. Nerlove, "Distributed Lags and Unobserved Components in Economic Time Series," *Ten Studies in the Tradition of Irving Fisher*, John Wiley, New York, 1967. K.F. Wallis, "Lagged Dependent Variables and Serially Correlated Errors: A Reappraisal of Three-Pass Least Squares," *Review of Economics and Statistics*, No. 4, 1967. D. M. Grether and M. Nerlove, "Some Properties of 'Optimal' Seasonal Adjustment," *Econometrica*, No. 5, 1970. M. Nerlove, "Note on Error Components Models" and "Further Evidence on the Estimations of Dynamic Economic Relations from a Time Series of Cross Sections," *Econometrica*, 1971. D.M. Grether and G. S. Maddala, "On the Asymptotic Properties of Some Two-Step Procedures for Estimating Distributed Lag Models," *International Economic Review*, No. 3, 1972. E.J. Hannan and D. F. Nicholls, "The Estimation of Mixed Regression, Autoregression, Moving Average, and Distributed Lag Models," *Econometrica*, 1972. G. S. Maddala and A. S. Rao, "Tests for Serial Correlation in Regression Models with Lagged Dependent Variables and Serially Correlated Errors," *Econometrica*, No. 4, 1973.

Renewed Interest in Simultaneous Equations

This attraction was also due to a revival of interest in the topic that had first made the Cowles Commission famous. Indeed, concern for the simultaneous-equation methodology appeared time and again at the Foundation and was particularly strong during the last period.

In 1956, before presenting Summers's research project, the report stated: "There are two parallel lines of research that could increase our knowledge of the properties of simultaneous-equations estimators. One is explicit mathematical analysis, *to which the Cowles Foundation will return when resources and personnel permit*" (my italics). This occurred not very much later when John Hooper joined the staff around 1959. Since then, a large number of econometricians dealing with simultaneous equations have spent more or less long periods at the Foundation. Most of their contributions can be presented under three major headings: evaluating the predictive accuracy of simultaneous-equation models, finding the small sample properties of various estimators and tests, and extending existing methodology to nonlinear simultaneous equations.

Hooper's research was concerned with various questions raised by the first subject -- construction of confidence regions for forecasts made from an estimated model, development of criteria by which multi-equation models can be evaluated on the basis of sample observations, and study of the impact of specification errors. Subsequently, the subject was no longer deeply studied at the Foundation for about a decade until it was taken up again by David Hendry in 1974-75, then by Ray Fair. The concern of Hendry was again to study the consequences of misspecification of a model and to find ways for the econometrician to select among alternative specifications.

Fair's research into econometric methodology is part of a much more important project that falls mainly outside the domain of this paper, namely, to revise somewhat the present approach and practices in macroeconometric work, at stages of both modeling and policy analysis. This research, recently presented in Fair's *Specification, Estimation and Analysis of Macroeconometric Models*,[6] must be considered as a contribution of the Cowles Foundation to macroeconomics, in the same way as was the work of L. Klein on the American economy in the forties. But it incidentally also touched on some questions of econometric methodology, and more particularly on how to evaluate and compare the predictive accuracy of the models.

Although it was not motivated by exactly the same concern as was the work of Hooper, Hendry, and Fair, Monograph 23 by T. Rothenberg, *Efficient Estimation with A Priori Information*, must be mentioned here. Published in 1973, it was the outcome of research done mainly at Cowles in the years 1965 and 1966. By following the classical approach to estimation in mathematical statistics, built in particular around the Fisher information matrix, Rothenberg gives a unified theory of estimation in the presence of prior information. How valuable is such information in increasing the precision of parameter estimation? What are efficient methods of incorporating this information into estimation procedures?

