Econometric Contributions of the Cowles Commission, 1944-47: A Retrospective View *

There have been annual reports, milestone reports, 50-year colloquia, and historical volumes or articles on the work of the Cowles Commission for Research in Economics during its days at the University of Chicago.[1] Some of these have been written by "insiders" and some by "outsiders", and I have never been entirely satisfied with the accounts of what happened or with the underlying interpretations; so this essay attempts to provide my particular point of view, and to relate developments in terms of academic life in the field of economics during and just after the second World War.

Naturally, every author has a tendency to relate events in terms of one's own interests, and, needless to say, my own interpretations differ from those already published. These are not wholesale differences and not differences with every author. This account will adhere

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* Contribution to a series of recollections on professional experiences of distinguished economists. This series opened with the September 1979 issue of this *Review.*

The author is fully appreciative of comments offered by my Cowles Commission colleague, Theodore W. Anderson.

[1] A partial listing includes:

CARL F. CHRIST, "History of the Cowles Commission, 1932-1952", *Economic Theory and Measurement* (Chicago: Cowles Commission for Research in Economics, 1952), 3-65.

CLIFFORD HILDRETH, *The Cowles Commission in Chicago, 1939-1955,* (Berlin: Springer-Verlag, 1986).

ROY J. EPSTEIN, *A History of Econometrics* (Amsterdam: North-Holland, 1987).

T.W. ANDERSON, "Trygve Haavelmo and Simultaneous Equation Models", *Scandinavian Journal of Statistics,* 18 (1991), 1-19.

KENNETH J. ARROW, GERARD DEBREU, EDMOND MALINVAUD, and ROBERT M. SOLOW, *Cowles Fiftieth Anniversary* (New Haven: Cowles Fundation, 1991).

CARL F. CHRIST, "The Cowles Commission's Contributions to Econometrics at Chicago, 1939-1955", *Journal of Economic Literature,* to be published.

BNL Quarterly Review, no. 177, June 1991.

closely to the time span indicated by the title, but interpretations involve some perceptions of prior and past developments; however, the actual events of 1944-47 form the basis for my interpretations. Some of the published accounts are from scholars who were not on hand for the whole period of interest to me, and some are based purely on historical research. Without having been on the scene, historians cannot capture voice inflections, gestures, body language, purely oral commentary (often in informal discussion) and other pieces of unrecorded information. There were daily discussions and frequent reliance on oral traditions at the Cowles Commission, then, and the "outside" accounts sometimes miss relevant points that related to this reliance on oral tradition. Lapses of memory can also affect "insider" views.

Since Cowles Commission econometrics had a profound effect on American and world economic analysis for the entire period after the second World War, it is worth while trying to get the story in as much detail as possible. For my own part, my years at Chicago, 1944-47, occupied my first professional *post*-doctoral appointment. It made a deep impression on my entire life's work, and I am grateful for this opportunity to put the relevant matters in my own perspective.

In 1944, at the annual meetings of the Econometric Society in Cleveland, Ohio, I attended in order to report on my dissertation research dealing with *The Keynesian Revolution*. It was not an econometric subject, although my graduate studies were strongly focused on econometrics. The meetings were small; the Society had not yet started its rapid development; and the wartime meetings were necessarily thinly populated. But three prominent members of the Cowles Commission staff were there: Jacob Marschak, Tjalling Koopmans, and Leonid Hurwicz, all presenting interesting papers that were forerunners of developments to come.