The importance of knowing the small-sample properties of simultaneous-equation estimators was recognized at Cowles very early, of course. This certainly explains why Summers's research

[6] Readers acquainted with this work and with my writings about the present confusion of ideas in macroeconomics understand that I do appreciate Fair's macroeconomic contributions.

applying the experimental Monte Carlo approach was launched in 1955. Ten years later, a new possibility appeared when, in a Ph.D. thesis at Stanford University, Joseph Kadane introduced an original analytical procedure for studying and comparing the small-sample properties of estimators. He was then immediately appointed at Cowles to develop and complete his work. His procedure is to examine the first few terms of Taylor expansions of the biases and sampling variances of estimators. It is original because the Taylor expansions are not derived under the theoretical hypothesis of an indefinitely increasing number of observations, but under the hypothesis that the variances of the error terms are decreasing to zero, the number of observations remaining fixed; this is "the small (J' approach." In several papers, J. Kadane so obtained a number of insightful results concerning the small-sample distributions of alternative simultaneous-equation estimators. The same approach was later used by Jon Peck for estimators of dynamic equations.

About ten years later, the theory of finite-sample distribution of simultaneous-equation estimators was taken up by John D. Sargan during a rather short visit at Cowles. He then extended the available results about the existence of moments of these estimators, thus clarifying conditions for the significance of various formulas that make approximations of an as then unknown validity.

When Peter Phillips permanently joined the Cowles Foundation in 1980 after a visit in 1978, he had already contributed in various ways to simultaneous-equation econometrics, in particular to its small-sample theory. His work continues at Cowles. His aim for this theory is to determine exact distributions of various estimators in general cases, for instance, when using the instrumental variables method, limited information maximum likelihood, two-stage generalized least squares, and so on. Concurrently, he is developing workable and accurate approximations to these exact distributions.

Although the presence of nonlinearities in macroeconometric models was long ago recognized to be frequent, it is only recently that the methodology of estimation for systems of nonlinear equations was seriously examined. This is now part of the Cowles Foundation research program. Fair has determined feasible procedures for the computation of full information maximum likelihood estimates in large nonlinear models. Phillips has clarified the conditions for consistency of the estimators, studying in particular the interplay between non-normality of the error terms and nonlinearity of the equations. Ray Fair and John Taylor have studied estimation and solution of nonlinear rational expectations models.

It thus appears that the early eighties witness at Cowles a very lively and productive phase of research in simultaneous-equation econometrics and so renew an old tradition. Much progress may again be expected, with its favorable impact on the efficiency of applied macroeconomics. The revival is part of an evolution that concerns econometrics throughout the world. There are indeed many developments, and quite lively ones. They have been stimulated by the formulation of new types of models that are appropriate for new domains of investigation or new types of data: qualitative response models, models with limited dependent variables, models to deal with the self-selection effect that is present in many cross-section samples, disequilibrium models, and so on. trying to find or discuss methods to be used with such models, groups of young econometricians are being led to reconsider a broad range of issues in econometric inference and to work out new theoretical pieces, which may have a broad relevance. I am even wondering whether we did not

recently enter a new phase of rethinking our methodology, the fruits of which will progressively appear in the years to come.

The Probability Approach

Some rethinking of a more basic nature also concerns the probability approach that was promoted by the Cowles Commission, the subject I address in the last part of this paper. Considering the development of the approach through five decades is again rewarding for a good appreciation of the present state of ideas.

Emergence of the Approach

During the twentieth century, economists have taken the habit of looking at statistics for making their theories or analyses more precise; at the same time systems of economic statistics have been built almost everywhere. This may have been the major change in our science during this century. But it is only natural that the basic principles to be applied for drawing conclusions from data did not emerge right away.

At first, collection of data was the main concern. But attention was quickly brought to the definition of indices, to the use of graphics, and more generally to methods of descriptive statistics. Truly econometric studies began when it was claimed that empirical economic laws result from regressions of the observed values of one variable against those of another one, or a few other ones.

Researchers began reflecting about what such studies achieved at about the time the Econometric Society and the Cowles Commission were founded. Ragnar Frisch then wrote his "Pitfalls in the Construction of Statistical Demand Curves." But the way out of the pitfalls was not immediately obvious. Actually, Ragnar Frisch's solution differed from the one that was adopted in the fifties by the maturing econometric movement. His "confluence analysis" belonged to the realm of descriptive statistics, even though it was inspired by a concern to protect the econometrician against the effect of the errors that disturb the observations.

What I am here calling "the probability approach" is simply adhesion to the principle that inference from a sample of observations must proceed within, and be judged with respect to, a prespecified stochastic model that is believed to represent correctly the generation of the data.

The first published expression of this principle in economics seems to be due to T. Koopmans in a little book, *Linear Regression Analysis of Economic Time Series*, that appeared in 1937 but was little read. Koopmans here insists on the role of the prior specification, specifies the error-in-variables model that seemed to be appropriate to what Frisch had in mind, then derives maximum likelihood estimates and discusses their properties.

The most influential text at the time seems to have been T. Haavelmo's memorandum, whose first manuscript was available in 1941, was certainly discussed in the New York-weekends small seminar, and was published in 1944 under the title "The Probability Approach in Econometrics."

The memorandum indeed considers quite carefully the role of the stochastic specification and the problems raised by its choice.

The principle was adopted without reservation at the Cowles Commission during the construction period of the forties. No one seems to have disputed there the notion that descriptive statistics was a blind alley; commenting on confluence analysis, T. Haavelmo gave a particularly strong form to this notion when he wrote: "The purely geometric properties of a set of points in the sample space are insufficient as a basis for statistical inference. In fact, a sample of observations is just a set of cold, uninteresting numbers unless we have a theory concerning the stochastic mechanism that has produced them."

One should not overemphasize the role of the Cowles Commission in this orientation of econometrics. The probability approach was bound to dominate econometrics and stimulate its progress in the forties, fifties, and sixties in the same way as it dominated and stimulated other branches of applied statistics. T. Koopmans, T. Haavelmo, and J. Marschak were just explaining to their economist colleagues what was an already-accepted principle in mathematical statistics at the time. For instance, in the introduction of his 1937 book, T. Koopmans explicitly starts with a presentation of R.A. Fisher's characterization of the "general method of statistics" as given in the first pages of a book that trained generations of applied statisticians. In his memorandum, T. Haavelmo similarly explains Neyman-Pearson theory.

Diffusion of the Approach

But, when arguing for the simultaneous-equation nature of many problems of applied econometrics, Cowles people were also showing that the probability approach was appropriate for dealing with these problems and that it could be applied. They were convincing more and more economists that the approach was a sound and workable one in their science.

A methodological conflict occurred, however, in 1947 with a group at the National Bureau of Economic Research. The Bureau had been involved for many years in a major project concerning the empirical study of business cycles. Masses of data had been gathered and were processed to determine the complex pattern of business fluctuations. In fact, technical questions raised by this work had been present in the minds of all economists dealing in the thirties with methods of time-series analysis: detection of turning points, search for empirical regularities, determination of periodicities, discovery of leading indicators, and so on; they had been present also at the Cowles Commission. In 1946 a heavy book appeared that was intended to present the method followed at the National Bureau and the main findings, concerning in particular possible changes in cyclical behavior of economic variables over time. Publication of the book, *Measuring Business Cycles*, written by two leading figures at the Bureau, A. Burns and w. Mitchell, was an important event for the economics profession.

T. Koopmans wrote a twenty-page review of the book under the title "Measurement without Theory." The review concentrated on the appropriate methodology for the analysis of business fluctuations; it opposed the National Bureau empiricism, qualified as belonging to "the Kepler stage," as against the structural-equation approach, claimed to belong to "the Newton stage." Needless to say, considering when it was written, the review was definitely a plea for the Cowles

Commission approach and a critique of the then much better known National Bureau methodology. For many readers, the self-confidence expressed by Koopmans was somewhat shocking; indeed, he did not seem to pay much attention to empirical results contained in the Burns and Mitchell book and, at the time, the structural-equation approach could not yet present results about business fluctuations. (There is one reference to Tinbergen's work in the review, but it concerns a particular empirical finding for the United Kingdom that is parallel to one of the very few Burns and Mitchell findings for the United States that Koopmans found worth reporting.)