Marschak prevailed on me to drop all other job search activities and join the staff at Cowles to develop what he said the country needed desperately – a new "Tinbergen" model of the US economy. He referred of course to the pioneering effort of Tinbergen at the League of Nations, before the War. Koopmans had been associated with that research team. But Marschak had something else in mind, namely, the construction of a new American model that would build on very recent developments in theoretical econometrics. He had in mind the work of Trygve Haavelmo and Abraham Wald (with the

collaboration of Henry Mann). At MIT seminars that I had organized in mathematical statistics, there had been presentations by Haavelmo and Wald; so some of the concepts that Marschak had in mind were immediately relevant to me. This is what Marschak said, at a roundtable session during the Cleveland meetings on the subject of forecasting postwar demand,

> "Yet it has not been proved convincingly that the action of the bulk of businessmen is unpredictable. Our present ignorance of laws of investment behavior may be partly the fault of statisticians. We still have to develop tools appropriate to the analysis of economic (or other non-experimental) data.
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> Because of the impossibility of controlling the economic variables in a laboratory, the existence of a number of simultaneous equations that determine income and savings makes it, in general, illegitimate to apply ordinary 'fitting' methods, though in exceptional cases this is still possible. Recent publications of Messrs. Haavelmo and Klein have pointed out these difficulties".[2]

I needed no coaxing to re-orient my first job sights because I had an instantaneous enthusiastic reaction to Marschak's suggestion, and within two months I was on hand in Chicago, at the Cowles Commission, working first on specifying investment functions.

It is often overlooked that Econometrics as a disciplinary field of study has three joint lines of interest: 1. economics, 2. statistics, 3. mathematics. Marschak directed the research work at the Cowles Commission very much along the lines of team activity in order to deal with all three aspects of econometrics. He assembled an unusual group of independent scholars but wanted to guide all together in teamwork. When he recruited me, it was explicitly to prepare model specifications according to received economic theory, using both microeconomics, macroeconomics, and aggregation or index theory to bridge the gap between them. In addition, I was assigned the task of data preparation and model estimation/testing. Other team members had different assignments. Haavelmo was recruited to work on econometric theory, Theodore Anderson to work on the underlying theory of mathematical statistics, Koopmans to work on overseeing all the pieces but especially on implementation of the work, through computation that was very complicated and tedious, given available facilities

[2] Discussion comments of JACOB MARSCHAK, *Econometrica*, 13 (January 1945), 59.

of the day. Herman Rubin worked on econometric theory and mathematical statistics; Leonid Hurwicz was not continuously in residence but contributed to all aspects of the work; Roy Leipnik was a mathematical statistician for the project. Don Patinkin was assigned work on a sectoral model for manufacturing, but drifted more towards an interest in the underlying Keynesian macrotheory. Others came at later stages to develop agricultural models, beyond what Haavelmo and Meyer Girshick had already done in that area, partly to test the new econometric theories.

It should be evident that an extraordinarily talented group of people was brought together to pursue a common research objective. In addition to those listed above who participated intensively on site, many other distinguished people were involved in frequent contacts or came to the Chicago site towards the end of the period being considered. Abraham Wald was on the Columbia faculty but stayed in touch. He stimulated Haavelmo's thinking about estimation of simultaneous equation systems in econometrics, extended the theoretical results in several directions, and contributed to the ingenious solution of many problems of econometrics and mathematical economics. Kenneth Arrow joined the Cowles Commission staff towards the end of this period, contributed to the central problem and then developed his original contributions in another direction in welfare economics. Herbert Simon was at the Illinois Institute of Technology and was associated with the Cowles Commission, making contributions to the underlying econometric theory, especially stability conditions and analysis of causal structure.

During this period, John von Neumann visited on occasion (to and from Los Alamos because travelers had to change trains in Chicago) to lecture on the new developments in game theory and to discuss computational strategy for solving the systems of nonlinear estimation equations that were involved with the Cowles Commission's new techniques. Simon Kuznets visited to discuss national accounting issues as well as the general issues for understanding the macroeconomy; Kenneth Boulding was a frequent participant in Cowles Commission seminars as was Albert Hart. The latter served on the research staff of the Committee for Economic Development, then located nearby, and participated in the first use of Cowles Commission models for macroeconomic projections. We shall return to this matter below. Michal Kalecki was at the International Labour Office, then located in Montreal, and came to the Cowles

Commission to lecture on his macrodynamic models. Jan Tinbergen and Ragnar Frisch, who were pioneers in the founding of the Econometric Society and contemporaries of Marschak in Europe, where so much intellectual activity along these lines took place during the 1930s, came to visit and lecture right after the end of the War in Europe.