To place this event in its proper perspective, one should remember that, although widely known, the National Bureau empiricism was not generally highly praised at the time. It was strongly opposed by pure theoreticians who considered the work uninteresting and did not pay much attention to numbers, except for pointing out that previous attempts at forecasting business cycles had failed. One then understands what a bad mood Koopmans's review generated at the National Bureau and why a reply was judged necessary.

A long interchange between R. Vining, coming in defense of the Bureau, and T. Koopmans was published two years later. In this interchange, a substantial part is taken by a discussion of the potentialities of a structural system built by aggregation of individual demand and supply equations derived from maximizing behavior. But the most interesting parts are, of course, those concerning the respective merits of the empirical approach and "the probability approach."

Actually, R. Vining was very moderate in his comments on Koopmans's review. He wrote: "The discussion seems somewhat strained to me, and ... I believe that one might raise the possibility that Koopmans' argument contains a misleading emphasis if not an error.... The work of Burns and Mitchell that is being criticized purports to be a work of discovery and hypothesis-seeking, and it is not clear at all what the meaning of 'efficiency' should be in this context. Statistical efficiency is an attribute of an estimation and testing procedure rather than of a procedure of search." He then insisted at length on the facts that the Burns and Mitchell book was explicitly designed to outline certain methods adopted in an explorative study of economic variations, that factual systematized knowledge in economics was meager, and that it was perhaps more important for economists to concentrate on problem I, "the searching for regularities and interrelations of regularities and the feeling around for interesting theoretical models," rather than on the subsequent problems of testing, estimation, and prediction. Although not giving up in the least his main line, Koopmans granted in his reply "...there remains scope for doubt whether all hypothesis-seeking activity can be described and formalized as a choice from a preassigned range of alternatives," i.e., whether all hypothesis-seeking activity can apply the probability approach.

But the latter soft expression of a doubt does not seem to have received much weight in the thoughts of Cowles people during the following years. The previous quotation of T. Haavelmo shows a complete confidence in the necessity and universality of the probability approach. It is typical of a position consistently taken in Cowles Commission writings about econometric methodology. The force of the movement was too strong for the rather cautious but thoughtful words of R. Vining to have changed its course.

Looking Back

I do not need to insist here on the enormous benefits that economics gained from adhesion of econometricians to the probability approach. Without it, most of the many quantitative results and tests that are currently reported in journals and books would not have been obtained. A good understanding of the approach must find its place toward the beginning of any teaching on econometrics.[7] Inference on economic phenomena will always be mainly based on it.

This is true in particular in the field of business fluctuations about which R. Vining wrote in 1949: "The discovery of such relations suitable for the prediction procedure that Koopmans has in mind has yet to be publicly presented, and the phrase 'underlying behavior equation' is left utterly devoid of content." Shortly after, a first answer was given by L. Klein with *Economic Fluctuations in the United States, 1921–1941*, which was Cowles Commission Monograph No. 11. It was only the beginning of a new activity: the building of macroeconometric models intended to serve for the current study and forecast of national economic evolutions. Although economic forecasts are far from being perfect, it is clear by now that macroeconomic modeling extended the horizon about which objective statements may be made. For the first three months but not much beyond, empirical methods of the National Bureau type are fairly reliable; on the other hand, macroeconometric models give useful forecasts up to some two years at least and have even correctly diagnosed longer term trends, such as the mounting unemployment in France. Moreover, the work on these models together with more analytical studies have shown that some behavior equations are indeed robust.

Notwithstanding this indisputable success of the probability approach, some of R. Vining's doubts have been recently echoed within the econometric literature. Some have argued that current econometric practice is often weak at the specification stage.

At the beginning of his book, *Specification Searches — Ad Hoc Inference with Nonexperimental Data*, E.E. Leamer correctly states that "specification searching" often occurs, based on the same data to be later used for estimation, that this activity is not recognized by present teaching, and that it is worthy of a systematic study intended at improving its methodology. He then goes on to note that the classical model of inference is not helpful for understanding specification searches, whereas the Bayesian approach yields insights. The book is indeed a proof that this approach is appropriate for dealing with a series of long-neglected questions.

The lag in methodological research, which Learner identifies, may have something to do with the way in which the probability approach was put to work by the Cowles Commission, as well as by most mathematical statisticians and econometricians at that time. Reference was made only to classical principles of inference, as if they necessarily resulted from the probability approach, which of course was not the case.[8]

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[7] Indeed, Chapter 2 of my *Statistical Methods of Econometrics* (North Holland Publishing Co., Amsterdam, third edition, 1980) strongly argues in favor of the probability approach, and I repudiate nothing of it.

[8] Careful readers of Chapter 2 of my *Statistical Methods of Econometrics* may have noted that, after arguing for the explicit specification of a stochastic model formalizing the prior knowledge about the phenomenon,

More particularly pointed to macroeconomic modeling, the critique raised by C. Sims also concerns the specification stage, which is said to often involve "incredible" hypotheses that unduly simplify what are known to be complex phenomena. One would overstate the critique if one forgot that, in all fields of science, useful hypotheses often are simplifications. But the critique deserves serious consideration.[9]

The positive proposals of C. Sims are also interesting in the present discussion. They suggest that econometricians should avoid introducing a priori restrictions and should concentrate their effort on a descriptive unconstrained study of the multidimensional stochastic process ruling the evolution of the main economic variables. Thus, these proposals recommend an approach that has much in common with the National Bureau empiricism and with R. Frisch's attempts at describing "geometric properties of sets of points in the sample space," attempts that were considered as rather uninteresting by T. Haavelmo.

Some of the writings of C. Sims and other econometricians working with him seem to argue for a complete replacement of the traditional macroeconometric methods by the new multidimensional time-series analysis they are promoting. Accepting to go that far would be tantamount to rejecting the probability approach. I had occasion to explain elsewhere why the arguments in favor of such a revolution cannot be accepted.[10] But, seen as providing a complement to present practices, the proposed analyses are quite valuable.

C. Sims's writings must properly be understood as a plea for more conscious exploratory analysis of the data, before any model is specified. They then transpose to econometrics recommendations made for all fields of application by some mathematical statisticians who, following I. Tukey, now promote all kinds of unconstrained data analysis.

As long as they are not understood as a negation of the probability approach but as stressing the importance of a well-conceived first exploratory phase in any analysis of data sets, these recommendations are healthy. They may expose statisticians to the risk of being dominated by computer specialists who know no subject matter of the field and think only of algorithms; but we econometricians are too well aware of the importance of economic theory to be in danger of that.

I explain what a Bayesian inference would be; classical principles of inference are then presented as providing ways of avoiding the difficult choice of prior distributions.

[9] One might, however, reflect on the lack of consistency between the various critiques now attacking macroeconomic practice. While C. Sims argues for a more careful reference to the facts, other economists are strongly stating sweeping conclusions based on very simple models that are much more incredible than current macroeconometric ones. What should we think, for instance, of studies of the role of monetary policy for economic stabilization when it is assumed that the price level instantaneously adapts to what is required for equality between the demand for money and the money supply?

[10] See my comments on T. Doan, R. Litterman, and C. Sims, "Forecasting and Conditional Projection Using Realistic Prior Distributions," *Econometric Reviews*, Spring 1984.

Concluding Remarks

Looking back in 1983 to the work on econometric methods that was done at the Cowles Commission in its second decade, we may find that its inspiration was at times a bit uncritical. After thirty years of development and application of this work, we may on occasion take a less dogmatic position about some issues. But we so often rely on this work that its relevance need not be debated.

A father cannot expect more than to see his son take up his business and find new ways of making it flourish. Cowles econometricians of the forties are truly the fathers of present day econometricians and, like successful fathers, have good reason to be proud.

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