One of the most fertile thinkers of this century was Leo Szilard, who took time from his work on the Metallurgy Project (a Chicago branch of the atomic bomb project) to come to the Cowles Commission to discuss his amateur interests in macroeconomics. His objective was to design a dynamic model that would be free of trade cycles. He presented a Cowles Commission seminar and had endless discussions with staff members on his scheme that consisted of two types of money, one for spending and one for saving. He designed a game with players representing a government policy official, a business executive, a labor leader, and a consumer. Some of the Cowles Commission staff members helped him to determine plausible initial endowments of cash balances for each player, and he hoped to be able to teach economic principles through the outcomes of his game, for which he constructed economic rules of play. As ever, Szilard's design for the game showed his extreme cleverness, but the actual game never was played on a large scale.

After Hiroshima, he devoted tireless energy to political campaigns to control nuclear weapons and enlisted Marschak and me to help him devise metropolitan configurations that would be *relatively* safe from bombing.

This talented group of people who were at the Cowles Commission then, or who visited, made for extremely lively discussions on many topics. It was an enormously fruitful period for idea generation, and it may be asked how this group could be brought together all at once? Much is owed to the talented leadership and organizational ability of Jacob Marschak, for it was not only the staff vintages of 1944-47 that were so productive but also succeeding vintages at Chicago and further at Yale (in the form of the Cowles Foundation).

The subject of econometrics was very youthful at that time. This entire group of people were dedicated to the quantitative methodology in economics, and mathematics was a principal tool. But the subject was hardly recognized in American academic curricula. I happened to be fortunate to have studied in Berkeley and at MIT, where the subject was first appearing, but, by and large, we operated in an academic underground and met open hostility on many occasions with distinguished senior economists. It is altogether different now; mathematical economics and econometrics are so fully accepted that they tend to dominate advanced teaching of economics.

The relatively few people who were drawn to mathematical methods in economics found it congenial to congregate at the Cowles Commission during the years being examined here, but there were a few other places too. In addition, job opportunities were scarce; post graduate scholarships were not abundant or generous; so we who took the subject very seriously were pleased to work together at this one place in very modest circumstances. We had relatively few demands other than to pursue our work.

There were two worlds of economics at Chicago then, "us" and "them", the former were the Cowles group, who were overwhelmingly New Deal democrats. I remember, vividly, the shock when Albert Hart came over from his office at CED and interrupted a Cowles Commission seminar one afternoon to tell us that President Roosevelt had passed away. We were all struck with grief.

The latter were the stalwarts of the Chicago School, and we nearly always took polarized positions at general economics seminars on campus. Our intellectual opponents were Frank Knight, Henry Simons, Lloyd Mints, and at the end of this period, Milton Friedman. Some faculty members were friendly with both groups, and Jacob Viner's main quarrel with us was on the issue of mathematical methodology.

In general, the members of the Chicago School, particularly Milton Friedman, were sympathetic with the non mathematical approach to quantitative economics pursued by the National Bureau of Economic Research under the leadership of Wesley Mitchell and Arthur Burns. When Tjalling Koopmans reviewed their massive work on *Measuring Business Cycles* under the heading "Measurement without Theory", bitterness reached even a higher pitch. As a visiting staff member of the National Bureau during 1948-49, I could sense the tension in the dispute over methodology with the Cowles Commission. It was not purely methodology, however. A central issue was that we members of the Cowles Commission were seeking an objective that would permit state intervention and guidance for economic policy, and this approach was eschewed by both the National Bureau and the Chicago School.

The ideological split in Chicago played some role in the move to Yale but it also brought the Cowles name back to the Alma Mater of Alfred Cowles. His views of economics were probably more compatible with our opponents at the University of Chicago, but Mr. Cowles was attached to the mathematical methodology, and gave free rein in the present sense of academic freedom to our research activities. Jacob Marschak once told me that he was originally dubious about accepting the directorship of the Cowles Commission because he found the anti Roosevelt (anti New Deal) attitudes of Mr. Cowles and his close associates to be intolerable, but a *modus vivendi* was finally worked out between them.

Substantive content and lasting effect on scholarship

The inspiration for the research focus at the Cowles Commission, starting in 1944, was the contribution of Trygve Haavelmo. His concepts that econometric model building should take account of the multiplicity and simultaneity of economic relationships are very powerful. Furthermore, the formulation of econometric models for modern methods of statistical inference should incorporate the simultaneous equation system specification. This, too, is important. When Haavelmo related this structure to sample design and stochastic specification of models, it is evident that he provided us with an insightful way of looking at the economy in quantitative terms.

These views fit well with the modeling of the economy as a whole in macrosystems, and when Haavelmo had visited MIT, just prior to my own departure for Chicago, he made it clear that his concept should have an effect on the work of building Tinbergen-type models. But it is also relevant for microeconometrics too, although there are obviously some problems facing individual economic agents or particular markets where simultaneity in feedback relationships is not so important.

It was the main push from Haavelmo, greatly assisted by the ideas of Abraham Wald, Jerzy Neyman, and others that provided the intellectual inspiration for the research project at the Cowles Commission. Important parts were filled in by Anderson, Girshick, Rubin, Koopmans, Hurwicz, Marschak and others in spirited seminars that were frequently and conscientiously attended. A feeling of accomplishment and eventual breakthrough permeated the group. Great faith was placed on the ability of sophisticated statistical methods, particularly those that involved advanced mathematics, to make significant increments to the power of econometric analysis.

I, personally, place more faith on the data base, economic analysis (institutional as well as theoretical), political insight, and attention to the steady flow of information. Some of the other members of the team showed disappointment that the results, when finally produced, were not sharper and more precise. Some searched for better mathematical economic theory, some for better samples of data, and others for still more advanced statistical method. In any event, postwar opportunities opened for all. Some left the group for new academic appointments – Hurwicz to Iowa State, Anderson to Columbia, Haavelmo to Oslo, Patinkin to Jerusalem (via Illinois), and Arrow to Stanford, but, more importantly, they turned to other subjects in econometrics or mathematical economics. Marschak turned toward the theory of teams and organization, Koopmans to activity analysis, Anderson to multivariate statistics, Arrow to welfare economics, Hurwicz to mathematical economics. At the beginning of the project Marschak used to say, in public meetings, "just give us three years, and we shall deliver powerful new results for economic analysis". He always had at the back of his mind that we would be able to help decisively with postwar economic planning.

Albert Hart approached us one day in 1945 and asked if we could use the Cowles Commission macro model for the United States to make postwar demobilization projections for the CED. He would supply the policy assumptions and we would supply the model based projections. I knew that the request came sooner than we had anticipated, but we agreed, hesitatingly, to have a trial. I, personally, felt that our results would be disappointing for the CED, not only on accuracy grounds but also because they would present a pessimistic outlook for a business group that was forward looking, enthusiastic about the economy, and generally upbeat.

To my surprise, this first exercise, though premature, was very bullish. We simply could not find pessimistic projections for the postwar economy. This result was unexpected, but I took the attitude that the model was telling us something special. I went to Washington for some economics meetings and visited economists at the Bureau of Budget, Department of Commerce, the Federal Reserve Board telling them about these results. The Cowles-CED projections were not taken seriously; the response in all cases was that we should

wait until mid year 1946, when we would find 6 million unemployed again and a return to conditions of the Great Depression. It was, to our adversaries, just a matter of time, waiting for events to unfold.

The projection for CED was not changed and turned out to be very good, but the Cowles Commission did not continue a regular forecasting activity. In general, the senior researchers at the Commission were not satisfied with the performance of models that had been constructed during the expansionary phase of the research program and there was relatively little carry-on activity in empirical model building with repeated applications over sustained time periods. Macro model building continued on a smaller scale after 1947, but it ceased to be the central thrust in the same way that it was during 1944-47, and research developments were pursued quite successfully in many related fields of econometrics.

There are many contributing factors to successful macroeconometric model analysis. Contributions from economics, mathematics, and statistics which formed the basis for early enthusiasm at the Cowles Commission were obviously important, but I feel that my colleagues wanted results that were quite robust, in small confidence areas, and very discriminating among competing hypotheses. Quantitative economics is not like that. It is inelegant, very tedious, very repetitive, and capable of forward movement in small increments. I admired the elegant theorems that my associates produced, but it seemed to me that their assumptions had to be very strong and not very realistic in order to get their beautiful results. I felt that if one paid unusual attention to data – very much in the painstaking tradition of Simon Kuznets – replicated analyses regularly, looked at more detail for the economy, learned as much as possible about realistic economic reaction, and stayed in touch with the economic situation on a daily basis that it would be possible to use econometric models for guidance, both in the fields of policy application and in pure *understanding* of the economy.

Of course, we never dreamed, in the wildest of circumstances, that we would have the computer power or the information flow that became available in the 1970s and 1980s, but good quantitative research moved forward in ways that were readily compatible with things that were to come.

Important lessons to be learned from the Cowles Commission experience are that statistical consistency or unbiasedness is not the most important property of estimators; precision is associated much more with variance or efficiency. It is possible to trade consistency for error variance and come out ahead. It is important to grasp the simultaneity of the macroeconomy but not necessarily to tie statistical estimation method exclusively to this property. It is more important to be able to update, correct, or revise estimates on the basis of a steady flow of important new information, and very flexible methods of estimation are needed for this purpose. The highly flexible methods can be more powerful in simple form than the more complicated procedures that we were following at the Cowles Commission. In particular, for an economy where detailed information is important, it is preferable to aim for large systems – many times larger than those of 1944-47 – and to handle them by relatively flexible, simple statistical methods instead of paying enormous attention to complicated estimation procedures for smaller manageable systems. The latter are highly uninformative in an era when information detail is at a premium.

When systems are large enough to provide the detail that is required for economic intelligence operations, flexible techniques that deal with less than the full system at each step of model preparation are to be desired. The data are changing fast through revisions and extensions. We cannot usefully go back to reestimate entire systems of hundreds or thousands of equations as frequently as we need to make new applications. It simply is not possible to build a system and lock it into place for some years, or even months. When complete systems are re-estimated, parameter estimates change in many seemingly remote parts of the whole. In addition, data are still sparse; we work fundamentally with small samples and cannot appeal exclusively to asymptotic results for statistical methods. In truly small sample theory, considerations beyond those that we were depending on during 1944-47 became more important than statistical consistency. I believe that the spirit of what we were trying to achieve in that beginning period can best be reached by statistical methods that are simpler than those that we thought were most powerful at the Cowles Commission. In the oral tradition, we discussed these simpler methods but only later were able to develop a full appreciation of them.

The methods of two-stage and three-stage least squares, which simplified some of the techniques that we were exploring at the Cowles Commission during 1944-47 are based to a large extent on asymptotic statistical theory and may not provide the most fruitful

research line for our econometric samples. It is not that they provide poor results; it is simply a matter that they, together with the techniques of limited and full information maximum likelihood estimation that we were investigating at the Cowles Commission during the early years in Chicago would not, by themselves, lead to significantly more powerful models.

Some of the most important statistical advances that have contributed to better econometric performance in recent years are better understanding of multicollinearity, ability to handle nonlinearities in behavior, better appreciation of the time shape of economic reactions, better separation of economic signal from noise, use of higher frequency of data flow, and better understanding of technological change. If more attention is paid to these issues, we can come closer to the high aspirations that we had in those glorious days of 1944-47 at the Cowles Commission in Chicago.

The gains over simplistic, unsystematic, and informal judgmental approaches may not seem to an outsider to be large, but they can be realized and put to good advantage for providing economic guidance to policy makers – if only they are willing to listen. I have never lost sight of the optimistic vision that Jacob Marschak and my Cowles Commission colleagues had during those very productive years at the close of the War.

Philadelphia, Pa.

LAWRENCE R. KLEIN